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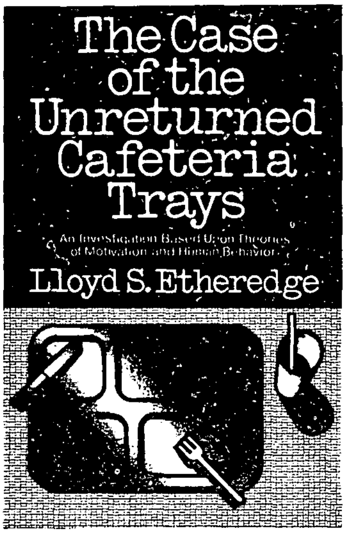
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The Costs of Major Wars: The Phoenix Factor*

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A major unexplored area in the field of international politics is the consequences of major wars for members of the international system in terms of power lost or gained. This paper explores these shifts of power among neutrals, winners, and losers as a result of these wars, using a sample of 32 cases and time series analysis. The findings register unexpected but systematic patterns after major conflicts; while winners and neutrals are affected marginally by the conflict, losers' powers are at first eroded. Over the long run (15–20 years), though, the effects of the loss dissipate; losers accelerate their recovery and soon resume antebellum status. It is this phenomenon that the authors call the phoenix factor.

Introduction

There is no end of books and articles about war, and yet we know little of the subject that is of use. Of the three major aspects of interest in war—its causes, its outcomes, and its consequences—our concern in this paper is with consequences. These, of course, are intimately related both to beginnings and outcomes. The relationship between studies undertaken in the past and the effort in hand is useful briefly to elucidate.

Specialists and laymen alike have traditionally assumed the existence of a tie between the outbreak of major wars and the distribution of power among the major actors in an international system. In a general way, this premise postulates which power distributions went with war and which with peace. But there has been no interest in viewing the problem from the opposite angle of vision: what effects did outcomes of the conflicts have on the power distribution of the international system?

Can an inferior power reduce a superior one by means of war? Can the superior turn back the inferior by winning such a war? In short, what is "gained" and what is "lost," in power terms, by war? The overriding answer to such questions, the one accepted in the past, is that major wars and their outcomes make a tremendous difference in the international system. The future of a belligerent nation depends on victory or defeat. Regardless of other rationales advanced, this is the "logical" basis for going to war. It seems a powerful reason, and it is widely accepted. The justification of wars is rooted in such convictions.

However, we suspect that such reasoning is invalid. Hence, this investigation. A problem confronting the researcher in this area is how to ascertain the validity of the propositions that the outcomes of war either do or do not affect the power distribution in the international system. Several separable tasks are involved:

1. Considerable attention must be devoted to the method by which power is measured.
2. Attention must be accorded to the selection of conflicts to be studied.
3. It must be determined whose performance, in any given conflict, is to be evaluated. In other words, who are the principal actors?
4. One's philosophical concern must initially be translated into theoretical propositions and then into operational hypotheses.
5. One should test and report one's findings.

These five points represent the table of contents of this paper.

The Measurement of Power Resources

Power has long been considered to be the capacity of an individual, group, or nation to control the behavior of others in accordance with its own ends.[1] It is an element of every

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*This paper is a portion of a much larger work on the beginnings, outcomes, consequences, and prevention of wars. We wish to acknowledge our gratitude to the Earhart Foundation for its financial support in connection with this project.

[1] A. F. K. Organski, *World Politics,* 2nd ed. (New York: Knopf, 1968), p. 104; Karl Deutsch, *The Analysis of International Relations* (Englewood Cliffs, N.J.: Prentice-Hall, 1968), p. 70; Hans J. Morgenthau, *Politics Among Nations,* 4th ed. (New York: Knopf, 1966), p. 26.

relationship, when each party has at its disposal resources, tangible or intangible, likely to alter the conduct of the other. Power becomes apparent only when a disagreement arises between the parties, in which event, the will of the more powerful usually prevails. The measurement of power, therefore, is vital to the prediction and explanation of joint behavior.

There have been attempts to study the actual degree of control which one nation has exercised over another. For the most part, however, specialists in international politics have retreated to measuring the resources that generate power.[2] This procedure presents problems. Power resources do not necessarily reflect the exercise of their potentialities, nor do reliable estimates include certain essential components (e.g., diplomatic skills, charismatic leadership, and internationally appealing belief systems) not susceptible to easy or dependable measurement. Moreover, hard estimates do not indicate the power a particular nation may ostensibly possess simply because other nations may mistakenly assume that it is either more or less powerful than is actually the case. The semblance of power often passes for its reality.[3]

This problem may be set aside for this study. A discrepancy between the possession of real power and the external perception of it is more likely to occur in time of peace than in wartime and is most likely to occur just before nations actually commit themselves to conflict. During and after wars, however, the two views tend to merge. Thus the perception and the reality of American military power were far apart until this country's gradual commitment in Vietnam when war brought the two together again. The same occurred in Korea more than a decade earlier.

Procedure for the measurement of objective power comprises two steps: (1) one must compose a list of all the indicators that can influence the exercise of national power and select the indicators considered most important; (2) one must determine a way of aggregating these components to obtain a single measure of national capability.

The Selection of Indicators. Among scholars interested in the construction of empirical estimates of national power, there is agreement that measures of economic, political, military, and demographic capabilities suffice to give a reasonably accurate overall indication of power.[4] Quantitative indicators are generally available. The economic capacity of a nation, for example, is suggested by per capita, total, or disposable output. Demographic capabilities are grossly reflected by total population or, more accurately, by the fraction in working or fighting age groups. Military preparedness may be inferred from expenditures on arms and the size of military forces. Only political capabilities are still unmeasured. The necessary data are usually not available and, if available, are not easy to interpret.

This problem of satisfactorily measuring political capability has important implications for our study and requires comment. It has been long assumed that economic development and the capacity of a political system to penetrate and mobilize the human resources of a nation go hand in hand. When a country evinced high degrees of social and economic development, it was taken for granted that it would exhibit a high estimate of the extent of mass-elite political linkages, that is, the ability of government to organize and motivate its population for national purposes and to muster national resources through political channels. This is only partially true.

Discrimination is required. High degrees of political mobilization do not necessarily accompany a comparable level of economic development. China, for instance, is highly mobilized politically, but not economically. The same statement is applicable to North Korea and North Vietnam. The outcomes of direct military confrontations between the United States and China, North Korea, and North Vietnam clearly indicate that if a nation is economically underdeveloped, estimates of its power which are based mainly on socioeconomic and military indicators may be totally unreliable.[5]

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[2] This distinction between national capabilities and national power is widely used in international politics. Throughout this paper we are concerned *solely* with national capabilities and power resources; however, to relieve the tedium of repeating over and over again "national capabilities" we shall use three terms—national capabilities, power resources, and national power—interchangeably.

[3] Ole Holsti, "The Belief System and National Images: A Case Study," *Journal of Conflict Resolution,* 6 (1962), pp. 245–51; cf. also K. J. Holsti, *International Politics: A Framework for Analysis* (Englewood Cliffs, N.J.: Prentice-Hall, 1967), Ch. 7.

[4] See, for example, Raymond Aron, *Peace and War,* Trans. Robert Howard and Annette Fox (New York: Doubleday, 1966); cf. also, William Coplin, *Introduction to International Politics* (Chicago: Markham, 1970); Organski, Ch. 6–8; J. David Singer, Stuart Bremer, and John Stuckey, "Capability Distribution, Uncertainty and Major Power War, 1820–1965," in *Peace, War and Numbers,* ed. Bruce Russett (Beverly Hills: Sage Publications, 1972), pp. 21–27; Nazli Choucri and Dennis Meadows, "International Implications of Technological Development and Population Growth: A Simulated Model of International Conflict" (Center for International Studies, MIT, mimeographed, 1971), pp. 23–24. This presentation considers technology an independent variable.

We include in our survey nations ruled by various forms of government, for there is no suggestion that economic development and democracy are uniquely harmonious. Often, the reverse is true. The United States and national-socialist Germany were, in the third and fourth decades of this century, economically developed, and both had political systems which could effectively mobilize human and material resources. Yet one nation was a democracy, the other a totalitarian dictatorship. We cannot directly measure the political resources which are a central feature of national power, and because without such data measurements of national power in underdeveloped countries are unreliable, we consider in this article only economically developed countries.

The Problem of Aggregation. The selection of critical indicators and a method of combining them to form a single measure is the first step. Until recently, no aggregation was attempted. Frequently, values of all the indicators were presented; the reader was left to bring them into some sort of focus. An intuitive or impressionistic measure could be derived if a nation scored equally high on all elements measured. However, if a country scored high in some areas and low in others, impressionistic estimates became fanciful.

Our choice of an overall yardstick of national capabilities, in preference to all others considered, is gross national product.[6] This decision may appear unwise. Might not a single economic indicator prove inadequate? Would not a more comprehensive index, composed of a number of other indicators, perform better? Unfortunately, more complicated indices do not perform so well as does this single one, and data for them are not as reliable for the period covered by this research.[7]

The utility of measurements of total output should not be surprising. Estimates of gross national product closely reflect the movement of the underlying variables crucial to the generation of national power resources: the fraction of the population of working and fighting ages and the level of productivity.[8]

Because total output is the result of the interaction between the size of the productive population and its level of productivity, the national equation can be expressed in the following fashion:

$$\text{Power} = \text{Population} \times \frac{\text{GNP}}{\text{Population}} = \text{GNP}$$

In this formulation, Population implies the size of the fraction of members of working age, and per capita product implies their productivity level. The interaction of components implies a weighting system. Fluctuations in productivity affect the importance of population upwards or downwards. One population twice as productive as another implies that two individual workers in the less productive economy are required to perform the labor of one in the more productive. This weighting system, while arbitrary, seems theoretically justifiable. More importantly, it reflects the realities of international politics.

Estimating Consequences of War. As critical as the problem of power aggregation is the decision about the procedure to follow in estimating outcomes of war. This can be accomplished in one of three ways. First, one can simply establish suitable points before and after a conflict and compare them, with the differences regarded as the costs of war. This method is often applied, but is vulnerable to major error.

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[5] Attempts to measure "political development" are reviewed in Irma Adelman and Cynthia Morris, *Economic Growth and Social Equity in Developing Countries* (Stanford, Calif.: Stanford University Press, 1973).

[6] Alternate explicit attempts to estimate a single measure of national capabilities are: Wilhelm Fucks, *Formeln Zur Macht* (Stuttgart: Deutsche Verlags-Anstalt, 1965); Clifford German, "A Tentative Evaluation of World Power," *Journal of Conflict Resolution*, 4 (March 1960), pp. 138–44; Organski, pp. 207–14; Singer, Bremer, and Stuckey, pp. 19–49; Klaus Knorr, *Military Power and Potential* (Lexington, Mass.: Heath, 1970); Ray Cline, *World Power Assessment* (Washington, D.C.: Georgetown University, 1975).

[7] We compared the performance of GNP over time with the measure developed by Singer, Bremer, and Stuckey, the only other multidimensional measure available for the same period. Both measures are highly correlated and the similarity increases as the reliability of data improves. (R^2 of .95 was obtained for the sample of major powers for 1870–1970.) See Jacek Kugler, "The Consequences of War" (Ph.D. Dissertation, University of Michigan, 1973), pp. 82–96.

[8] See Steven Rosen, "War, Power, and the Willingness to Suffer," in *Peace, War and Numbers*, p. 171; and Charles Hitch and Roland McKean, *The Economics of Defense in the Nuclear Age*, 2nd ed. (Cambridge, Mass.: Harvard University Press, 1967) Pt. 1; Norman Alcock and Alan Newcombe, "Perceptions of National Power," *Journal of Conflict Resolution*, 14 (1970), pp. 335–43. For opposite views see Klaus P. Heiss, Klaus Knorr, and Oskar Morgenstern, *Long Term Projections of Political and Military Power* (Princeton: Mathematica Inc., 1973).

A second technique consists of calculating losses from a conflict by taking into account those changes that one estimates would have occurred had no conflict taken place. This procedure controls for normal growth, but is still based on the difference between two points. The rates of change are stipulated to be the same as those before the onset of the conflict. This procedure is clearly superior to the first, but it is weak because any comparison of the last year *before* and the first year *after* a conflict, work with times that are plainly abnormal, thus rendering difficult, if not impossible, the estimation of war costs. On the other hand, choosing points more widely separated distorts even more the measurement of war's effect. Moreover, the method produces misleading results because it maximizes the effects of different growth rates across countries and usually imputes high losses to rapidly growing nations.[9]

A third method represents a significant improvement over the others. Three steps are involved: (1) a base period prior to the war is selected, and a trend during this period is established; (2) the trend is extrapolated and serves as a moving base for comparison with actual performance; (3) differences between real behavior and the extrapolated lines are estimated. Figure 1 should facilitate an understanding of the mechanics of the model. The procedure we suggest permits the avoidance of distortions resulting from others proposed.

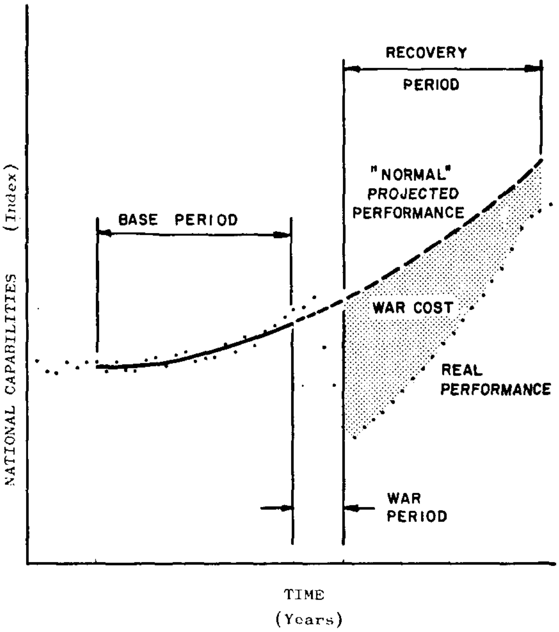

Figure 1. The Costs of Major Wars

Forecasting. Our efforts to extrapolate require additional comment. The attempt to estimate changes over time by extrapolating trends from the base period is obviously a crucial factor since the quality of the projections is of paramount importance. The difference between anticipated and actual behavior rests on where we place the extrapolated line. If the selected series is well behaved with only minor fluctuations from year to year, regression techniques can be used to extrapolate from the pattern established in the base period. These techniques are particularly well suited for time series analysis, provided a variety of tests are performed guarding against violations of assumptions and controls are introduced reducing undesirable effects of fluctuations. However, a number of interrelated questions must be answered: (1) Of what durations should the base period and the projections be? (2) How is one to deal with the abnormalities in periods serving as bases for forecasts? (3) What models should be employed in making projections?

Let us begin with the question of the lengths of base and extrapolation periods. Where a base period is said to end and a recovery period to begin are critical moments to choose. For example, Germany began to arm and mobilize long before the declaration of hostilities in World War II. Italy quit that war long before peace was declared. Such contingencies abound in wars, and adjustments become increasingly arbitrary. To avoid bias, the starting point of conflict for all contenders was judged to be the first full year of active confrontation between two sides. The beginning of the recovery period was judged to be the first full year after belligerence came to a halt.

From these two points, the official beginning of the war and the official termination, we selected a base period of 19 years and extrapolated onward for 20. Our decision was not arbitrary. We were aware that the longer the time segment available to fit the regression line, the more reliable the prediction. On the other hand, the longer the period, the less characteristic were the growth trends.[10] Our own selections were made by trial and error. Similar regressions were run for periods of 15, 20, and 25 years prior to the two conflicts in order to determine the stability of results. Those obtained for regressions using a period of 15 years were considerably less stable than those of 20, and regressions using a period of 25 years did not produce improved results. A period of 19 years seemed most suitable, because it provided as much separation as possible between the two world wars and a sample of years large enough to permit the use of at least 16 points in all estimates.

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[9] See Frank Notestein, Irene Taeuber, Dudley Kirk, Ansley Coale, and Louise Kiser, *The Future Population of Europe and the Soviet Union; Population Projection 1940–1970* (Geneva: League of Nations, 1944), Ch. 3; also Gregory Frumkin, *Population Changes in Europe Since 1939* (New York: Augustus M. Kelley, 1951).

We extrapolated for 20 years. Clearly, a recovery period is difficult to isolate. Some effects of war linger. French resentment simmered for decades after their defeat by Germany in the Franco-Prussian War. German anger and revanchism, after their defeat by the Allies in World War I, produced World II. We are primarily interested in the period when the effects of war are immediate and dominate the behavior of the international system.

Much of the analysis that follows rests on our choice of a forecasting model. We decided that linear models would not serve; they imply that yearly additions to total output are constant. They assume, therefore, a decrease in growth rates as the base expands. Such assumptions seemed invalid; the problem of underestimation is reduced with logarithmic transformations, because constant growth rates can be estimated with an ever-extending base.

To check our choice, we tested the performance of logarithmic adjustments with data unaffected by war from 1870 to 1913. In all cases, the nonlogarithmic projections consistently underestimated the real performance of the nations considered, and outliers systematically increased in the latter part of the series. In all instances better fits resulted from data which had been logarithmically transformed.[11]

One caveat, however, should be made: there is no assurance that the patterns of growth for the period of 1900–1970 which we used for this study are identical to those of 1870–1913 used to test our model. The log model excludes all acceleration of growth rates over time. We know that national growth sometimes undergoes such acceleration.

An additional matter should be raised. Normal growth is essential if one is seeking to forecast the future through extrapolation. Periods between wars are regarded as times when normal growth takes place. This is not the case. Wars are but one kind of disturbance; depressions and revolutions can also occasion havoc with patterns of total output, as did the Great Depression of the 1930s. This economic downturn overlapped the period of recovery from the effects of World War I and the end of the era which served as the base for our projections from 1946 to 1965. No analysis could be carried out unless the effects of the depression were eliminated or at least substantially reduced. The simplest and most efficient way was to omit some outlying points to obtain estimates for nations affected by the cycle. It was impossible and theoretically unacceptable, however, to eliminate all of the effects of the depression, and some of the results show the distortions which originate in this disturbance in the base period.

Nor was the depression the only disturbance in the trends of our base period: two revolutions and accompanying civil wars also created major problems. The disastrous effects of the Spanish revolution and civil war on Spain's economic performance caused us to drop that country from our sample altogether, in spite of our wish not to reduce further the already small sample. The Russian revolution and civil war also had extreme effects on the economic performance of the country. Moreover, data were available only for 1895, 1899, 1913, 1920, and yearly after 1928. Nevertheless, we retained the USSR for compelling reasons: Russia was a critical actor in World War I.

Making the best of the situation, we used the data we had and maintained our procedure of estimation of consequences. Reliability measures were quite good, but it should be clear that results in the Russian case cannot be considered as stable or reliable as other projections.

Projections are not predictions. The fitting of data statistically is not equivalent to predicting how much real output there would have been in the absence of war. Moreover, even the correspondence between estimates and reality is not a final test of the validity of the estimates themselves. As Frumkin reminds us, absurd and unscientific estimates occasionally hit the mark.[12]

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[10] Scholars have justifiably cautioned that given the deficiencies in data and models in the social sciences, projections should not exceed 20 years; see Heiss, p. 107; Simon Kuznets demonstrates that growth rates of developed countries remained constant or increased in the last century; see his *Economic Growth of Nations* (Cambridge, Mass.: Harvard University Press, Belknap Press, 1971), Ch. 1.

[11] Kugler, pp. 116–17.

[12] Frumkin, p. 17.

Standardization. Consequences of war as they affect power distribution cannot be estimated merely by the calculation of comparative national losses. Differences in size and rates of growth render meaningless comparisons of absolute losses. Total national capabilities of a country the size of Denmark are smaller than losses of a major power involved in the same great war. Again, identical absolute losses for a fast- vs. a slow-growing nation imply greater absolute losses for the faster growing nation because of its expectation of greater growth. Standardization is obviously required.

We have used three methods. First we constructed an index. Theoretically, the optimal choice of a base year would have been 1930, for it stands at the halfway point in the period under analysis. This was, however, a year of general depression; thus, 1913, the base year established by Angus Maddison who provided us with the original data, was a more useful point of departure, for it was the last year of a very long period of peaceful growth.

We also held constant the size of all nations in our sample to avoid the distortions produced by any changes in boundaries rather than by human or material losses incurred during the war. We dealt with the problem by using throughout the boundaries of 1970. Maddison, whose national product data we used, had followed the same procedure. In his adjustments of his gross national product series Maddison assumed territorial changes to influence output in direct proportion to population changes.[13]

And we chose *years* as our unit of measure in order to cope with differences in size and growth rates among nations. Our estimation however was complicated by the fact that our original index was logarithmically transformed and prevented simple evaluations of losses in time units as deviations from the original regression, because algebraic manipulations could not be applied to them. This difficulty was resolved by reversing the dependent and independent variables in the regression estimates and reducing the resulting residuals to time units.[14] For time is obviously a commodity equally available to all and one that is not affected by growth rates. Thus, when we say that one country has lost or gained 15 years, we mean only that its power resources diminish or increase relative to the performance of that nation in the base period. Because we thought it important to establish negative signs corresponding to losses and positive signs corresponding to gains, the residuals are multiplied by -1.

Independent estimates of normal performance were run for all relevant nations and included the study of each of the wars considered. In all cases, the model was:

Time = a + b log of the index of total output of each nation + error, where Time = 0, 1, 2, . . . , n, where n is the length of the base period.

In this model, the slope and intercept have no significance; only the residuals are important. Two-variables are used in the estimate, and goodness-of-fit measures are not affected when we once again reverse the model to the more familiar function, with total output as the dependent and time as the independent variables.

The Reliability of the Projections. As we have noted earlier, estimates of normal growth after a war depend on stable data for trends in the prewar period. A variety of interrelated statistical techniques was used to determine the reliability of the regression estimates, given the assumptions of that model. The coefficient of determination, R^2 (showing the proportion of total variance explained), was used to estimate the reliability of the trend in the base period. Average values were utilized to establish levels of tolerance for residuals central to the evaluation of costs of war.

Table 1 displays results of major tests conducted. It is evident from a consideration of Table 1 that the coefficient of determination for the period before World War I is consistently in the high nineties, and that only in the case of the United Kingdom and Japan can one obtain an R^2 figure below that level, .80 and .87 respectively. Such estimates are not unexpected, given the behavior of the series in that period. Thus, one could consider projections of normal trends after World War I to be stable and reliable.

Results of World War II, following similar criteria, are less satisfactory. Only after controls are imposed to minimize the effects of depression does the coefficient of determination for most countries rise to the levels reached for World War I. Without such controls, coefficients run as low as .04 for Austria, .30 for the United States. But in the cases of some

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[13] Angus Maddison, *Economic Growth in the West* (New York: Twentieth Century Fund, 1964), pp. 194–203; see also Kugler, pp. 31–36.

[14] Thomas Sanders, Lutz Erbring, Department of Political Science, and J. Landwehr, Statistical Research Laboratory, University of Michigan, generously contributed to the solution of this difficult methodological problem. They are not responsible, of course, for any weaknesses of the final product.

Table 1. Measures of Goodness of Fit and Range of Residuals in Base Period

	World War I					World War II					
			Residuals for Base Period 1895–1913 (Years)						Residuals for Base Period 1921–1939		
Nation	Number of Observations	R^2	Max. Loss	Max. Gain	Last Year (1913)	Number of Observations	Depression Years Excluded	R^2	Max. Loss	Max. Gain	Last Year
United States	19	.97	–1.7	2.0	–.9	19 16	 1932–1934	.30 .66	 –6.2	 4.6	 2.6
West Germany	19	.99	–1.0	1.0	.7	19 16	 1931–1933	.76 .87	 –4.5	 2.7	 –1.9
United Kingdom	19	.80	–6.0	5.2	–.1	19 16	 1931–1933	.83 .84	 –2.9	 2.3	 –2.3
Japan	19	.87	–4.1	5.0	–.3	19 –	 none	.92 –	–2.4 –	2.6 –	– .7 –
USSR	3		Special Estimate			19 16	 1931–1933	.98 .98	 –1.3	 1.5	 .7
Italy	19	.96	–2.4	1.9	–.6	19 16	 1931–1933	.80 .80	 –4.2	 3.3	 .8
France	18	.92	–2.4	2.7	1.2	19 16	 1931–1933	.56 .56	 –5.1	 5.5	 4.5
Belgium			Data Unavailable			19 16	 1931–1933	.65 .67	 –5.1	 5.5	 4.4
Denmark	19	.98	–1.5	1.3	.6	19 16	 1931–1933	.95 .95	 –2.8	 2.2	 .3
Sweden	19	.98	–1.8	1.5	1.4	19 16	 1932–1934	.89 .93	 –2.5	 2.1	 .1
Switzerland			Data Unavailable			19 16	 1931–1933	.79 .79	 –3.6	 4.8	 3.0
Canada	19	.97	–1.6	2.3	.6	19 16	 1932–1934	.37 .59	 –5.9	 6.1	 3.2
Australia	19	.96	–2.4	1.8	.4	19 16	 1931–1933	.56 .79	 –5.0	 6.4	 1.1
Netherlands	14	.96	–1.3	1.1	.7	19 16	 1932–1934	.75 .78	 –4.0	 4.4	 2.4
Norway	19	.95	–2.2	2.3	2.3	19 16	 1931–1933	.98 .98	 –1.2	 2.0	 .1
Czechoslovakia			Not a Nation			17 14	 1933–1935	.47 .61	 –6.8	 3.4	 n.a.
Hungary			Not a Nation			15 12	 1931–1933	.65 .72	 –3.4	 3.7	 n.a.
Yugoslavia			Not a Nation			19 16	 1932–1934	.80 .83	 –4.9	 3.9	 .1

nations considered, even after controls are established, the coefficients remain low: Austria (R^2 = .17), France (R^2 = .56), Canada (R^2 =.59), Czechoslovakia (R^2 = .61), the United States (R^2 = .66), and Belgium (R^2 = .67). Such evaluations require exegesis.

Austria's performance is explained by her troubled history in the postwar period: Austria-Hungary was dismembered after World War I; the economic depression was particularly acute; there was civil war; and finally, Austria was absorbed by the Third Reich. A stable estimate was not possible. Therefore, Austria was dropped from the sample. France also represents a special case. French growth was very slight in the interwar period, and the regression estimate, while producing a good fit, was statistically not significant because it was close to zero. The remaining low correlations show the effects of the depression of the 1930s. Although the impact of the economic catastrophe is minimized when outliers are removed from the calculations, it is plain from Table 1 that the effects of the depression are still visible; residual values of maximum losses reach −12.3 years in the case of the United States when no controls are imposed and are cut almost in half, to −6.2 years, when three outliers are deleted.

The residuals in our table indicate three points: (1) estimated trends are more reliable for the period prior to World War I than for that before World War II; (2) we expect that the performance of nations over time in the sample will not depart more than two or three years from the projected line during the period covered; (3) the sharp improvement in values in the last year of observation suggests that the forces depressing the economies of the nations considered were being overcome.

One more important item should be noted. Because the information contained in the residuals was so crucial to this study, some additional tests were carried out. In the base period for World War II, some residuals were high when previous values were high. These traces of tracking, however, were sharply reduced when the outlying points for the depression years were eliminated. This operation considerably increased our confidence in the accuracy of the regression model designated.[15]

Choice of Test Cases

In discussing problems connected with the measurement of war consequences, the choice of conflicts to be used is vital. If results are to represent what actually happens as a consequence of war, test cases must be representative as well. Since it is not possible to draw a representative sample of conflicts, it is important to choose those which maximize opportunities for the behaviors we wish to study to surface. If, as the results of such extreme tests, consistent patterns do not occur, we could at least assert with confidence that different findings were not likely to be obtained in less extreme cases.[16]

Thus, our choice of wars to consider could not be arbitrary. We sought two conditions: (1) wars whose contestants were totally committed to victory; otherwise, we would not know if the actual victors would have won had the vanquished made a more serious effort; (2) wars whose consequences were not altered by the interference of other powers. We looked for wars in which the nations which appeared to be victors and vanquished were so in reality, to insure that the consequences ascribed to victory and defeat were properly matched.

Some categories of wars had thus to be excluded: wars between small powers and limited wars did not offer conditions suitable for the testing of our propositions. Even assuming that small countries fought as devotedly as possible, one could not be certain that the consequences were not determined by the behavior of nations not directly participating. The Middle East offers a good illustration of this difficulty: Egypt, Syria, Jordan, and Israel have been combatants in several all-out wars over the years. All contestants have fought to the limits of their strength. Yet the losers have always alleged that the outcomes and consequences were chiefly influenced by the interference of outside interests, specifically, the United States and the Soviet Union. Consequently, such conflicts do not provide adequate tests for our hypotheses.

Limited wars are equally if differently perplexing. In such conflicts, one or both sides limit their efforts and their objectives. Outcomes, therefore, demonstrate only what the contestants actually accomplished, not what they might have achieved had they committed themselves fully to the fight. We must select for testing wars sufficiently important and hard-fought to eliminate possible suspicion that the combatants were not exerting maximum efforts. Limited wars, therefore, do not make good test cases for our propositions.

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[15] See Kugler, Ch. 4 and App. III.

[16] This procedure was used by economists in the study of economic depressions. See Arthur Burns, ed., *The Business Cycle in a Changing World*, 2nd ed. (New York: National Bureau of Economic Research, 1969), pp. 3–53.

We have chosen for consideration conflicts[17] in which the contestants on each side included at least one major power as an active participant, for wars involving great powers can be expected to be massive undertakings, and their outcomes are not easily influenced by external forces. We also insured, in two ways, that the great powers were themselves completely committed to the struggles. We stipulated that wars chosen should show a quantum leap in the number of battle casualties, by comparison with the carnage produced by any previous war. We further insured that the commitment of all combatants was absolute by selecting wars on whose outcome depended loss of territory and/or population.

The availability of data is a final and indispensable criterion in the selection of wars suitable for the testing of our proposals. Lack of data has been a problem besetting most rigorous studies of international politics. Only very recently has research made available reliable time series data, in a small number of areas, going back to the end of the nineteenth century. Such series, however, are still very rare; quality and availability drop virtually to the vanishing point when material occurring in the period prior to 1900 is at issue.[18]

The stringent theoretical and data requirements reduced to a very small number the sample of utilizable wars. Although the Napoleonic Wars and World Wars I and II meet the theoretical criteria we have established, only for the latter pair have we time series data at frequent enough intervals and of high enough quality to permit the analyses we wished to undertake. Here, therefore, we must raise the question of whether or not the analysis of two wars can be more generally representative than the examination of simply two case studies, with all the limitations this approach has on the drawing of broad inferences beyond the cases themselves.

We think that the results of this study should be considered sources of significant generalization because, although the number of wars we analyze is extremely small, the sample of 31 cases we observe is adequate. These cases are observed throughout the two most terrible wars in history and include most of the countries of the central international system. Moreover, the two great wars we examine are those which offer the best opportunity for testing our propositions. The reduction of our area of study to two wars undoubtedly has sharply restricted the nature of the inferences that one might wish to make. On the other hand, the process developed allows tests on a much enlarged sample as soon as improved theory, methodology, and data permit it.

Actors

Some would argue that in the study of war one does not learn much of value if sole attention is given to the behavior of nations, and that other levels or kinds of analysis are more appropriate. In some cases, one might justifiably prefer to concentrate on units of analysis other than nations. Were one interested in questions related to causes and outcomes, particularly the former, it might be meet to observe the views and behavior of military leaders, politicians, diplomats, industrial and labor officials, and mass publics. However, if one is interested in the effect of victory and/or defeat in war on the distribution of international power in postwar periods, this is not the case. Here decision making and the behavior of individuals play an inconsequential role; the nations are the only actors.

Casting nations as uniquely relevant actors poses its problems. In order to compare the performance of a country before and after war, one must be certain that one is comparing the behavior of the same entity over time; otherwise, differences in behavior would legitimately be ascribed to the fact that the entity itself is different. The problem inherent in this kind of comparison is that nations are not as stable over time as one could wish. Ideally, one would expect a nation to have as a minimum: (1) a fixed territory and population; (2) governmental sovereignty over both; and (3) the formal diplomatic recognition of most of the international community. Reality is different from the ideal. Nations shrink and expand, established countries disappear, and new ones appear in their places. Some are administered, at least in part, by international bodies, while still others make a token effort to supervise populations and territories within their legal jurisdiction but in fact are regulated by rebel groups prompted by authorities operating abroad. Increasingly, this reality has been recognized;[19] the most exhaustive list of nations compiled to date has been established on the premise that not all the elements of sovereignty, territorial continuity, and international recognition need be present all the time.[20]

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[17] We accept Quincy Wright's theoretical definition of war as "the legal condition which equally permits two or more hostile groups to carry on conflict by armed force," in *A Study of War*, abr. ed. (Chicago: University of Chicago Press, 1964), p. 7; and we further accept the operational definition of Lewis Richardson that any conflict resulting in the death of approximately 300 people can be considered a war. Since our concern is with international war we utilized the list of wars between 1815 and 1965 provided by J. David Singer and Melvin Small in their *Wages of War, 1816–1965: A Statistical Handbook* (New York: Wiley, 1972), pp. 17–19, 30–32, and 58–70.

[18] The bulk of the capability data for our study comes from Angus Maddison, "Trends in Output and Welfare," unpublished manuscript, 1972. The gratitude of the authors goes to Dr. Maddison, who generously allowed us access to a data set, as yet unpublished, which made this study possible. For a complete data set see Kugler, App. II.

In seeking the cast of nations whose performance we should measure, it was clear that it should incorporate all belligerents in World Wars I and II, with a group of nonbelligerents serving as a control group. We began with the comprehensive list of belligerents compiled by Singer and Small in their classic data-gathering effort, *The Wages of War,* to which we added, as an initial massing for a control group, all of the nations in existence at the time of the conflict under study which had not participated in it.[21]

The first list had to be pared. We immediately eliminated from our sample all nations that were not developed,[22] since there was no way to estimate their power satisfactorily. A number of combatants in the two wars had to be dropped from our consideration for this reason. This category is unfortunately long, including Mongolia, Bulgaria, Rumania, Ethiopia, Greece, Brazil, and Turkey. On the other hand, South Africa, New Zealand, Poland, and Finland were eliminated from the study because data were inadequate.

We also had to take into account the fact that Australia, Norway, Germany, Japan, and Austria were not totally free and independent for part of the period important to this study. Australia had not achieved dominion status; Norway had not been separated from Sweden. *De jure,* therefore, they were not free, although they were free in fact and in action. Finally disregarding their legal status, we treated them as free and independent units. Germany and Japan were not free because of their occupation after World War II; Austria was annexed by Germany before the onset of that war. For our purposes this inhibition could be devastating: loss of freedom of action was not important if it occurred during a war, but a nation under the control of one or more other nations either before or afterwards might lack independence in the allocation of its resources; this, in turn, could be a factor influencing its prewar pattern of growth or its recovery period. We decided that there was no reasonable way to establish control for the possible data distortions due to foreign occupation and determined simply to consider as continuous units all nations which recovered their identities after occupation. Germany and Japan were included without giving weight to their occupation periods. This sample was further reduced when we lost Spain and Austria because reliable estimates could not be generated.

The Analytic Groups: Belligerents/Nonbelligerents; Winners/Losers; Active/Occupied. Having assembled our basic components, we divide our cast into belligerents and nonbelligerents, winners and losers, both active and occupied. First, a distinction must be made between nations which were belligerent and those which were merely hangers-on, always present in major coalitions. This determination is not easy to make. Argentina, for example, declared war on the Axis in the waning days of World War II. She never dispatched a soldier to the war area, and for most of the war her sympathies were clearly with the opposite side. Was Argentina a belligerent? Brazil is another case in point: she sent only a minuscule force to fight, and her principal contribution was permission to use an air base for refueling planes destined for Africa. How is one to separate the belligerents from the countries whose behavior ranged from total neutrality to one of merely symbolic participation?

Much the same kind of difficulty presents itself within the category of winners. The position of France in World War II is an example of this problem: for much of the war France was fully occupied by Germany. Yet France began and ended that war on the side of the winning coalition. Was France a winner, in the same sense as England, the Soviet Union, or the United States can be thought winners? One would be forced to reject such a notion. The same distinction separates losers. Italy began the war on the side of the Axis, but switched allegiance in the middle, and ended as a member of the Allied forces. Is Italy to be construed a winner or a loser?

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[19] See Deutsch, p. 70; John Herz, *International Politics in the Atomic Age* (New York: Columbia University Press, 1959), p. 104.

[20] Singer and Small, pp. 58–90 and Bruce Russett, J. D. Singer, and Melvin Small, "National Political Units in the Twentieth Century: Standard List," *American Political Science Review,* 62 (September 1968), 932–51.

[21] Ibid.

[22] Wherever more than 50 percent of the working-age males were in non-agricultural pursuits, the country was considered developed.

Operational distinctions such as those above require definitions which clarify any ambiguities arising from the theoretical conceptions; thus the following definitions of the analytic groups:

1. Belligerent nations are those whose participation in the conflict resulted in military losses of 5000 troops.
2. Nations whose strict neutrality or symbolic participation resulted in losses of less than 5000 troops were considered nonbelligerents.
3. Nations still fighting in the final third of the conflict on the same side as at its beginning were classified as active belligerents.
4. Belligerents which in the last third of the conflict were no longer members of the coalition they had joined at the beginning were classified as occupied belligerents.
5. Nations that retained all their territories or extended them immediately after a conflict and as a direct result of that conflict were considered winners. The rational support for such a definition is plain: no victor in a major war would tolerate the loss of any territory under its jurisdiction.
6. Loss of territory would be construed as the behavior of a loser. Even transfer of territory, with full compensation made for that loss, is considered an overt sign of defeat; for no victor would submit to such terms, and only a loser would have no alternative.[23]

The analytical groupings that we use from different combinations of our three fundamental dichotomies—belligerent/nonbelligerent, active/occupied, and winner/loser—do not exhaust all the logical combinations one could explore. They do, however, satisfy the theoretically interesting possibilities for the analysis of war. Such groups can be most simply and elegantly expressed in the theoretical language of set theory:

Active Belligerent Winner $= B \cap (O \cup T)^C$
Active Belligerent Loser $= B \cap T \cap O^C$
Occupied Belligerent Winner $= B \cap O \cap T$
Occupied Belligerent Loser $= B \cap O \cap T$
Nonbelligerent $= B^C$

The above can best be represented in a Venn diagram (Figure 2).

We are also interested in an additional distinction that crosses all the analytic groups we have established. We should like to explore the consequences of war for major powers as well as for the entire group of nations; we should also like to analyze the consequences of both wars and then, individually, for each of the two wars. The reason for this attempt to disaggregate is that our sample is so small that it is imperative to see whether or not the patterns of behavior we find are a by-product of our aggregation and, as such, not present when disaggregation occurs. Two points need emphasis, however. First, we do not wish to disaggregate at the level of individual nations. Second, the results of disaggregation are not

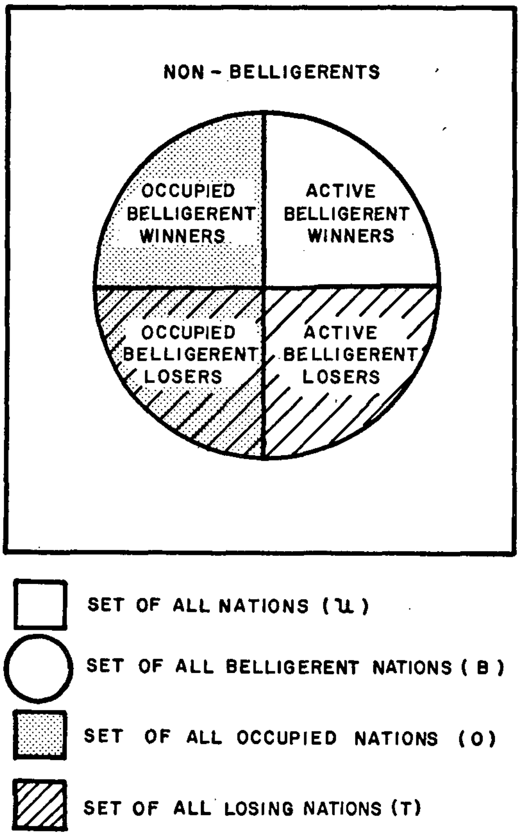
NON - BELLIGERENTS
OCCUPIED BELLIGERENT WINNERS
ACTIVE BELLIGERENT WINNERS
OCCUPIED BELLIGERENT LOSERS
ACTIVE BELLIGERENT LOSERS
SET OF ALL NATIONS (𝔘)
SET OF ALL BELLIGERENT NATIONS (B)
SET OF ALL OCCUPIED NATIONS (O)
SET OF ALL LOSING NATIONS (T)

Figure 2.
Venn Representation of Analytical Groups

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[23] Charles Kindleberger, "International Political Theory from the Outside," in *Theoretical Aspects of International Relations*, ed. William Fox (Notre Dame: Notre Dame University Press, 1959). Our operational indicator of a loss in war is based on the observation that winners in the past have extracted from losers some territory, while never giving up any of their own. We assume, but have no evidence, that losers would have exacted territory from their adversaries had they won the war and cannot rerun the conflicts to see what would have happened had the outcome been reversed.

independent findings for they are based on the same sample.[24]

The distribution of our total cast of nations into analytic groups discussed above can be seen in Table 2. The sample of 18 nations and 31 cases respectively was the very best we could assemble given our theoretical constraints and the lack of data. It is scant, but sufficient for the research we wished to undertake.

Theoretical and Empirical Propositions

Theoretical Propositions. To many observers, the power position of members of the international system seems obviously influenced by their choice to participate in conflict and by their fortunes in war. Most scholars and practitioners have believed that it made the greatest difference to the power of a country in the modern age whether it won or lost a war. Very few disputed this view.

In our effort to separate the long-run from the short-run consequences of war and to present, in an approximate way, the distribution of opinion, three important theoretical propositions, with different short- and long-term results, seem to emerge.

The first proposition asserts that in the short run the movement of power between winners and losers creates a pattern much like the gradual opening and then closing of a pair of scissors. J. M. Keynes maintained that the gap which developed between winners and losers as a result of war would continue to increase for a short time in the future and the losers would fall behind at an ever-growing rate. Interestingly enough, Keynes also thought that in the long run, losers laid waste by economic ills would contaminate their partners in trade and bring chaos to the entire system. Thus, the winners would move downward to join the losers, and the gap between them would tend to disappear.[25]

Table 2. Final Sample of Nations Used in the Analysis

	Active Belligerent Winners	Active Belligerent Losers	Occupied Belligerent Winners	Occupied Belligerent Losers	Nonbelligerents
Australia	I & II[a]				
Belgium			II		
Canada	I & II				
Czechoslovakia				II	
Denmark			II		I
France*	I		II		
Germany (West)*		I & II			
Hungary		II			
Italy*	I	II			
Japan*		II			I
Netherlands			II		I
Norway			II		I
Sweden					I & II
Switzerland					II
Russia (USSR)*	II	I			
United Kingdom*	I & II				
United States*	I & II				
Yugoslavia			II		
Total WWI	6	2	–	–	5
Total WWII	5	4	6	1	2

[a] I = World War I; II = World War II; * = Major Powers.

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[24] Our results are not simply the effects of the aggregations. The number of nations in each of our major analytic categories is very small; however, the number of points used in our base period in order to make our calculations is quite adequate. Conscious that aggregations inevitably distort results, we inspected the behavior of each of the countries to see if their individual performance *deviated widely* from the performance of the analytic groups, and we found that this was not the case. Moreover, similar behaviors of the analytic groups is found in partition after partition making clear that aggregation does not cause the results obtained.

[25] John Keynes, *The Economic Consequences of the Peace* (New York: Harcourt, 1920). For discussion of the short-term effects of wars on belligerents, see United States Strategic Bombing Survey, *The Effects of Strategic Bombing on the German War Economy* (Washington, D.C.: Government Printing Office, 1945); cf. also, *The Effects of Strategic Bombing on*

A second view suggests that as a result of war all nations lose national power, but the winners do not lose as much as losers, and the gap thus created between victors and defeated nations continues for a lengthy period of time. Norman Angell was a supporter of this notion.[26]

A third proposition, advanced by A. F. K. Organski, asserts that while it is indisputable that losers suffer a good deal more than winners and are in a much worse position immediately after conflicts, levels of power distribution return reasonably soon to the patterns they would have followed had no war taken place. The mechanics of the change work in approximately the following way: After their defeat (and the plummeting of their capabilities) losers accelerate their recovery. Winners, in the wake of victory, show a rate of recovery in capabilities depleted by war which is substantially slower than that of losers. Neutrals are not affected. Within a relatively short period of time, all nations return to the levels of national capabilities they would reasonably expect to hold had there been no war. There is a convergence of winners and losers; the major reason for this seems to be the speed with which the losers recover. They appear almost literally to rise from the ashes of their defeat.[27] There is a "phoenix factor" at work.

Empirical Propositions. Let us phrase our empirical propositions in the precise operational language of this study. We have eight hypotheses: The first four represent short-range expectations; the second four represent long-range expectations. Our propositions for short-range effects are:

H_1—Belligerent countries that emerge as winners of major wars gain in power; nonbelligerent countries retain their antebellum power patterns; belligerent losers suffer substantial power losses.

H_2—In the period immediately after a war, belligerent winners and nonbelligerents retain antebellum power patterns; belligerent losers suffer substantial power losses.

H_3—After major wars, all belligerent countries suffer major losses in their power capabilities; nonbelligerents retain antebellum power patterns.

Our null hypothesis is stated thus:

H_4—After major wars, the power patterns of belligerents and nonbelligerents are not affected in a systematic manner.

Our propositions for long-range effects are:

H_5—All groups involved in war suffer long-range losses of power and do not regain power patterns at levels established before the onset of major conflicts.

H_6—Belligerent winners and nonbelligerents retain antebellum levels of growth; power cleavages between belligerent winners and nonbelligerents, on the one hand, and belligerent losers, on the other, are rapidly erased by the accelerated recovery rate manifested by losers.

H_7—Differences which result from victories and defeats in major wars are maintained or even slowly increased; thus, the immediate postwar gap between belligerent winners and nonbelligerents, on the one hand, and losers, on the other, is maintained or possibly enlarged.

Our null hypothesis for long-range expectations is:

H_8—The postwar patterns of all groups considered are not affected in a systematic manner as a result of war.

Two final points: it should be clear that in order to test our propositions about long-range effects, the null hypothesis for short-range expectations must be disproved. Second, the two sets of hypotheses, as well as the theoretical propositions that precede them, do not represent all the logical possibilities, but do summarize the views expressed in the literature regarding the outcomes of war and the effects of these outcomes on power distribution.

Now let us turn to what we have found.

Findings

We first wish to examine the behavior of the entire sample; then, in order, that of the subset of great powers taken together; and then that of both for each war taken separately. Had our sample of countries and wars been larger, the performance of all countries taken together would have offered evidence of the kind of behavior we could expect from winners, losers, and neutrals as a consequence of even a major war. However, due to the smallness of the sample, it seemed wise to explore, as deeply and broadly as possible, how important subsets of the total sample behaved. It is important to note that while each of the partitions of the

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Japan's War Economy (Washington, D.C.: Government Printing Office, 1946).

[26] Two works argue at length that there are enormous permanent losses as a result of war. See Norman Angell, *The Great Illusion* (New York: Putnam, 1933), and John Nef, *War and Human Progress* (Cambridge, Mass.: Harvard University Press, 1950).

[27] For the effects of war on belligerent populations, see United Nations, *The World Population Situation in 1970*, Population Studies No. 49 (New York: United Nations, 1971).

total sample is separately and discretely observed, they are not independent, since the sample is identical for all.

The first partitions permit us to see the behavior of all nations in both wars. Only three of our analytic groups (active belligerent winners, active belligerent losers, and nonbelligerents) are considered. Occupied belligerent winners and losers are excluded because, by our definition, there were no occupied countries during World War I.

The results (Figure 3) show that in the first two postwar years, belligerent winners and nonbelligerents lose from 1.5 to 3.5 years. The deviation of both groups from expected performance is minimal. Active belligerent losers, however, suffer losses in a comparable period of 20.4 to 21.6 years. The difference between the two groups is substantial: 19 years.

In the long run, the nonbelligerent nations retain growth rates characteristic of their prewar performances. Active winners incur an average deviation of about three years from the zero line, indicating that losses suffered after postwar demobilization are maintained. Among active belligerent losers, however, the Phoenix phenomenon manifests itself. Losers begin and maintain a steadily accelerating recovery rate after the war and overtake the winners in the eighteenth year of the postwar period. At the conclusion of that period, differences in power distribution among all groups have been eradicated; levels of power return to points one would have anticipated had no war occurred.

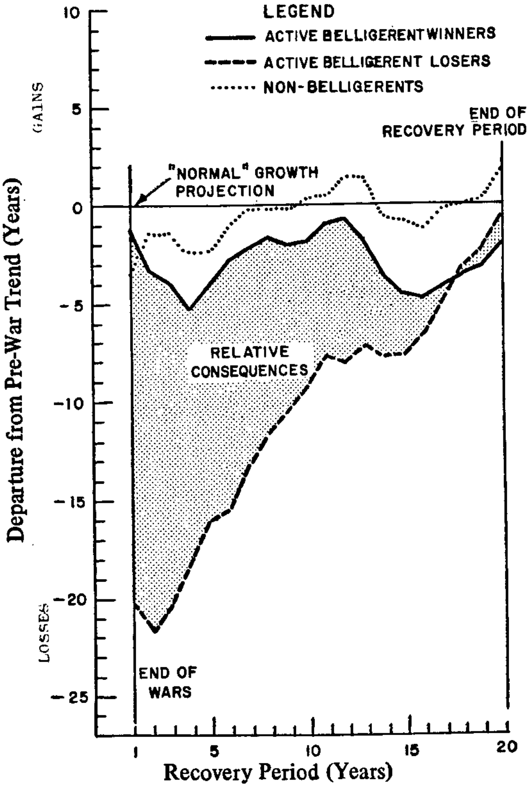

Figure 3. Partition I: General Consequences
Sample: All Nations, Both Wars

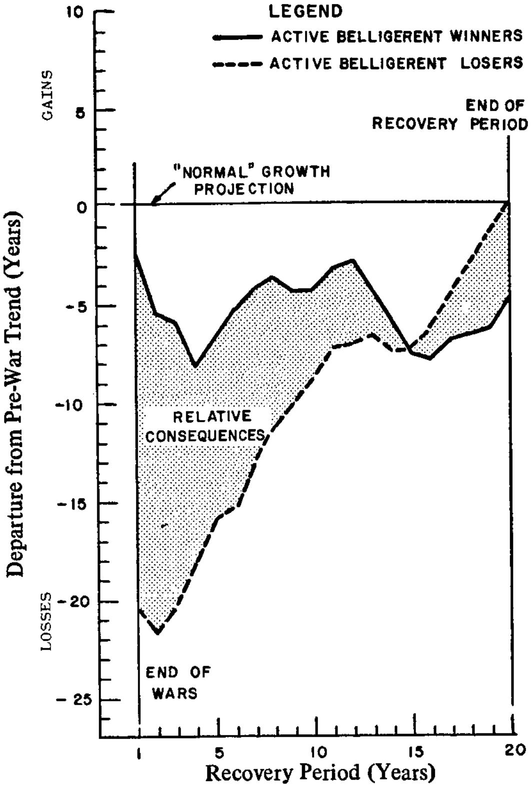

Figure 4. Partition II:
Major Power Consequences
Sample: Major Powers, Both Wars

Thus, the results we obtained strongly support hypotheses two and six.

In testing the second subset of actors in our breakdown, the behavior of the great power system over two wars, the results are very much the same (see Figure 4). Only active belligerent winners and losers are considered, for none of the great powers was neutral in World War II. Active winners begin with a two-year loss immediately after the war, then recover briefly, only to slide away once more. Overall, their performance is below expected levels and is heavily influenced by depressions in the interwar period. However, the trajectory as a whole reveals no loss in relation to prewar capabilities.

The active belligerent losers, on the other hand, suffer a 20-year loss immediately after the war, but recover rapidly, overtaking the winners in the fifteenth year. After this point, both groups resume previous patterns.

Hypotheses two and six are confirmed again by a study of the composite data of the great powers for both wars. In our next four operations, we partitioned our sample to show the performance of the entire system and the subsystem of great powers for each world war.

Our third partition permits us to observe the behavior of the entire system after World War I (see Figure 5). In the short run, nonbelligerents appear slightly affected during the first year after the war, but remain within one year of the zero line during the second. The active belligerent winners incur losses of from five to seven years right after the conflict. Active belligerent losers suffer losses of from 21 to 25 years. Winners lose, but losers suffer four times more severely. There can be no doubt that in the short run, there are serious power consequences both to winners and losers of major wars. Again, the evidence in large part supports hypothesis two, although active belligerent winners do suffer markedly.

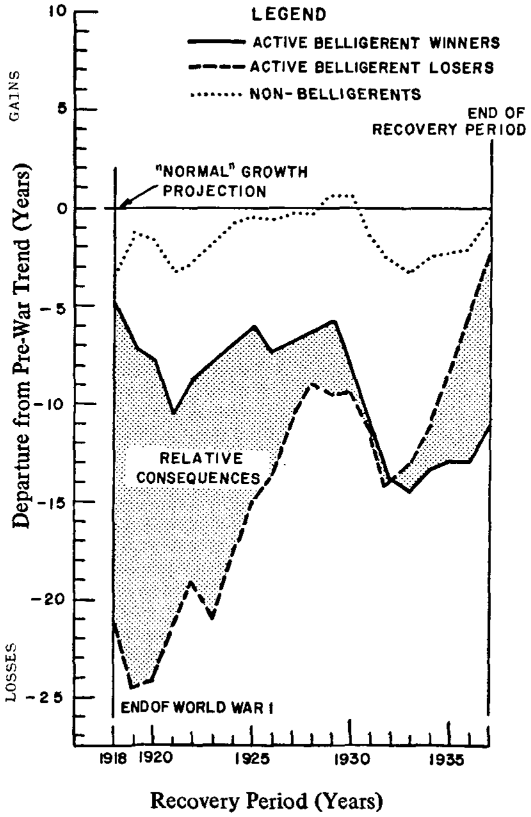

Figure 5. Partition III:
World War I Consequences
Sample: All Nations, World War I

The evaluation of long-term consequences is intimately related to the effects of economic depression. Consider first the initial 12-year period after World War I. The characteristics described in hypothesis six are supported. Active belligerent winners recover very slowly from war effects; nonbelligerents retain previous growth patterns; active belligerent losers, after the immediate postwar period of heavy loss, display a substantially faster rate of recovery than winners. Then the Great Depression strikes and, within two years, performances of all groups are diminished. Belligerents, however, seem to suffer more than nonbelligerents. After 1933, predepression trends reestablish themselves, but the subsequent period is too short for adequate evaluation.

The great power system during World War I behaves in much the same way described for the sample as a whole (see Figure 6). Active belligerent winners fare slightly worse than the whole sample, but differences are so marginal that we feel secure in concluding again that the results support hypotheses two and six. We draw this conclusion despite the disruption caused by the depression. The graph shows the depression to be a major factor in distorting the recovery patterns of all countries taken together, not merely for World War I but for both conflicts. Moreover, the depression is also a major reason why our projections for the period following World War II are so weak, underestimating egregiously the growth trends (see Figure 7).

In our fifth partition, all major analytic groups are represented for World War II. Some of these representations, however, are so tentative that one needs to take the information they convey with caution.

The consequences of World War II on power distribution, in its immediate aftermath, are much as expected. In the first year, active belligerent winners are slightly ahead, but move toward the zero line in the second year. Nonbelligerents are slightly below the zero line but move to points within tolerance limits (two years for this partition) in the second year. Active belligerent losers suffer substantial losses, between 16 and 17 years, in the first 24 months after the war, and the occupied belligerent winners lose from 11 to 14 years in the same period. The differences of loss between

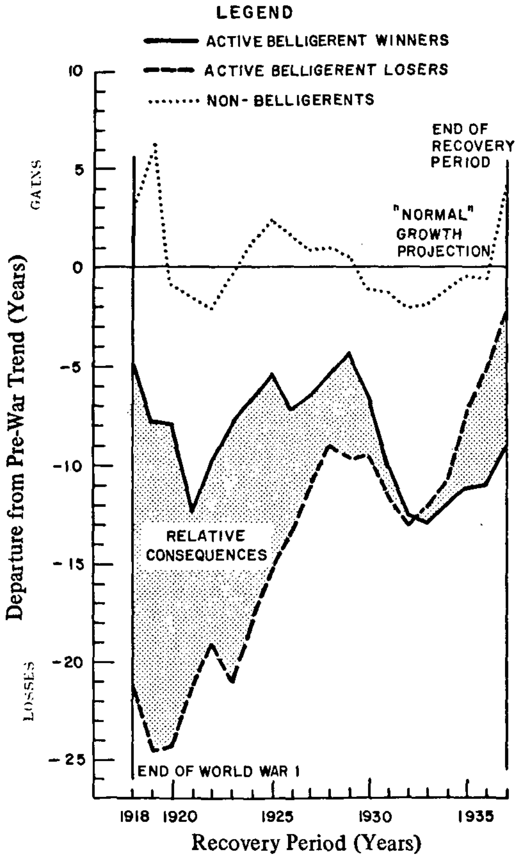

Figure 6. Partition IV:
World War I Major Power Consequences
Sample: Major Powers, World War I

active winners and active losers range from 21 to 23 years in the initial period.

In the long-run analysis we find, for the first time, evidence to support the proposition that the gap between winners and losers can continue instead of closing.

A number of points should be made. The logarithmic projections seriously underestimate the growth of the system. An indication of this is to be seen in the fact that nonbelligerents make increasing gains over time, indicating that prewar patterns distorted by the depression are not a good indicator of the behavior of the group as a whole in the period after World War II. In some part, however, as Kuznets suggests, economic growth since 1945 is due to a liberalization of trade in the industrial world and is therefore unexpected.[28] It is our impression that a more accurate projection of growth trends would place the zero line approximately where one finds the trajectory for the nonbelligerents. In any event, our main concern is with active winners and losers, and we must first establish that distortions in the pattern do not, in that respect, affect relative calculations. Over the entire period, the active belligerent winners maintain a constant but slight edge over nonbelligerents amounting to about three years. Absolute differences between winners and losers are not distorted by the acceleration of recovery rates.

One should note that here, too, active losers enjoy a sharply accelerated recovery pattern and regain the prewar level of growth within the stipulated period. They do not actually close the gap between themselves and the winners because of: (1) the acceleration of the entire system; (2) a decided deceleration in recovery in the last five years of the period,

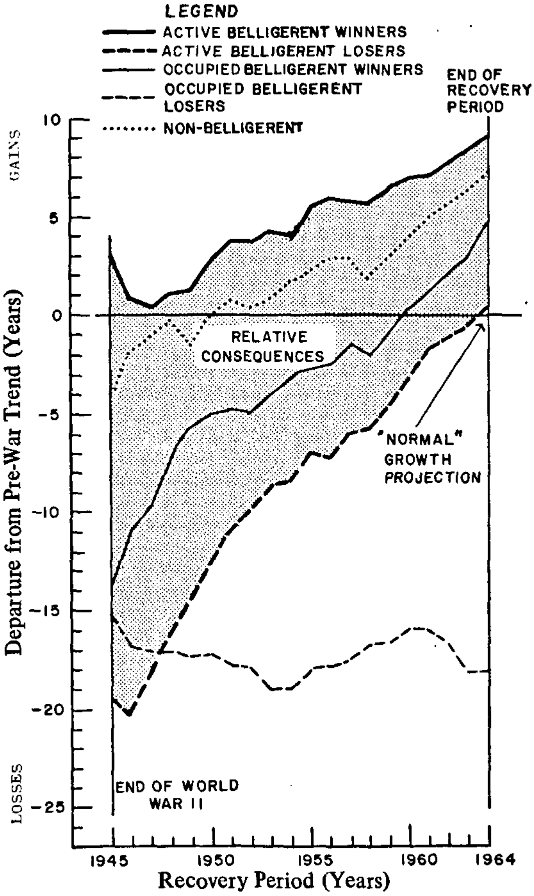

Figure 7. Partition V:
World War II Consequences
Sample: All Nations, World War II

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[28] Kuznets, p. 43.

which may have caused an absolute loss of from five to eight years.

Nevertheless, a gap of roughly nine years remains at the end of the recovery period, and one might argue that we have, as a result, evidence supporting hypothesis seven, that winners maintain the advantage they gain from victory over the long-run.

The remaining analytic groups behave in interesting ways. The performance of occupied belligerent winners is somewhere between that of active winners and losers and follows closely the performance of the former. Occupied belligerent winners regain prewar rates in 15 years, surpass them, and come close to convergence with winners at the end of the recovery period.

Since only Czechoslovakia falls into the category of occupied belligerent losers, one obviously cannot refer to "findings" in observing the behavior of one country. But one should note that the matter merits investigation when sufficient data become available; for if other occupied belligerent losers behave as does Czechoslovakia, we may have identified the real losers in major wars—the nations which do not recover. It is possible that we have also identified the conditions necessary to support an alternative hypothesis to those advanced here. This may be significant. In all the analyses so far offered, we have discerned only marginal differences in the long-range consequences of victory and defeat on the power distribution of the system. However, if the case of Czechoslovakia is a true indication of what obtains for other occupied belligerent losers, it would then be clear that had the victors insisted on occupation, exploitation, and repression of defeated populations, our findings would be dramatically different. It may be that victors can delay the recovery of the vanquished by occupation and repression. For example, if Hitler, with his plans to depopulate and exploit his victims, had won the war, the vanquished might not have recovered. Had Hitler been victorious, hypothesis five might have been sustained.

Be that as it may, the results of the fifth partition should not be viewed separately, but rather must be compared with those of partition six (see Figure 8) for the deviations disappear when one observes only the subsystem of great powers in World War II, which behaves entirely in consonance with the expectations of hypotheses two and six. In the short term, active belligerent winners suffer no loss, occupied belligerent winners and active belligerent losers, suffer a loss of 20 years of "normal" growth in the first two years after the war's end. The relative differences between active winners and the latter two categories in this period are 20.9 and 18.1 years respectively. Evidence on long-term effects fully supports hypothesis six. Active winners maintain expected growth patterns, while active losers accelerate recovery rates and overtake winners in the sixteenth postwar year.

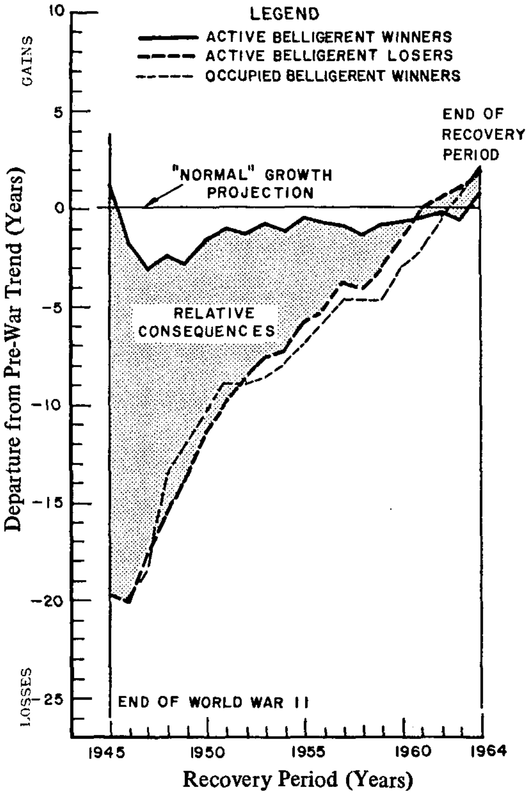

Figure 8. Partition VI:
World War II Major Power Consequences
Sample: Major Powers, World War II

How is one to reconcile these results with those obtained when examining the behavior of the total sample? Some explanatory points should be considered: the differences noted across the two partitions are not rooted in the behavior of all our analytic groups. The behavior of the active losers and occupied winners changes only slightly from one partition to the other. It is also plain that the logarithmic model we chose projected accurately the performance of great powers in the period after World War II. Not projected accurately, therefore, are the behaviors of the smaller winners. Their unprecedentedly rapid growth inflates the performance of the entire sample.

The Phoenix Factor

Most unexpected and interesting is the discovery that after wars the active losers catch up with winners in comparatively short order, and that the system of international power begins to behave as one would have anticipated had no war occurred. We cannot explain the phenomenon; we do not know why losers rise from the ashes as they appear to do.

We can, however, make some surmises. It is plausible to believe that structural elements may play a part. For example, favorable occupational distributions may help to accelerate recovery rates, as may the destruction of obsolescent plants and industrial equipment. It is probable that attitudinal factors may also play a significant role in increasing the pace of recovery. A defeated but economically developed population living in the midst of destruction will recall the antebellum status quo and be motivated to rebuild. So motivated a populace would have the technology to make an economic system function well. It is also plausible that the defeated population would exert a greater effort to recover than that of a victorious country—the latter more intent on enjoying the spoils of war. The necessity for work and sacrifice is evident to all members of a vanquished society. Charles Tilly found, for instance, substantially fewer strikes in Italy and Germany for a time after World War II than in England and France.[29]

These reasons are credible, but we have no assurance that any of them is accurately to be judged responsible for hastening the recovery rates of defeated nations. We do know, however, that one aspect widely believed to be influential had little if any effect. Losers do not rise from the ashes because winners pick them up and help them to their feet. Were this true, it would completely overturn the results we have obtained. For if losers could not recoup their losses without aid, the gap between them and winners would remain if an active winner refused this kind of assistance. This would support the continuing-gap proposition, one of the possibilities previously hypothesized. The point was of major concern, and we tested it with two propositions. First, we were interested to discover whether or not aid was positively associated with the recovery of the recipient and, second, whether or not a large-scale foreign assistance effort does start recovery on its way, as some economists believe.

Our test was simple: since the United States was the major source of aid after World War II, and since such help was dispensed annually between 1948 and 1961, we compared the amounts of aid given by the United States, in totals and per capita, with the relative growth rates of the recipients—Japan, Germany, Italy, the United Kingdom, and France.[30]

Had there been a direct relationship between aid and recovery, and if one controlled for population, growth rates would show increases as a result of aid. Had aid intensity been a factor one would also expect that growth rates would show strong gains after those years when recipients received particularly large gifts. Because we are dealing with time series data we replicated each evaluation controlling for possible linear influences of time. Table 3 displays the connection between United States aid and the recovery rates of the beneficiaries.

From a consideration of the above figures, we can determine only the weakest association between external aid and recovery; the variance explained by the coefficient of determination is always below .1, indicating that growth and foreign aid, totals or per capita, are almost wholly independent of each other. Such relationship as may exist is negative: the countries that received most of the aid for the longest period performed worst. The United Kingdom received much more aid on a total and per

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[29] Charles Tilly, personal communication to the senior author.

[30] One would expect the growth rates of aid recipients to show effects of the foreign help a year after aid had been received. We tested our assumptions of time response by lagging aid and recovery rates for 2 and 3 years and our results remained unchanged. We calculated the recovery rates of each recipient by subtracting the growth rate in the year aid was received from the growth rates posted in the following year. For example, if a nation posted −15 years of growth in 1948 and −13 years of growth in the following year the recovery rate was adjudged 2 years. It should be emphasized that negative numbers do not necessarily indicate a lack of growth but rather a lack of recovery.

Recovery rates were calculated as follows:

$$\text{Recovery Rate} = \text{Relative Growth}_{i+1} - \text{Relative Growth}_i$$

Where i = Recovery Years

Data on aid was obtained from the Agency for International Development, Office of Statistics and Reports, *U.S. Economic Assistance Programs Administered by the Agency for International Development and Predecessor Agencies, April 3, 1942-June 30, 1971* (Washington, D.C.: AID, 1972), pp. 46, 68–76; population figures from Arthur Banks, *Cross-Polity Time-Series Data* (Cambridge, Mass.: MIT Press, 1971), Segment 1, pp. 3–54; our sample included only West Germany, Japan, France, and the United Kingdom as we were only interested in major power behavior. No data on Soviet aid was available.

Table 3. Correlation of U.S. Aid With Recovery of Recipients, 1948–1961[a]

	Aid Years	Aid	Aid Partialed on Time	Aid Per Capita	Aid Per Capita Partialed on Time
Recovery Rate	1948–1961	–.01 (N = 63)	–.17 (N = 63)	–.01 (N = 63)	–.15 (N = 63)
	1948–1953	–.33 (N = 23)	–.21 (N = 23)	–.18 (N = 23)	–.30 (N = 23)

[a] The correlation coefficient R is used to indicate positive or negative influences, but R^2 is never above .1.

capita basis than France; France received much more than Italy; Italy much more than Germany; and Germany much more than Japan.[31] Yet it was Japan that enjoyed the more rapid rate of recovery, followed by Germany, Italy, and France, with the United Kingdom bringing up the rear. It is, therefore, very hard to credit the conviction that foreign assistance and recovery are closely associated.

These particular findings are not completely unexpected.[32] Many economists have questioned the efficacy of this kind of aid.[33] What the figures underscore is that foreign assistance, as a form of investment in the economy of another country intended to incline it toward faster recovery, is not very effective. The variables truly important to recovery lie within the devastated nations themselves. Previous patterns of performance are far more significant than external aid.

Conclusions

We began this inquiry with a number of questions: Does the outcome of a major war reshape the distribution of international power? Does it make a real difference–in power terms–whether a country wins or loses a major war? How long can winners hold on to their advantages? How long do losers stay behind?

Let us begin with a simple list of what we have found.

1. Systematic patterns in the distribution of power (as measured by gross national product) are registered after major conflicts.
2. The power levels of winners and neutrals are affected only marginally by the conflict.
3. Nations defeated in war suffer intense short-term losses; the outcome makes much difference to them in the short run, especially in terms of power levels.
4. In the long run (from 15 to 20 years), the effects of war are dissipated, because losers accelerate their recovery and resume antebellum rates; they may even overtake winners. Soon, the power distribution in the system returns to levels anticipated had the war not occurred. We have evidence that this happens and we can speculate about the explanation, but we have no definitive solution. There is substantial research remaining to be done.

If one wishes to forecast the behavior of a country 15 or 20 years after the end of a war, one should not refer to the outcomes of that war, whether the country in question participated in it or not, whether it was a winner or a loser. The best indicator of the power posture of a nation less than a generation after the conclusion of a war is its performance before that conflict.

One other finding should be mentioned. It is clear that the assistance offered by winners to losers is not a significant factor in the losers' recovery rate.

We are tempted to suggest that the outcomes of war, insofar as international power is concerned, make no difference. We cannot forget, however, that we have found a tracing which indicates that we may have heard only part of the story. While winners cannot help losers to recovery by contributing aid, whatever the quantity, winners may be able to prevent or delay the recovery of losers. If the behavior of Czechoslovakia is an accurate indication, the victor may retard the recovery of the loser by occupation and exploitation. Adequate information on this aspect of the question is not available; there are merely hints on which to base speculation.

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[31] Kugler, pp. 196–202.

[32] The literature on aid is immense; an excellent review is available in I. M. D. Little and J. M. Clifford, *International Aid* (London: Allen and Unwin, 1968). The "big push" proposition was derived from Paul Rosenstein-Rodan, "International Aid for Underdeveloped Countries," *Review of Economics and Statistics*, 43 (May 1961), 107–38, and his classic arguments on the "big push" proposition in "Problems of Industrialization of Eastern and South Eastern Europe," *The Economic Journal*, 53 (June 1943), 204–07, elaborated in "Notes on the Theory of the 'Big Push'" in *Economic Development for Latin America*, ed. Howard Willis (New York: St. Martin's Press, 1961), pp. 57–66.

[33] For a classic discussion of the uses of foreign aid see Milton Friedman, "Foreign Economic Aid: Means and Objectives," and Charles Wolf, Jr., "Economic Aid Reconsidered," in *The United States and the Developing Economies*, ed. Gustav Ranis, rev. ed. (New York: Norton, 1973), pp. 250–78.

Such findings are clearly tentative. In this case, however, the tentative nature of the conclusions should be stressed once more because we have been plagued with data problems. If our findings are confirmed through comparable and more exhaustive researches, the implications for study strategies could be substantial.

If the distributions of international power and changes in that distribution are shaped by differential rates of growth across critical sectors of a nation's life and across nations of that system, and in the long term such rates cannot be altered even by the most violent international interactions, such as major wars, then what must be studied are the causes of such alterations and not the interaction of countries. Thus, independent variables in international relations are *not* found in international relations, but in the growth of the units that comprise the system. Some scholars already study international politics in this fashion, but this is still a very different conception of the field from that traditionally held.

A Stable System of Mutual Nuclear Deterrence in the Arab-Israeli Conflict*

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Many professional observers have come to the conclusion that, despite denials and technical problems whose solutions are not publicly known, Israel already has nuclear weapons or has completed all but the final steps in their fabrication. It is also widely believed that one or more Arab states will come into atomic possession within a 10 to 15 year time frame. The present analysis explores the consequences of the establishment of a regional mini-balance of terror. The central hypothesis is that apocalyptic images and doomsday visions have been accepted too readily and out of proportion to the arguments that are given, and that a stable system of mutual deterrence may be viable in the Middle East and may make a positive contribution to the process of political settlement. Problems of rationality, credibility, second-strike force survivability, escalation, tactical nuclear weapons, accidents, permissive action links, terrorism, preventive war, and the disclosure of nuclear weapons possession are discussed.

Do we stand at the brink of the transformation of the Arab-Israeli conflict to a nuclear stage? Israeli officials have consistently and explicitly denied that Israel possesses the atomic bomb,[1] and they have affirmed repeatedly for over ten years that "Israel will not be the first to introduce nuclear weapons in the Middle East."[2] But at least 16 independent reports published outside Israel have asserted that it either already has nuclear weapons or is going ahead with determination to get them, including statements to this effect by Anwar Sadat,[3] the United States Central Intelligence Agency,[4] *Der Spiegel,*[5] and the Soviet newspaper *Moskovski Komsomoletz.*[6] Mohammed Hussanein Heikal claims to have information that there are 6 to 10 "heavy and primitive . . . dirty bombs . . . almost certainly stored on a particular air base outside Eilat;"[7] Yassir Arafat has reliable reports from "our own sources inside Israel" that there are exactly 3 to 5 bombs;[8] and *Time Magazine* has come into

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[1] For example, Prime Minister Yitzak Rabin said on Danish television on December 17, 1974, that "Israel is not a nuclear power and Israel has no nuclear weapons." *International Herald Tribune,* August 1, 1975. Foreign Minister Yigal Allon repeated in October, "We don't have nuclear warheads." *Newsweek,* October 20, 1975.

[2] This formula apparently was first used by Levi Eshkol in the Knesset, May 18, 1966. In recent years, a caveat sometimes appears that, while Israel will not be the first, she won't be the second either.

[3] Sadat told Iranian publisher Farhad Massoudi that he believes Israel already has nuclear weapons. "The Middle East Ready to Explode, Sadat Says," *New York Times,* December 17, 1974.

[4] "CIA Says Israel Has 10–20 A-Bombs," *New York Times,* March 16, 1976.

[5] "Tod aus der Textilfabrik," *Der Spiegel,* May 5, 1969.

[6] Cited in "Russians Report on Israeli Arms," *New York Times,* August 9, 1972. Additional independent press reports appeared in *Al Ahram,* November 23, 1973, and October 18, 1975; *Boston Globe,* July 31, 1975; United Press International wire reports published February 14, 1976, and November 25, 1976; *New York Times,* July 18, 1970; and November 25, 1976; *New York Times,* July 18, 1970, and October 5, 1971; *Washington Post,* March 16, 1976; *Jane's Aircraft 1972–73,* p. 565; *SIPRI Yearbook 1972,* p. 312; Robert J. Pranger and Dale R. Tahtinen, *Nuclear Threat in the Middle East* (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1975); and London *Sunday Times* Insight Team, *The Yom Kippur War* (Garden City, N.Y.: Doubleday, 1974), p. 282. The leading work on the subject is still an International Institute for Strategic Studies monograph by Fuad Jabber, *Israel and Nuclear Weapons* (London: Chatto and Windus, 1971) which concluded that "declaratory policy aside, Israel has gone steadily ahead in the development of the capacity to build atomic bombs," p. 123.

[7] "Cairo Editor Urges Nuclear Arms for Arabs," *New York Times,* November 24, 1973, and "Israel Has Nuclear Arsenal, Egyptian Journalist Claims," *Los Angeles Times,* October 19, 1975.

[8] Quoted in *New York Times,* April 4, 1975.

possession of information that 13 warheads were "hastily assembled at a secret underground tunnel during the 78-hour period at the start of the 1973 October War [and] . . . were sent to desert arsenals where they remain today, still ready for use."[9]

Many of these reports of Israeli nuclear weapons appear to be nothing more than chains of conjecture and unsubstantiated rumors, often drawing upon other reports based on equally slender evidence. One well-informed U.S. government official told this writer that even the Central Intelligence Agency "doesn't really know, but is just guessing like everybody else." The fact that many observers find the reports credible may have more to do with the perceptual frames underlying images of Israel than with "the facts" themselves. People believe that Israel is obsessed by a complex of insecurity, living under an ambient state of siege. Jewish science is resourceful, and Israel has an impressive technological base for a small country. It might be impossible for a Jewish government, some of whose own members are concentration camp survivors, *not* to take at least the preliminary steps to a "last resort" deterrent, given the widespread belief that the Arabs are bound to get stronger in the future. The secrecy that Israel has maintained around the Dimona reactor naturally has encouraged suspicions and speculation. Even in Israel, according to a March 1976 poll, 62 percent of the people believe that their country already possesses atomic arms while only 4 percent think it does not, and 77 percent think that it *should.*[10] Moshe Dayan has openly called for developing nuclear weapons because, in the long run, Israel cannot hope to match the quantities of conventional weapons that the Arabs can obtain,[11] and because of America's current weakness and the limitations of its mediating role.[12]

The widespread belief that Israel has nuclear weapons seems to have spurred consideration on the part of Egypt and other Arab states as to whether they should pursue the nuclear option also. As early as October of 1965, Heikal argued that it might be necessary for Egypt to build a nuclear capability to deter an Israeli atomic threat.[13] In 1970, Colonel Qadaffi sent the second man in Libya, Major Jalloud, to buy a tactical atomic weapon from Chou En-lai, but he was sent home with the message that every nation must practice self-reliance and that atomic bombs were not for sale.[14] Sadat himself said in December of 1974, "We shall also find a way of having atomic weapons,"[15] and in August of 1975 he formed a top level council headed by the president and including the minister of war, the foreign minister, and the former head of the air force to oversee nuclear development for "all purposes."[16] In May of 1977, President Assad told a group of British journalists that if Israel had nuclear weapons, the Syrians also would acquire them.[17]

In terms of capabilities, several Arab states are now on the threshold of a vast expansion of programs for civilian applications of atomic energy, including the acquisition of reactors for power plants and the training of large numbers of personnel in nuclear technologies. Inevitably, civilian programs reduce the lead-time for the development of nuclear explosives. Egypt has announced plans to set up 10 reactors over the coming 20 years to supply an aggregate 10,000 megawatts of power by the year 2000.[18] The first two reactors, rated at 1,200 MW, have already been negotiated with Westinghouse of the U.S.[19] (By way of comparison, Israel's Dimona reactor, which is the subject of most of the speculative reports on atomic weapons, is rated at only 24 MW.) Egypt is also cooperating with Iran on nuclear projects.[20] Libya is getting a 10 MW "atomic center" from the USSR,[21] and a 600 MW nuclear power plant from France.[22] India, West Germany, and Sweden have also shown interest in helping Colonel Qadaffi's government with its civilian nuclear program.[23] Iraq is getting a 900 MW reactor from France and technical aid in the nuclear field from Italy.[24] Jordanian officials have announced that their country possesses between 200,000 and 300,000 tons of uranium ore in its known phosphate reserves.[25]

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[9] "How Israel Got the Bomb: Special Report," *Time Magazine*, April 12, 1976. Twelve months later, in a less highly publicized survey of the "Nuclear World," *Time* quietly demoted Israel to the class of countries which might achieve a "bomb capability within a few years." May 2, 1977, p. 13.

[10] "Most Israelis Want N-Weapons," Reuters report carried in *Egyptian Gazette*, March 27, 1976.

[11] Quoted in *Jerusalem Post Weekly*, March 30, 1976.

[12] In a press conference in Paris on March 19, 1976, and in a lecture in Tel Aviv on March 29, 1976, quoted in Shlomo Aronson, "Nuclearization of the Middle East: A Dovish View," *Jerusalem Quarterly*, no. 2 (Winter 1977), p. 36. Dayan was out of the government at the time of these statements, and Prime Minister Rabin specifically disavowed any governmental responsibility for such statements made by "Knesset members." Quoted in "No Change in Policy," *Australian Jewish Times*, April 15, 1976.

[13] *Al Ahram*, October 15, 1965. Quoted in Yair Evron, "The Arab Position in the Nuclear Field: A Study of Policies up to 1967," *Cooperation and Conflict*, no. 1 (1973), pp. 19–31.

[14] Reported in Mohammed Heikal, *The Road to Ramadan* (London: Collins, 1975), pp. 76–77.

[15] "Mideast Ready to Explode, Sadat Says," *New York Times*, December 17, 1974. See also, "Fahmy Warns: Egypt Would Make A-Bomb," *Jerusalem Post Weekly*, June 18, 1974.

[16] "Egypt's Nuclear Body," *Canberra Times*, August 11, 1975.

[17] Quoted in "New War Threat," *Australian Jewish Times*, May 12, 1977.

[18] "Egypt Pushes A-Power Ambition," *Christian Science Monitor*, December 22, 1975, and "Middle East Planners Push for Nuclear Energy Development," *Middle East Economic Digest*, 19 (November 21, 1975), 11, 12.

Nevertheless, the Arab civil programs should not necessarily be considered evidence of incipient nuclear weapons development. Most of the reactor agreements announced will not result in completed projects until the mid-1980s, and upon completion all announced facilities will be subject to thorough inspection. For example, the American reactors going to Egypt, according to a statement by Secretary Kissinger, will be "under the most exhaustive safeguards in existence in any country."[26] Moreover, even if one or more Arab states came into the possession of fissile materials *enriched to weapons grade* (and there would be many obstacles to reaching this point), the capacity of such low technology states to fabricate bombs is doubted by some nuclear physicists. While many fabrication technologies can be inferred from open literature,[27] actual production from these plans would require sophisticated facilities, equipment, and materials that are presently available from relatively few suppliers in the high technology states and whose export is controlled by a "trigger list" signaling military nuclear intent. It would be much more difficult for a Libya to conceal a clandestine program than for a middle technology state like India or Israel.[28] By way of illustration, one manifestation of the difference can be seen in the fact that Israel had 2,850 publishing scientists in 1975 compared to a total of 1,192 for all 13 major states of the Arab League,[29] and this ratio of 2.4:1 has remained essentially unchanged since 1967.[30]

However, Arab nuclear development might not depend on indigenous capabilities. India, which is now running a trade deficit with the Middle East in excess of $600 million per year, has signed agreements for the transfer of nuclear technology with Egypt, Iraq, Syria, and Libya.[31] India is sympathetic to the Arab cause, is seeking closer relations with some moderate Arab regimes, and might look upon military nuclear assistance as redressing an imbalance caused by an already nuclear Israel. The last argument might become more persuasive if Israel were perceived to be rattling nuclear sabres at its neighbors. Taiwan, Korea, and Pakistan may also become potential "supplier" states in the future.

In the net assessment, many professional observers and proliferation scholars have arrived at the conclusion that, despite denials and technical problems whose solutions are not publicly known, Israel *has* nuclear weapons and the Arabs will also have them within 10 to 15 years.[32] If the Arab-Israeli conflict *is* transformed to a nuclear stage, we will need a conceptual apparatus to comprehend the first regional "balance of terror" outside the central European theatre. The purpose of the present analysis is to explore the *consequences* of the introduction of nuclear weapons in the Middle East, should it come about, and particularly to examine the possibility of a regional balance of terror as a path to stabilization. The central hypothesis is that apocalyptic visions and doomsday images have been accepted too readily and out of proportion to the arguments that are given. A stable system of mutual deterrence may be viable in the Middle East and may make a positive contribution to the political de-escalation of the Arab-Israeli conflict.

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[19] "Westinghouse to Supply Nuclear Reactor," *Middle East Economic Digest*, 20 (May 21, 1976), 13.

[20] "Cooperation on Nuclear Projects," *Middle East Economic Digest*, 20 (June 25, 1976), 13.

[21] "France to Build Nuclear Power Plant," *Middle East Economic Digest*, 20 (March 26, 1976), 32.

[22] "France is to Build Libyan Atom Plant," *New York Times*, March 23, 1976, 9.

[23] *Christian Science Monitor*, December 22, 1975.

[24] "Italy Signs Atomic Energy Protocol," *Middle East Economic Digest*, 20 (January 23, 1976), 21.

[25] "Uranium Deposits Believed Important," *Middle East Economic Digest*, 20 (March 12, 1976), 15.

[26] Quoted in "Egypt Pushes A-Bomb Ambition," *Christian Science Monitor*, December 22, 1975. On the general subject of American controls, see *U.S. Foreign Policy and the Export of Nuclear Technology to the Middle East*, Hearings before Subcommittees of the U.S. House of Representatives, Committee on Foreign Affairs (Washington, D.C.: Government Printing Office, 1974).

[27] See, for example, Mason Willrich and Theodore B. Taylor, *Nuclear Theft: Risks and Safeguards* (Cambridge, Mass.: Ballinger, 1974).

[28] I am indebted to Professor Carlo Schaerf of the Department of Physics, University of Rome, for calling these problems to my attention.

[29] *Who is Publishing in Science?* (Philadelphia: Institute for Scientific Information, 1976) tallied from indexes.

[30] Compare A. B. Zahlan, "The Science and Technology Gap in the Arab-Israeli Conflict," *Journal of Palestine Studies*, 1, no. 3 (Spring 1972), 22.

[31] David Shirreff, "India Struggles to Close Trade Gap," *Middle East Economic Digest*, 20 (February 13, 1976), 2–3, 34–35.

[32] Fuad Jabber estimated in 1971 that the lead time for a crash Egyptian program would be no more than seven to eight years (p. 141). A team of scientists including Henry W. Kendall, Professor of Physics at MIT, George B. Kistiakowsky, Emeritus Professor of Physical Chemistry at Harvard, Daniel Ford, Executive Director of the Union of Concerned Scientists, and George Rathjens, Professor of Political Science at MIT told the House Committee on Foreign Affairs in 1974 that the minimum time required for Egypt to build

Nuclear Balance, Conventional Balance, and Political Settlement

Stabilization through regional nuclear deterrence is sometimes contrasted to two other approaches to stability that are said to be conceptually distinct: a negotiated *political settlement* or a "negative peace" based on containment by *conventional military forces.* However, it will be argued here that political settlement, conventional balance, and nuclear balance are not competitive approaches but in fact are synergistic. A stable balance of forces is not an *alternative* to a political settlement but more often a precondition. *Containment* leads to *settlement* when, after a long period during which potentially aggressive states are held at bay by their would-be victims, they eventually moderate their demands and accept a compromise. The Helsinki accords, for example, merely recorded the negative peace that had prevailed for 25 years in Central Eugope. They were based not on a perception in either camp that the final postwar lines of division were equitable or optimal, but simply on a universal belief on both sides that no change in the status quo could be achieved by force of arms at an acceptable cost. Moreover, the need for a stable military balance does not terminate when an agreement is signed. Where conflicting objectives are a permanent condition, as in Central Europe and the Middle East, the continuing viability of the negotiated settlement itself often depends on an environment of military stability.

Even if the need for a military balance is accepted, the relationship between reliance on conventional forces and resort to nuclear deterrence is controversial. Some have argued that nuclearization in the Middle East would be redundant (i.e., that there is a stable relationship of conventional forces), would introduce new instabilities, and would divert budgetary resources away from conventional arms. However, there are reasons to doubt that a balance can be maintained in the Arab-Israeli region similar to that between India and Pakistan, North and South Korea, Iraq and Iran, or China and Taiwan by conventional means alone. First, conventional balance depends critically on a number of independent variables, each of which is highly volatile, including (1) a finely tuned management of arms flow into the region from many independent sources of supply available unequally to the two sides; (2) harmonizing the impact of multiple technological innovations introduced at a rapid rate; (3) controlling for the shift and flux of regional alliances and interference by the great powers; and (4) managing the effects of highly unequal rates of economic development. A conventional balancer in a region of such fluid and revolutionary patterns has the problems of an acrobat on a high wire in a swirling windstorm: in principle it can be done, but only with supreme skill and exertion.

Moreover, the problem of creating a stable conventional military balance in the Middle East is compounded by the special nature of perceptions in this region. Abba Eban described the perceptual element from Israel's point of view succinctly in 1965, two wars ago: "We want to create doubt, and eventually resignation and despair [in the Arab mind] about the dream of eliminating Israel from the world's map."[33] But Israel has now decisively won four wars, the most recent under worst-case conditions, and yet the Arabs are far from "resignation and despair" and indeed *some* seem ready to contemplate a fifth, sixth, nth round, rolling indefinitely into the future.[34]

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nuclear weapons would be six to ten years after initiating a large scale civilian nuclear reactor program. See *U.S. Foreign Policy and the Export of Nuclear Technology to the Middle East*, pp. 182–83.

[33] *Jewish Observer and Middle East Review*, July 2, 1965. Quoted in Jabber, p. 104.

[34] For example, Lt. Col. Haytham Al-Ayubi, head of the military research department of the Palestine Research Center (Beirut): "The fifth round will not be the last round. There will be a sixth, seventh, eighth . . . until both sides finally realize that the only solution lies in setting up a [secular] democratic state. . . ." "Is War Inevitable?" translated from the Beirut weekly *Monday Morning* by Maha Samara in *Atlas*, January 1975, p. 36. Similar though less extreme views were expressed to the present author during visits with officials and scholars in Cairo and Damascus during October/November 1975 and February 1977.

The failure of the containment theory is explained by some Israelis in terms of a kind of Arab madness, an endless *jihad* in which there is no concept of ends-means rationality nor any calculation of the relationship between actions and their consequences.[35] Others explain it as an attempt to externalize the contradictions within Arab societies by demonizing Israel and maintaining a permanent state of hostility. But several "rational actor" explanations deserve attention before resort is made to socio- or psychopathological analysis. First, past defeats are not necessarily indicators of the future military balance. Unless one assumes a racialist theory of Arab inferiority, tiny Israel's past victories against the populous Arab world tend to be perceived as "unnatural" events to be explained by special circumstances. Heikal echoed a widely held view when he said that

> a military or political balance between the 100 million Arabs and the 3 million Israelis cannot be kept forever. Perhaps the Soviet Union and China–300 million to 800 million–can preserve a balance of a sort in spite of the disparity of numbers, but a ratio of 100 to 3 is absurd. With modern technology, the population gap becomes more significant.[36]

Arab observers explain Israel's past victories in terms of special factors in each case, such as the intervention of great powers in Israel's behalf (1956), Israel's success in catching the Arabs unprepared (1967), the failure of the Arab planners fully to comprehend and exploit their advantages at critical moments (October 6–9, 1973), and above all, the technological gap in equipment and manpower competence that has operated in Israel's favor since 1948. Most Arab strategists believe that, inevitably, the modernization of their countries will close the technological gap, and this will be reflected in a considerable alteration of combat loss ratios, relative manpower competence, and battle outcomes.

Moreover, Arab strategists can look to the example of other revolutionary movements which, after years of reverses and against seemingly hopeless odds, finally achieved their goals: Vietnam, Algeria, Angola, Mozambique, China, etc. In their own case, the revolution in oil prices might seem to signal the beginning of a process that is bound, over time, to alter the regional balance of power. OPEC oil revenues alone now exceed $100 billion per year, while Israel's GNP is only slightly in excess of $10 billion. Israel does not have the resources to continue the present rate of acceleration in military expenditures for long, while the Arab coalition, if it can maintain some degree of cohesion, could, in principle, double and redouble its military expenditures again. Israel is increasingly dependent on the United States, and Washington must weigh its interests in this small country against the strategic importance of the Arab world. Diplomatically, Israel is increasingly isolated and in a corner, and time does not seem to be on her side. It would not be unreasonable for an Arab strategist to conclude, from this ordering of the facts, that the wind of history is behind the Arab sail, and that the trends in the balance of power are bound to favor the Arabs if only they have the patience to let these processes mature.[37]

The final problem of the conventional balance of power as an approach to stability is its assumption that states only initiate wars when they expect to achieve military successes on the battlefield, and that they do not fight when they expect to lose. But the Arabs have shown in 1973 that the political result of a war need not be a direct reflection of the simple military outcome. The purpose of active warfare, in other words, may not be to achieve movement in combat, but in diplomacy. Moreover, a sophisticated understanding of the diplomatic environment can insure that the battlefield losses remain within tolerable limits. The military moves by both sides are restrained by the superpowers, and if the battle really begins to go badly, a cease-fire can be imposed. Thus, even against a highly unfavorable balance of conventional power, the Arabs may have a viable war option at an acceptable level of cost to achieve defined political objectives.

For all these reasons, it appears that *conventional* containment alone cannot bring stability to the Middle East, even if the balance of forces can be equilibrated during rapid technological, economic, and political changes. However, the addition of a nuclear level of deterrence has the potential to compensate for the deficiencies of the conventional balance of power. Israel in particular may be drawn to the nuclear option to put a halt to what might otherwise be an unending series of small wars. Ironically, the case from Israel's point of view has been put forth most eloquently by an Arab-American scholar, Fuad Jabber:

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[35] The issue of Arab rationality is taken up below.

[36] Heikal, *The Road to Ramadan,* p. 43.

[37] This perception of the future military balance is presented here to illustrate why some Arab strategists believe the military balance to be tipping. It is not meant to reflect the present author's assessment—indeed, I believe that in spite of the disparity of numbers, the conventional military balance is likely to remain strongly in Israel's favor well into the future. See Rosen, "The Arab-Israeli Military Balance in the 1980's," *Jerusalem Journal of International Relations,* 4, no. 1 (Fall 1978) forthcoming, and my forthcoming book, *Balance of Power: The Arab-Israeli Case.*

> Where conventional power has failed, weapons of mass destruction would be expected to succeed in convincing Arab populations first and their governments second of the futility of continuing their confrontation with Israel. With the realization that Israel cannot be militarily defeated, the rationale behind the permanent state of war, the economic blockade, and the policy of non-acceptance and non-recognition might be expected to break down. Moreover, whatever tendencies towards recognition and negotiations may already exist in the Arab world would be enormously strengthened. Hitherto, governments willing to negotiate had not dared to act because their position at home would have become untenable. In a nuclear context, the survival imperative might provide enough justification to make such approaches possible.... The psychologically erosive effects of the nuclear logic would be at work on the Arab will, gradually producing that pervasive feeling of "doubt and eventually resignation and despair about the dream of eliminating Israel from the world's map" that Israel's doctrine of deterrence had always sought to create.[38]

In this way, a nuclear deterrent could enhance the stability of the balance of power by creating the *perception* of balance that conventional arms alone cannot produce.

Arab Perceptions of Israeli Nuclear Weapons

Empirical evidence on Arab perceptions of Israeli nuclear weapons is fairly thin, but the few indications that are available support the theory of a strong deterrent effect. For example, as early as 1961, Iraqi Colonel Hussan Mustafa wrote in a Baghdad daily that "the atomic weapons would serve as a permanent deterrent to the Arabs and would compel them to avoid provocations." Moreover, "Israel's acquisition of atomic arms will decrease the value of the ordinary weapons in the hands of the Arab armies and the importance of their numerical superiority."[39] In early 1966, Ahmad Samih Khalidi, in "An Appraisal of the Arab-Israeli Military Balance," wrote that, when Israel gets the atomic bomb, there will be a "stalemate." Egypt will also get atomic weapons "and then neither country will dare attack the other" and "the sands will have run out for the Arabs."[40] Later in 1966, according to Abdullah Schleifer, the Syrian Left-Ba'athists called for immediate struggle against Israel because once Israel got the bomb, "none of the Arab armies, however strong by then, would even consider the idea of liberating Palestine at the risk of nuclear destruction...."[41]

Recent Arab commentaries carry forward the same theme. For example, the pro-Arab London monthly, *The Middle East*, in a 1977 assessment asking "Can the Arabs go to War?" concluded that "since [nuclear weapons] were developed [by Israel] at the end of the 1960s, there has never been the realistic possibility of a total Arab victory. The Arab states simply could not afford to win to the extent of overrunning the heavily populated heartland of Israel."[42] Arab military strategists do not seem to doubt that Israel would be driven to the nuclear "last resort" if the conventional shield were broken. For example, Haythem Al-Ayubi believes that

> Israel might use nuclear weapons if the Arab armies penetrate beyond the Green Line [i.e., the pre-1967 boundary] with a force capable of inflicting a military defeat on Israel. The Zionists think that if Arab forces drive past the Green Line, this will lead to the annihilation of the Jewish population....[43]

Similarly, Riad Ashkar, a well-known strategist at the Institute for Palestine Studies, believes that "if Israel finds herself ... unable to stop an Arab advance within the territory of Israel proper, and if it feels about to be destroyed, then Israel might think that the only way out for them is to resort to the use of nuclear weapons."[44]

These and similar statements support Jabber's view that a nuclear deterrent might succeed where conventional containment has failed, to being about the realization that neither the present balance of forces nor any foreseeable future power alignment will offer a viable military option to destroy Israel. The existence of nuclear weapons in the region will induce moderation and a revolution of declining expectations in the Arab "street," as the end-of-the-world character of atomic war is understood by both mass and elite elements within the Arab world. It is possible that nuclearization will reduce the political appeal of militant factions within the Arab world and that the so-called "rejection front" of regimes and organizations refusing to have anything to do with Israel will lose all credibility. This is a "trickle-down" theory—the new restraint will be evident first among the top leadership, but eventually the moderating effects of the danger of annihilation will percolate through the entire Arab polity. Also, the great powers, and notably the Soviet Union, will have new incentives to restrain their Middle Eastern clients.

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[38] Jabber, pp. 146–47 and 133.

[39] From a series of articles in January 1961 in the Baghdad daily *Al-Ahali* written by a retired colonel. Translated in Avraham Ben-Tzur, "The Arabs and the Israeli Atom Reactor," *New Outlook*, 4 (March-April 1961), 21.

[40] Ahmad Samih Khalidi, "An Appraisal of the Arab-Israeli Military Balance," *Middle East Forum*, 42, no. 3 (1966), 55, 63.

[41] See Abdullah Schleifer, *The Fall of Jerusalem* (New York: Monthly Review Press, 1972), 76.

[42] "Can the Arabs Go to War?" *The Middle East*, no. 29 (March 1977), 23.

[43] "Middle East: The Military Dimension," *Journal of Palestine Studies, No. 16*, 4, no. 4 (Summer 1975), 24.

[44] Ibid., p. 11.

While there is no doubt that Israel's highly concentrated population would be vulnerable to massive damage from even a few nuclear weapons, a few observers have raised the objection that the much larger number and dispersion of targets in the Arab countries might reduce the deterrent effectiveness of a small Israeli nuclear force. However, the population of Egypt is concentrated in the upper third of the country, including Cairo, Alexandria, and the towns of the Nile Delta; a third of the Syrians live in Damascus, Aleppo, Homs, Hama, and Latakia; and Jordanian society is centered in Amman. No city of the Arab world would be immune to long-range strikes by the Israeli Air Force. It seems reasonable to assume that an Israeli deterrent capable of hitting five Arab capital cities would be quite sufficient to constitute an "unacceptable" level of damage.

Rationality

Another objection that is often raised in discussions of the nuclearization of the Middle East is the assertion that the politics of this region tend to irrational and spectacular acts. The Arab-Israeli conflict is characterized at times by intense and uncompromising fears and emotions, expressed in extreme utterances and bitter images. The practical issues are overlaid by elements of religious war and transcendent moral imperatives that some claim would justify the sacrifice of millions. The Arab side in particular is widely believed to have a tendency to emotionalism and loss of self-control. One study of the Arab mind, for example, points to "the frequency with which the flood of anger, violence, or other intense emotion sweeps over the dam of self-control and in an astonishingly short time transforms the entire personality."[45] Poverty and frustration are so pervasive that there is a great deal of what Morroe Berger calls "free-floating hostility,"[46] and another observer finds that "conflict is so frequently on the verge of breaking out that personal relations seem to be directed at avoiding or covering up the slightest tendency toward the expression of difference."[47] Ciro Zoppo relates this directly to the problem of nuclearization:

> Should Egypt and Israel travel the nuclear road to military options, the slender reed of rational calculation may not sustain, in a nuclear confrontation ... the weight of the Arab quest for justice. Arab policies have in the past, on occasion, appeared suicidal.[48]

Alan Dowty asks, "Can [advocates of nuclear stabilization] explain Nasser's behavior in the 1967 crisis as an exercise in rational policy-making?"[49]

No strictly scientific and objective judgment is possible on the question of rationality, but there are some reasons to doubt the most alarming views. Nadav Safran has stated the case precisely:

> Israelis and Arabs, like all people, act on the basis of an interplay between emotional considerations and coercive pressures.... A given degree of emotional commitment sets a threshold that coercive pressures must reach in order to induce "rational," that is to say, calculated behavior.... In the Arab-Israeli conflict, the degree of emotional commitment has been high, indeed. But this has not meant that either side was emotionally "incapable" of yielding, but that efforts by one party to affect the behavior of the other necessitated the marshalling of vast means of pressure. Although individuals sometimes "crack up" or commit suicide rather than submit to the pressures of reality, in the case of nations this has seldom if ever happened.[50]

Past perceptions of the Arab states in particular may have been clouded by the climate of intense hostility and by the lack of normal intercourse with Israel and the West. Comparison with the case of China suggests that an adversary whose behavior once seemed irrational from a distance may look quite restrained after relations are improved and more objective and detailed information becomes available. Future studies of Arab policy may find, retrospectively, a consistent and rational pattern of behavior that was not perceived earlier. Nasser's 1967 miscalculation in Sinai may look no worse than Truman's misassessment of China's willingness to intervene in Korea, or Kennedy's groundless expectation that the Bay of Pigs incursion would trigger a popular uprising against Castro. Casual statements about "crazy states" may come to be seen as simplistic and even racist as applied to the Arabs, whose behavior seems to this observer to have exhibited the same care in assessing the costs and benefits of alternative policies as that of the great powers. And, as the risks of miscalculation escalate, greater restraint will be evident.

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[45] Raphael Pitai, *The Arab Mind* (New York: Charles Scribner's Sons, 1973), p. 160. See also Sania Hamady, *Temperament and Character of the Arabs* (New York: Twayne, 1960), pp. 205, 211, 222.

[46] Morroe Berger, *Conflict in the Middle East* (London: Allen and Unwin, 1971), quoted in John Laffin, *The Arab Mind Considered: A Need for Understanding* (New York: Taplinger, 1975), p. 108.

[47] Ibid.

[48] Ciro Zoppo, "The Nuclear Genie in the Middle East," *New Outlook*, February 1975, p. 24.

[49] Alan Dowty, "Nuclear Proliferation: The Israeli Case," *International Studies Quarterly* (forthcoming).

[50] Nadav Safran, *From War to War* (New York: Pegasus, 1969), p. xvi.

If rationality is not assumed, the prospect for the Middle East is dire with or without nuclear weapons. For example, the scarce water systems of the region depend on a highly developed network of reservoirs, pumping stations, and water carriers. Many elements in this fragile net would be vulnerable to conventional explosives. Still more vexing is the possibility of a modern equivalent of the classic tactic of the poisoning of wells, perhaps in the form of chemical or bacteriological warfare through the water supply. Small canisters of advanced toxic agents could be lobbed into reservoirs by a variety of means, and existing filtration systems would not be effective to protect the civilian populations. While each state has taken some precautions to defend its storage and delivery installations, there is no absolute defense in this sphere. Security depends on mutual offensive threats—i.e., on deterrent threats posed by each state against the other—a hydrological version of mutual assured destruction. The fact that rational interest has restrained total warfare in this field should give some encouragement as to the prospect of a rational nuclear balance.

If rationality is assumed, there are still quite a number of problems for the design of a stable system of nuclear deterrence in the Middle East, including: (1) securing second-strike forces against pre-emptive counterforce attacks; (2) controlling escalatory tendencies of tensions, incidents, and even subnuclear wars that may continue to occur; (3) preventing unauthorized use of nuclear weapons by overzealous subordinates; (4) preventing accidents; (5) preventing seizure of weapons by terrorist groups; and (6) controlling the disclosure process during which the introduction of atomic weapons into the region becomes publicly known, so as not to upset the delicate balance of forces in the area. Key problems in this list are taken up serially below.

Second-Strike Forces

The protection of a second-strike retaliatory force from destruction by an incoming counterforce surprise attack is a classic design problem for stable deterrence. The potential aggressor must believe that it will suffer retaliation at an unacceptable cost, and this means above all that some portion of the defending state's strike force must survive the initial assault. Of the many approaches to the problem of protecting the retaliatory force,[51] only a few are promising for the Middle East.

1. *Launch-Under-Attack.* Unprotected aircraft or missiles could be launched as soon as an incoming attack is detected. To avoid the possibility of catastrophic error, this would have to mean that unambiguous signals of an enemy attack have been received—in practice, that their nuclear-capable aircraft or missiles have crossed an explicit "red line." Given the extremely short flight times of Mach 2 bombers and surface-to-surface missiles in the Middle East—generally from 5 to 15 minutes on the most direct routes[52]—the timing would be such that an almost instantaneous decision would be necessary. This had led some, notably Ciro Zoppo, to conclude that a Middle East balance would be "one of the most unstable that one can imagine . . . a hair-trigger situation with strong urges to pre-empt."[53] Clearly, a stable balance of terror cannot depend *primarily* on a launch-under-attack system. However, as noted by John B. Walsh, Deputy Director, Strategic and Space Systems, DDR&E, launch-under-attack is

> a very sound augmentation to any other survivability scheme, and hence, I think we should

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[51] Warner Schilling has described nine ways of dealing with the fixed site ICBMs of the superpowers. See Schilling et al., *American Aims and a Changing Europe: Dilemmas of Deterrence and Disarmament* (New York: Columbia University Press, 1973), pp. 25–26.

[52] See Steven J. Rosen, *Military Geography and the Military Balance in the Arab-Israeli Conflict* (Jerusalem: Hebrew University, 1977), Jerusalem Papers on Peace Problems, no. 21.

[53] Zoppo, p. 24.

> never deny ourselves this capability. Moreover, we should always make it clear that such a capability is inherent in our systems and we are willing to exercise it. I think it is a strong deterrent capability in conjunction with other systems and can have some capability of its own.[54]

It is not inconceivable that this option will have a deterrent effect in the Middle East as well.

2. *Continuous Circulation.* This can take the form of either random occupancy of a large area on land, sea, or air or a crisis-oriented system in which the launch vehicles scatter on warning. This system faces the problem of avoiding detection by the increasingly sophisticated surveillance systems operated over the Middle East by the superpowers and, more recently, by the local states themselves. Israel now has the Hawkeye E-2C Airborne Early Warning System with an APS-125 overground radar which can track as many as 300 vehicles at a time out to a radius of 250 miles and can monitor intercepts of up to 30 targets, in a surveillance cylinder from the surface up to 100,000 feet.[55] Continuous circulation will not be a high-confidence survival mode for the Arabs in the future, though it may have some possibilities for Israel until the Arabs get a Hawkeye equivalent or real-time satellite data transmission from a superpower.[56]

3. *Hardened Silos and Aircraft Shelters.* The superpowers protect their land-based ICBMs in reinforced concrete silos hardened from 300 to 900 psi; Sweden keeps some of its Viggen aircraft in underground caverns carved out of rock in remote areas adjacent to highways suitable for takeoff (similar to the Swiss "underground aircraft carriers"); and Egypt has constructed over 450 reinforced concrete shelters up to 10 feet thick to protect its combat aircraft on the ground.[57] However, a ballistic or cruise missile or glide bomb with a circular error probability of less than 150 feet and a warhead of 20 kilotons would have a kill probability greater than 97 percent even against a target superhardened to withstand 1,000 psi overpressures.[58] And a wide array of precision guidance technologies are now becoming available with accuracies better than 150 feet, including laser designation, electro-optical, infrared, and radar seekers, radar area correlation, distance measuring equipment, microwave radiometers, and satellite position fixing.[59] With accuracies down to ten feet, it may be possible to destroy many hard targets even without nuclear warheads by using fuel air explosives.[60] While the most advanced precision guidance technologies have not yet been introduced into the Middle East, Israel has reported error distances of only 200 to 300 feet in test firings of its (nuclear-capable) Lance missiles;[61] it produces its own air-to-air (Shafrir), antishipping (Gabriel), and air-to-surface (Luz-1) missiles; and it is believed to have the electronics know-how to produce more sophisticated guidance packages in the future. While Egypt's past efforts in the missile field were not successful,[62] the new large-scale Arab military industries to be built in Egypt with Saudi aid will have a large element of French participation.[63] Moreover, European and American guidance packages may become available to the Arabs as off-the-shelf technology. Overall, terminally guided PGMs are quite likely to become part of the "normal" inventories of Middle

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[54] Testimony in Hearings before the Senate Armed Services Committee, March 19, 1976, *Fiscal Year 1977 Authorization for Military Procurement . . . , Part 2, Research and Development* (Washington, D.C.: Government Printing Office, 1976), p. 6257.

[55] "Airborne Early Warning for the U.S. Navy," *International Defense Review,* 8, no. 5 (October 1975), 679–82; and Philip J. Klass, "E-2C Radar to Provide New Flexibility," *Aviation Week and Space Technology,* July 12, 1976, p. 51.

[56] This discussion of options is an extensive reconsideration by the author of some of the arguments given in his "Nuclearization and Stability in the Middle East," in Onkar Marwah and Ann Schulz, eds., *Nuclear Proliferation and the Near-Nuclear Countries* (Cambridge, Mass.: Ballinger, 1975), pp. 170–71.

[57] On the Egyptian shelters, see General Salah El Dine Abdel Hamid Ali, General Abdel Sattar Megahed Arafa, and Colonel Salah El Dine M. El Dardery, "The Scientific Effect of the October 1973 War: Modification of Planning-Construction and Maintenance and Repair of Military Airports," paper delivered to the International Symposium on the October 1973 War, Cairo, October 1975; and Steven Rosen and Martin Indyk, "The Temptation to Pre-Empt in a Fifth Arab-Israeli War," *Orbis,* 20 (Summer 1976), 279–80.

[58] Based on calculations from Kosta Tsipis, *Offensive Missiles,* Stockholm Papers no. 5 (Stockholm: Stockholm International Peace Research Institute, 1974) and other sources; see Rosen, "Nuclearization. . . ," pp. 166–68.

[59] See Richard Burt, *New Weapons Technologies: Debate and Directions,* Adelphi Paper no. 126 (London: International Institute for Strategic Studies, 1976), esp. p. 3.

[60] See Steven Rosen and Desmond Ball, "Fuel Air Explosives for Medium Powers," *Pacific Defence Reporter,* April 1977; and Rosen and Indyk, p. 280.

[61] "Israel Test-fires Lance," *Flight International,* May 15, 1976, p. 1312.

[62] Egypt's efforts were described in Lewis A. Frank, "Nasser's Missile Program," *Orbis,* 11 (Fall 1967), 746–57.

[63] "France Will Help Egypt Build Her Arms Industry," *Egyptian Gazette,* April 5, 1976, p. 1.

Eastern powers within a decade, and they will render obsolete the protection of second-strike forces in hardened sites whose location is known. Hardening will be a viable survivability option only in the near-term.[64]

4. *Protective Concealment and Dispersal.* One of the most promising approaches to survivability in the Middle East, particularly for Arab states with large open areas, is concealing the location of weapons-bearing aircraft and missiles (possibly including the use of camouflage and decoys). The blast overpressure resistance of an unhardened missile on a gantry is estimated at 2 psi; a combat aircraft on the ground is disabled at 3 to 5 psi. The maximum overpressures generated by a 20-kiloton warhead detonated at optimum burst height would drop below 2 psi at distances greater than 2.3 miles from ground zero; that of a 1-megaton weapon at 7 miles. If we assume a random occupancy pattern for second-strike missiles and aircraft on the ground, and that the incoming first-strike force consists of fewer than 50 warheads, the aggressor would have little chance of achieving a disarming counterforce first-strike provided it did not know which vehicles carried the nuclear strike forces and provided that the area of dispersion was large. Moreover, it would not know if additional second-strike forces were already airborne or at sea at the time of its attack.

A caveat should be added here, on the additional problems of concealment if the adversary possesses the means to detect and track the fissile material in the warheads during peacetime. The unclassified literature on surveillance and verification techniques generally indicates that peacetime detection of warhead movements depends on photographic, infrared, radar, and radio wave means of detection which could be evaded, and that neutron, gamma-ray, and X-ray sensors, which would be more difficult to counter, are effective only after the warheads are detonated.[65] However, a few published and unpublished sources speculate that ultrasensitive surveillance equipment may be able to detect the tiny amounts of radiation that normally escape the shielding of atomic weapons in storage.[66] If this is so, a survivability system depending on concealment and dispersal may require proliferated decoys emitting low-intensity radioactivity with signatures comparable to those of atomic warheads, to saturate the enemy target acquisition system with fakes. For states able to develop atomic weapons, the fabrication of such devices should be relatively simple. Overall, concealment and dispersal is the most promising low-cost alternative.

5. *Land-mobile Multiple Aim Point Shelter System.* The survivability system for land-based ICBMs that the United States seems to be moving to adopt when improvements in Soviet delivery vehicle accuracy render existing silos obsolete is a system in which missiles mounted on transporter-launchers "would move from one relatively hard shelter to another within a complex." It is a deception game, essentially a pea-and-shell game, in which "the attacker would have to target all of the shelters" within

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[64] However, medium-range surface-to-surface missile systems *presently* available in the Middle East in 1977 do not possess counterforce-capable CEPs. Israel's Jericho (range 280–300 miles) is said to have guidance problems (Insight Team, p. 283) and has been rated as accurate within one kilometer (3280 feet). See *Le Monde*, April 25, 1968; and Jabber, *Israel and Nuclear Weapons*, p. 96. The Arabs' Scud-B (range 180–200 miles) is reported in one study as having an error probability of 1300 feet, and the unguided Frog-7 is known to be inferior to the Scud. Robert E. Harkavy, "The Strategic and Diplomatic Implications of the Israeli Nuclear Weapons Program" (Mimeo, Kalamazoo College, Michigan, 1974), p. 39. A later draft of this paper is to appear as *Israel's Nuclear Weapons: Spectre of Holocaust in the Middle East.* Monograph Series in World Affairs (Denver: University of Denver Press, 1977). The Pershing (range 460 miles) promised to Israel during the Sinai II negotiations (but not delivered) has accuracy problems so severe that a Pershing II with radar area correlation guidance is reported to be under development to correct this deficiency (*Jane's All the World's Weapons Systems 1976,* p. 42.) The Soviet Scaleboard SS-12 (range 400–500 miles), reported in the *Middle East Economic Digest* of June 11, 1976, to be under delivery to Syria, appears to derive from the SS-13 and SS-14, which are regarded as low accuracy missiles. See Doug Richardson, "Soviet Strategic Nuclear Rockets Guide," *Flight International,* December 11, 1976, p. 1732. Of presently available systems, the Lance is the most accurate but has a limited range (50–75 miles) and a CEP well in excess of 150 feet.

[65] See Herbert Scoville, Jr., "A Leap Forward in Verification," in Mason Willrich and John Rhinelander, eds., *SALT, The Moscow Agreements, and Beyond* (New York: Free Press, 1974), pp. 160–82; and U.S. Arms Control and Disarmament Agency Publication 85, *Verification: The Critical Element of Arms Control* (Washington, D.C.: Government Printing Office, 1976).

[66] See John W. R. Taylor and David Monday, *Spies in the Sky* (New York: Scribner's Sons, 1972), p. 117; and Desmond J. Ball and Edwin Coleman, "Alternative Missile Basing: Analysis of a Land-Mobile ICBM System," *Survival,* 19, no. 4 (July/August 1977). Shlomo Aronson has called attention to reports that American monitoring stations were able to detect the nuclear "radiating freight" of Soviet ships headed for Alexandria during the October War, picking up the "low radiation" by "highly sensitive, secret American equipment" (privately circulated communication).

every complex, "since he would not know in which shelters the missiles were [actually] deployed."[67] With a limited number of attack missiles, they could not hit all the shelters. As described by Desmond Ball and Edwin Coleman in their "Tootle" version, the system consists of a series of semi-hardened shelters (about 125 psi) in concentric circles linked by roads to a central station where the missile transporter is based. The shelters are so spaced that no two of them can be taken out by a single enemy warhead, and the distances are such that the missile can be moved from the central station to a randomly selected shelter when warning of the enemy attack is received and before the incoming warheads impact. It is a time-urgent system, but one without the catastrophic error potential of the launch-on-attack system. The critical questions for adaptation of this system to the Middle East are the shorter warning times and the land requirements and cost of the system. Here, the assumed warning time is as little as five minutes (for the sites closest to the enemy border) but the missile system and its transporter are lighter (relatively short range rather than intercontinental missiles) and therefore can be moved more rapidly and developed at lower cost. If we assume a five-minute warning time, 200 kiloton warheads on the attacking missiles, and the development of a 40 mph transporter, a ten-shelter "Tootle" could be built in a circle with a diameter of six miles. Israel could space a very large number of such systems (possibly including dummies) in the vast undeveloped areas of Negev and northern Sinai, and the areas available to Egypt, Syria, and Jordan are many times as large. Ball and Coleman estimate the cost of a U.S. 1,000 ICBM, 10,000 shelter system at $15 to $30 billion, including research and development, shelters, ICBMs, transporter/launchers, roads, and command and control installations. If we make the simplifying assumption that the Middle East unit cost would be identical (in fact, it would probably be lower with smaller missiles and shelters, lighter launchers, and shorter and less heavily surfaced roads), a 25 missile, 250 shelter land-mobile multiple aim point shelter system in this area would cost approximately $375 to $750 million. By way of comparison, Israel's first 25 F-15s cost $675 million, though like the shelter system their cost can be amortized over a decade or more. In summary, a "Tootle" solution is another viable (albeit more expensive) option for the Middle East.[68]

6. *Other Survivability Options.* The above five options do not exhaust the possibilities. Other concepts examined by American strategists include off road mobile, inland waterway, rail mobile, hard capsule, revetted rail, pool, pool/canal, canal, covered trench, buried trench, hard rock tunnel, and soil tunnel,[69] in addition to various submarines, surface ships, and air platforms. The purpose here has been to indicate the range of options rather than to provide an exhaustive analysis of each possibility. But the examples developed should be sufficient to establish that the technical problems of a stable second-strike system of mutual assured destruction are solvable in the Middle East.[70]

Controlling Escalation of Crises

Another design problem for a stable balance of terror in a region of continuing hostility will be to raise the nuclear "firebreak" as high as possible should fighting break out and to control the escalatory tendencies of subnuclear crises. Low-intensity threats, such as border incidents, commando raids, fedayeen infiltration, and Israeli reprisals would not be likely to cross the nuclear threshold, but full-scale conventional wars in the Sinai, Golan, and the West Bank would raise incalculable dangers of uncontrolled escalation. If Israel remains in control of a substantial part of these territories, it is not inconceivable that the Arab states might mount an October-scale assault in defiance of the Israeli nuclear deterrent, on the reasoning that Israel's nuclear weapons are usable only as a last-resort to guarantee the ultimate survival of the state and not for such secondary objectives as holding Hebron, Sharm el-Sheikh, or Mount Hermon. If so, strategic nuclear weapons in themselves would not halt the endless round of border wars in the peripheral areas. Indeed, the more stable the relationship of strategic nuclear deterrence, the wider the scope for subnuclear provocations without fear of escalation.

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[67] See Secretary James Schlesinger, *Annual Defense Department Report FY 1976 and FY 1977* (Washington, D.C.: Government Printing Office, 1975), pp. II–28.

[68] See Ball and Coleman. The present analysis assumes that the movement of the transporter from its station to the randomly selected shelter will not be tracked by enemy radars or infrared or electro-optical sensors. The use of smoke, decoys, or other protective measures might be required.

[69] See testimony of Colonel Vigil W. Munsey in *Hearings Before the Senate Armed Services Committee, Fiscal Year 1975 Authorization for Military Procurement . . . April 2, 1974 Part 6* (Washington, D.C.: Government Printing Office, 1974), p. 3347.

[70] Fuad Jabber points out that, even without sophisticated second-strike defenses, no first-strike could be counted on to destroy *all* the nuclear weapons available to the other side and, given the geographic concentration of Middle East populations, even one or two remaining warheads would cause what may be considered unacceptable damage to the initiator of a nuclear exchange. Jabber, *Israel's Nuclear Option and U.S. Arms Control Policies,* California Seminar on Arms Control and Foreign Policy research paper no. 9 (Los Angeles: Crescent Publications, 1972), p. 34; and Jabber, *Israel and Nuclear Weapons,* Chapter 11.

Nor would Israeli evacuation of all the territories held since 1967, as advocated in many settlement proposals, eliminate the problem entirely. Most of these proposals anticipate that the relinquished territories would remain demilitarized areas or limited forces zones, so that they would not subsequently become the launching points for future Arab attacks against Israel. It is entirely conceivable, however, that sometime after the agreements establishing such a regime, successor Arab governments in Egypt, Syria, and Jordan, or a PLO-originated government in the West Bank, would choose to reintroduce large-scale forces into the demilitarized areas, whether to resume the war against Israel, or in defense against perceived Israeli threats, or merely to assert the prerogatives of sovereignty over territory. In such a case, Israel might feel compelled to initiate a conventional preventive war in the territories, to stop the buildup of Arab forces in areas proximate to the Israeli heartland and to liberate Israel from what might be seen as another "stranglehold of aggression."

Large-scale conventional wars in the 1967 territories could challenge the restraining codes of nuclear conduct that are essential to a balance of terror. If, for example, an Egyptian force broke through the Sinai passes and hill chains and reached as far as the Abu Aweigila triangle of northeastern Sinai, Israel would not know whether this unrestricted advance of Arab armies flushed with victory would stop at the Green Line or penetrate into the Jewish state itself. Similarly, an Israeli advance into the Damascus plains past the Sassa lava ridges or toward the recapture of Hebron might induce the Arab states to consider the nuclear option. Any scenario involving full-scale mobilization or conventional war must assume that there would be a tense nuclear alert, with unprecedented dangers of miscalculation. *The world has not yet seen a full-scale conventional war involving directly opposed armies of two nuclear-armed parties.*

The introduction of *tactical* nuclear weapons could alter this picture substantially, both in relation to the correlation of forces in the affected theatres and in relation to the extension of atomic deterrence from the heartlands to the territories. Usable theatre nuclear weapons (TNW) would have to be much smaller than the present TNW in Europe (which range up to 400 kilotons). In this tiny area, where distances are measured on a doll-house scale (with the exception of Sinai), battlefield use of multikiloton weapons might be self-deterred by fear of collateral damage to one's own population and army. However, subkiloton mini-nuclear weapons could be developed with yields as low as .01 kiloton (artillery-delivered nuclear projectiles in Europe are as small as .1 kiloton), for use against a wide variety of battlefield targets.[71] Larger interdiction weapons could also be used, possibly in the form of atomic demolition munitions to destroy bridges, cave in tunnels and defiles, cut roads, and otherwise create barriers to slow enemy movement or induce concentrations of its forces. Battlefield nuclear weapons could be used to defend a border[72] or to arrest the movement of an adversary who had achieved theatrewide conventional preponderance or a blitzkrieg breakthrough in an area of local superiority.[73]

TNW can also be a signaling device to convey the message that a major threshold on the scale of violence has been entered, reminding the adversary of the linkage between the forces in the field and the strategic forces in reserve.[74] They add a potentially useful rung on the ladder of escalatory responses. If we infer from the reports cited earlier that Israel is likely to enjoy an atomic monopoly for approximately ten years,[75] but that during this period the

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[71] Though it should be noted that a mini-charge requires just as much nuclear explosive as a charge of greater force, in order to achieve critical mass. The low force is obtained by designing in a low degree of efficiency, and apparently requires substantial knowledge of nuclear weapons technology. See Anders Thunborg, *Evolution of Doctrines and the Economics of Defense* (Stockholm: Swedish Institute, 1973), Appendix: "What are mini-nuclear weapons," pp. 60–62.

[72] John F. Scott, *Nuclear Strategy for Defending a Border* (U.S. Army War College: Strategic Studies Institute, Military Issues Research Memorandum ACN 75042, 1975).

[73] James R. Schlesinger, *The Theatre Nuclear Force Posture in Europe: A Report to the United States Congress in Compliance with Public Law 93–365* (Washington, D.C.: Department of Defense, 1975) released July 1, 1975, pp. 1–2, 12–16.

[74] Colin S. Gray, "Theatre Nuclear Weapons: Doctrines and Postures," *World Politics,* 28 (January 1976), 300–14. Also, Colonel Stanley D. Fair, *The Changing Roles for Theatre Nuclear Forces* (U.S. Army War College Strategic Studies Institute, Military Issues Research Memorandum ACN 76044, 1976).

[75] See footnote 32.

Arabs will use their oil wealth and influence to accumulate ever-larger inventories of conventional weapons, an Israel fully committed to nuclearization might use this period to develop TNW to enhance its war-fighting capabilities within the secondary theatres and, still more important, to create in the minds of its adversaries an escalation linkage between combat in the territories and strategic nuclear weapons. This would increase the risks and costs of subnuclear wars for the Arabs, and it might extend the effect of Israel's atomic deterrent to have at least some application in the West Bank, Sinai, and Golan. Israel might well be attracted to develop capabilities that would discourage Arab efforts to nibble at the edges in contentious areas.

However, we are assuming for the present analysis that eventually the Arabs will match whatever nuclear capability Israel might have including the tactical atomic level. If a war should occur in the territories at a time when *both* sides possess strategic *and* tactical weapons, the potential for escalation will be greater yet. Presumably, it will be of mutual interest to avoid the use of TNW, and if they are used, to see that an intrawar limiting process continues to operate to protect civilian population sanctuaries. But historical analogies suggest that rational interest alone can fail to arrest the escalation from limited to total war, even when there is a highly developed process of signaling and tacit bargaining. There are bound to be some factions who will call for TNW use to limit casualties, to turn the tide of battle, to bring a more decisive victory, to end the war earlier, or to break the morale of the enemy. The advocates of escalation will play upon the belief that the other side eventually is found to escalate anyway, or conversely, that it has only a limited ability to respond. Accidental violations of limiting principles by the enemy may be misread as intentional acts signaling the end of restraint. As the distance between "allowed" and "disallowed" weapons and targets narrows, as both sides try to bring within their "disallowed" sanctuaries an increasing number of targets regarded as legitimate military objectives by the enemy, and as the "fog of war" thickens, the boundaries between permissible and impermissible actions will be eroded. There may be a sequence of small escalatory steps whose end result the leaders are unable to foresee, but which will lead incrementally to unlimited warfare. And, in the heat of battle, restraint may fail under the pressure of anger demanding expression. Once the hounds of war are unleashed, the decision-makers may be overwhelmed by the social forces they have activated.[76] No one could be certain that a full-scale war in the territories would not bring out tactical and eventually strategic nuclear weapons.

This principle of *uncertainty*—as to the conditions under which nuclear weapons might be used, the numbers, kinds, and locations of atomic weapons available to the parties, and the problem of controlling escalation of subnuclear wars—is at once the most *dangerous* element of a Middle Eastern mini-balance of terror, and yet the most *restraining* influence. Nothing so clarifies the mind of a statesman as the knowledge that his miscalculation tonight could lead to annihilation in the morning. Strategies of flexible response, graduated deterrence, and calculated escalation in Europe seem to have contributed to an environment of such generalized uncertainty that even low-level confrontations are eschewed for the incalculable destabilizing effects that they might have. It is possible that TNW could have a similar restraining influence in the Middle East. With or without TNW, the probability of full-scale conventional offensives in a nuclear Middle East would be quite low.

Accidents, Unauthorized Use, and Terrorism

If intentional use of nuclear weapons can be controlled, a stable system of deterrence also depends on preventing accidental detonations, controlling unauthorized use of nuclear weapons by subordinates, and preventing the seizure and use of atomic devices by terrorist organizations. The United States and the Soviet Union use sophisticated organizational arrangements and technological devices to guard against these dangers. In the American case, as described by Lyndon Johnson:

> the release of nuclear weapons would come by Presidential decision alone. Complex codes and electronic devices prevent any unauthorized action. Every further step along the way from decision to destruction is governed by the two-man rule. Two or more men must act independently and must decide the order has been given. . . . An elaborate system of checks and counter checks, procedural and mechanical, guard against any unauthorized nuclear bursts. In addition, since 1961 we have placed permissive-action links on several of our weapons.

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[76] On the failure of efforts to limit strategic bombing of cities in Europe during World War II, see Frederick M. Sallagar, *The Road to Total War* (New York: Van Nostrand Reinhold, 1969).

> These are electromechanical locks which must be opened by secret combination before action at all is possible, and we are extending this system.... We have taken every step man can devise to insure that neither a madman nor a malfunction could ever trigger a nuclear war.[77]

James Schlesinger described another set of more recent technological devices that add to overall weapons security by preventing the use of a nuclear weapon "in the most unlikely event that one is captured," including "improved internal protective hardware which would selectively disable the weapon either permanently or temporarily."[78]

Once a state comes into possession of nuclear weapons, it is in the interest of the entire international community (including and perhaps especially its adversaries) that it also possess the means to manage them safely.[79] Yet the most sophisticated permissive action links, disabling devices, and other security systems may be beyond the technological or economic means of small powers to develop for themselves. If this proves to be the case, it may be in the higher interest of the great powers, however opposed they may be to proliferation itself, to pass along weapons security technologies that will reduce the chance of accidents and unauthorized detonations as well as disable weapons that fall into the hands of terrorists or fanatical insurgent groups.

It should be noted that the possibility of theft of a *complete nuclear device* from a national arsenal, which appears to be a manageable security problem, is not the only means by which terrorist organizations or insurgent movements could come into possession of atomic weapons. Recent studies of terrorism focus on the prospect of the theft of *nuclear fuels* from various points within the reactor fuel cycle, and then the use of various methods of processing to enrich these materials to fissionable grade.[80] However, these scenarios are not dependent on the existence of nuclear *weapons* in the region, but rather focus primarily on the security problems of *civilian* nuclear power programs. The security or insecurity of civilian nuclear activities will exist whether or not atomic weapons programs are initiated, and therefore the main terrorist threats are a separate issue from the design of a stable nuclear balance of terror.

Disclosure

Another problem in the design of a system of mutual deterrence is the potentially traumatic transitional period during which the existence of atomic weapons is disclosed and before the new relationships settle into routine patterns. The degree of instability caused by the transition to nuclear weapons will depend partly on the circumstances of their introduction and disclosure. If disclosure is incremental, and takes the form of a series of rumors, followed by reliable reports, followed by official pronouncements or demonstrations, with adequate time periods between stages, there may be a process of "weaning" or assimilation during which the parties will have time to adjust to the new idea without undue alarm. Conversely, if atomic weapons possession were suddenly disclosed at the height of a crisis, the possibilities of precipitous action and miscalculation would be heightened and the deterrent effect might be lost.

Several Israeli authors have suggested that a policy of calculated *ambiguity* is the optimizing choice of Israel, whether or not Israel has gone ahead with nuclear weapons development.[81] George Quester sees this as a kind of "psychological warfare," and he foresees the day when Arab civilian programs will lead to offsetting "rumors of Egyptian bombs emerging periodically to match rumors of Israeli bombs."[82] Quester views this as a negative trend, but to the advocate of nuclearization as a path to stability, it is a necessary and constructive process to cushion the shock.

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[77] Address by President Johnson at Seattle, "The Direction and Control of Nuclear Weapons," September 16, 1964, in U.S. Arms Control and Disarmament Agency, *Documents on Disarmament 1964* (Washington, D.C.: Government Printing Office, 1965), p. 431.

[78] Schlesinger, pp. 24–25.

[79] See Joel Larus, *Nuclear Weapons, Safety, and the Common Defense* (Columbus: Ohio State University Press, 1967).

[80] See Willrich and Taylor; also Augustus R. Norton, "Nuclear Terrorism and the Middle East," U.S. Army Command, *Military Review*, 56 (April 1976), 3–4; and Brian Jenkins, *Will Terrorists Go Nuclear?*, California Seminar on Arms Control and Foreign Policy, discussion paper no. 64 (Los Angeles: Crescent Publications, 1975).

[81] See Yair Evron, "Israel and the Atom: The Uses and Misuses of Ambiguity, 1957–1967," *Orbis*, 27 (Winter 1974), 1342, 1332, 1333; Avigdor Haselkorn, "Israel: From an Option to a Bomb in the Basement?" in Robert M. Lawrence and Joel Larus, eds., *Nuclear Proliferation: Phase II* (Lawrence: University of Kansas Press, 1974), p. 165; and Dowty, forthcoming.

[82] Testimony in *U.S. Foreign Policy and the Export of Nuclear Technology to the Middle East*, p. 189; and George Quester, "Can Proliferation Now Be Stopped?" *Foreign Affairs*, 53 (October 1974), 83.

From Israel's point of view, perhaps the greatest hazard that might be associated with open disclosure would be the reaction of the United States. The profound antipathy of American policy makers toward proliferation might lead to punitive sanctions in the form of a reduction of economic aid or a restriction in the further supply of conventional arms.[83] The Arms Export Control Act of 1976, passed by the 94th Congress and signed into law by President Ford, prohibits economic and military assistance to any country which receives nuclear reprocessing or enrichment equipment, technology, or materials from any other country without thorough provisions for inspection and oversight, unless the president determines that termination of assistance would have "a serious adverse effect on vital United States interests" *and* unless the president "has received reliable assurances that the country in question will not acquire or develop nuclear weapons."[84] It is not clear whether Israel would be in technical violation of this law if it received no further uninspected assistance from abroad (though it would appear that the conditions under which France assisted Israel in the construction of the Dimona reactor in 1958–1962 would constitute a violation if Section 305 applies retroactively). But even if Israel is not in technical violation of existing legislation or policy, an open policy of nuclearization would almost surely disrupt the already fragile relationship with Washington.

If Washington alone restricted the flow of arms and aid—whether on grounds of Israeli self-sufficiency or as a punitive sanction—and the Arabs continued to receive uninterrupted supplies, the effect of nuclearization would be to upset the conventional arms balance which would continue to be of primary importance even after nuclearization. However, the United States might be inclined to adopt the opposite posture and to keep Israel well supplied with conventional weapons to raise the nuclear "firebreak" and reduce the probability that Israel would feel compelled to resort to nuclear response during a conventional war.[85] Perhaps there will be a period of trauma involving a temporary interruption of aid, after which behavior will return to the normal pattern. However, it is clear from past behavior that any restriction in the flow of American arms of however brief duration will be regarded as a serious handicap in Jerusalem, and this will be a major disincentive to develop nuclear weapons or to disclose them if they exist. On the other side, the possibility that Israel might be driven to nuclearization will act as a restraint on the United States in the degree of pressure it applies against Jerusalem during the hard bargaining ahead on broader diplomatic issues, so the nuclearization threat cuts both ways.

The United States' antipathy to an explicit Israeli disclosure of nuclear weapons possession might be less effective as a deterrent to disclosure if one or more of the following conditions exists at the time: (1) Proliferation becomes epidemic on a world scale, and particularly if there is clear evidence of atomic weapons development or acquisition by one or more Arab states; (2) Washington perceives a significant, large-scale alteration of the conventional military balance against Israel to be taking place but is unable or unwilling for political or technical reasons to provide the huge quantities of countervailing arms required; (3) Continued published rumors of Israeli nuclear weapons lead to a situation in which the world becomes inured to the idea, so that official disclosure is reduced to a fairly minor step; (4) The ambient Arab threat to Israel intensifies considerably, and Washington no longer believes itself to have the leverage to exercise a moderating influence on the Arabs nor does it feel it can expect the Israeli government to resist the pressure of alarmed public opinion inside the country; (5) The Arabs move to the Soviet camp en masse, and the polarization of the region generates a new willingness in the United States to support a hard line; (6) America has already made excessive use of negative sanctions against a wide variety of other allies for various reasons, and it faces a general loss of credibility as an ally if it continues coercing friends; (7) Washington is already applying severe sanctions against Jerusalem to force concessions on other issues, so that the incremental cost in American displeasure following nuclearization is reduced to a tolerable level (indeed, America might have to choose between using its leverage for diplomatic goals or to halt proliferation, not being able to accomplish both); (8) One or more Western allies in other regions have recently fallen to Soviet-sponsored attacks, and the general mood in the United States shifts to a harder line. It is entirely possible that one or more of these conditions will materialize, and that the problem of American reaction to Israeli disclosure will be less severe at the time than one would assume today.

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[83] See Fuad Jabber, "Not By War Alone: Curbing the Arab-Israeli Arms Race," *Middle East Journal,* 28 (Summer 1974), 240; also Aubrey Hodes, "Implications of Israel's Nuclear Capability," *Wiener Library Bulletin,* 22 (August 1968), 4.

[84] P.L. 94–329, "Arms Export Control Act," Section 305, "Nuclear Transfers." See *Congressional Quarterly Almanac 1976,* p. 215.

[85] See Richard Burt, "Proliferation and the Spread of New Conventional Weapons Technology," *International Security,* 1 (Winter 1977), 119–39. Secretary Kissinger declared himself in opposition to "overly severe constraints which would seriously set back, rather than advance, our nonproliferation efforts" including those which would "cast further doubt on the credibility of U.S. supply commitments and the constancy of our policy at precisely the moment when we can least afford such doubts." Statement on U.S. nonproliferation strategy before the Senate Committee on Government Operations, March 9, 1976, reprinted in *Department of State Bulletin,* 74 (March 29, 1976), 410. On the other hand, Senator Symington has been a vigorous advocate of strong measures. See also Donald Brennan et al., *The Implications of Precision Weapons for American Strategic Interests* (Harmon-on-Hudson, N.Y.: Hudson Institute, 1975), Report 2264-RR, pp. 25–33.

Preventive War

Another danger in the disclosure process that has impressed some observers is the possibility of *preventive war* by one side to arrest the atomic development of the adversary. President Nasser warned in 1960 that, if it were certain that Israel was constructing a bomb, "it would mean the beginning of war between us and Israel, because we cannot permit Israel to manufacture an atomic bomb. It is inevitable that we should attack the base of aggression."[86] Major General Ariel Sharon, a leading Israeli hawk, echoed this theme in 1975: "If we discover that the Egyptians are working on a nuclear weapon of their own . . . we'll have no choice but to wipe them out."[87] Others have discussed the possibility of a Soviet disarming attack.[88] But these prophecies of "surgical strikes" to excise an adversary's "nuclear cancer" have a long and dubious history. Harrison Salisbury and others adumbrated Soviet strikes against the Chinese atomic facilities in Lanchow, Paotow, and Lop Nor;[89] some "revisionist" historians of the origins of the Cold War have claimed that several key U.S. military officials, reportedly including Secretary of the Navy Francis P. Matthews and Commandant of the Air War College General Orville Anderson, favored preventive war against the Soviet Union in the late 1940s in order to smash its nascent atomic technology;[90] and a popular American magazine found it necessary to warn readers in 1948 against the simplistic idea of "using the atomic bomb against the Russians before they crack the formula and use it against us."[91] These scenarios of disarming surgical strikes seem to excite the imagination of the general public and some alarmed scholars, but they do not seem to have the same credibility for responsible commanders-in-chief.

In the Middle East, one scenario that has attracted wide interest is the prospect of a Soviet strike against Israel; but Moscow would have to consider the possibility of an American response and even Israeli counterthreats to the Baku industrial region and the major cities of south-central Russia, including Odessa and Tbilis, or to the Black Sea/Mediterranean fleets.[92] The arresting idea of direct Israeli deterrence cannot ignore the fact that Soviet strategic air defense is by far the most massive and extensive in the world, consisting of more than 5,000 radars, 2,600 fighter-interceptors, and almost 12,000 surface-to-air missiles. However, its low altitude intercept capability is limited[93]—though it is being corrected to a degree by the introduction of the Su-15 Flagon-E—and a desperate Israel might overfly weakly defended areas at treetop levels to execute a limited strike on southern Georgian cities. Even a small possibility that unrestricted escalation could result in destruction of a

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[86] Quoted in *Jewish Observer and Middle East Review,* December 23, 1960; cited in William B. Bader, *The United States and the Spread of Nuclear Weapons* (New York: Pegasus, 1968), p. 96.

[87] Interview in *New York Times,* February 8, 1975.

[88] For example see Haselkorn, pp. 168–69.

[89] Harrison Salisbury, *War Between Russia and China* (New York: Bantam Books, 1970), pp. 126–27. The United States reportedly expressed its concern about this possibility to the Soviet Union. See John Newhouse, *Cold Dawn* (New York: Holt, Rinehart, & Winston, 1973), pp. 164–65.

[90] See David Horowitz, *From Yalta to Vietnam* (London: Penguin, 1967), p. 263; and I. F. Stone, *The Hidden History of the Korean War* (New York: Monthly Review Press, 1952), pp. 92–94. Preventive war options were assessed officially by the National Security Council in 1950 and, according to all reports, rejected categorically on philosophical, political, and military grounds. See NSC-68, *A Report to the National Security Council by the Executive Secretary, on United States Objectives and Programs for Security* (Washington, D.C.: Government Printing Office, April 14, 1950), pp. 11, 53, and 37.

[91] "Is War Inevitable?" *Woman's Home Companion,* 75 (March 1948), 43, excerpted in Peter G. Filene, *American Views of Soviet Russia, 1917–1965* (Homewood, Ill.: Dorsey Press, 1968), p. 235.

[92] See Haselkorn, p. 165 ff; and J. Bowyer Bell, "Israel's Nuclear Option," *Middle East Journal,* 26 (Autumn 1972), 386–87.

[93] "Soviet Air Defenses," *Air Force Magazine,* March 1976.

Soviet city could be expected to act as a moderating influence on Russian policy.[94]

Conclusion

Overall, the problems of preventive war, accidents, second-strike force survivability, escalation, and unauthorized use and theft of weapons appear to be manageable in the Middle Eastern context. The design of a stable regional system of deterrence will require imagination in adapting the techniques and conventions of superpower atomic diplomacy to a quite different context. But it would be quite unjustified to assume that the introduction of nuclear weapons in the Arab-Israeli conflict inevitably will lead to catastrophe.

On the other hand, even if it is accepted that nuclear weapons may make a contribution to the stabilization of the Arab-Israeli conflict, they are not a panacea for all the problems of the region. They will not resolve the deeply rooted conflict of historical claims and national interests between the Arabs and the Israelis. They will not resolve the issues of the final disposition of the 1967 territories—indeed, the outcome to these questions will depend more on political negotiations than on *any* balance of military forces whether or not nuclear weapons are introduced in the Middle East. They will not solve the problem of the Palestinian Arabs, nor will they prevent guerrilla actions. They will not bring status and prestige to their owners—indeed, in Israel's case they may reinforce its image as a pariah state. And, contrary to the major argument of one proponent of Israeli nuclearization, they will not decisively reduce the dependence of Israel and other regional states on outside aid from America and other sponsors.[95]

The one thing that strategic and tactical nuclear weapons may do, and this at considerable risk and expense, is to stabilize the military balance in the region in a manner that conventional arms alone cannot achieve, and thereby create an essential precondition for a durable political settlement.

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[94] The classic expression of the theory of finite deterrence is Pierre Gallois, *The Balance of Terror* (Boston: Houghton Mifflin, 1961), pp. 1–14. See also Geoffrey Kemp, *Nuclear Forces for Medium Powers: Part I: Targets and Weapons Systems,* Adelphi Paper no. 106 (London: International Institute for Strategic Studies, 1974).

[95] Robert W. Tucker advocates an open Israeli nuclear deterrent primarily to reduce the dangerous dependence of the Jewish state on American aid and to enable it to depend to the maximum extent possible on its own resources. See his "Israel and the United States: From Dependence to Nuclear Weapons?" *Commentary,* 60 (November 1975), 29–43. This proposal rests on the highly arguable supposition that conventional military expenditures could be reduced substantially after nuclearization. Moreover, it implies that the sole cause of Israel's dependence is military expenditures, which is demonstrably not the case. Israel's post-1973 reliance on vastly increased American aid is accountable primarily to the widening of her balance of trade deficits from an annual average of approximately $1 billion before the war to the $4 billion range following it. However, less than half (46 percent) of the increase in the balance of payments deficit between 1972 and 1975 is accountable to defense imports, the remainder representing the consumer goods import surplus, worsening terms of trade, and other non-defense-related factors. Israel's civilian sector historically imports over one-third more than it exports. The post-1973 global inflation of prices therefore automatically widened the absolute deficit, even though export growth rates have exceeded civilian import growth in recent years. Had there been no *defense* imports or exports at all, Israel's balance of payments deficit without U.S. aid and other donations would still have exceeded $2.2 billion in 1975. See "Balance of Payments" in *Bank Leumi Economic Review,* no. 85 (October 1976), pp. 2–5. Israel's total imports in 1975 were $7,430 million, of which defense accounted for approximately $1,500 million. See David Kochav, "Economics of Defense: Israel," in *Military Aspects of the Arab-Israeli Conflict* (Tel Aviv: University Publishing Projects, 1975), p. 181. Conclusion: if defense imports were reduced to zero, Israel's financing requirement for U.S. aid would be reduced by about half, but would still approximate $1 billion per year, and the U.S. might not be willing to provide any economic assistance to a nuclear Israel. And of course it is quite unrealistic to assume that conventional defense imports would be discontinued, if indeed they would be reduced at all. A similar case could be made regarding the effects of nuclearization on Egypt's aid requirements, most of which also derive from civilian rather than military demands.

Party Systems and Government Stability in the Indian States*

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This paper contributes to the substantive and methodological discussion of the issues concerning the causes of cabinet instability through analysis of data from Indian state politics. The focus of the analysis is on explaining the duration of Indian state governments in days with variables measuring the degree of fragmentation and cohesion in the party system, the composition of the cabinet, the characteristics of the opposition, and the role of ideological differences. A substantial amount of the variation in the durability of coalition governments is explained with variables that measure the degree of party system institutionalization and the extent of political opportunism, but ideological factors do not explain much of the differences in durability of governments. It is also found that none of the measures used can explain much of the variation in one-party majority governments for which, it is argued, explanations must be sought that focus on leadership skill and on relationships between leaders and factions in a dominant party.

Introduction

The study of the causes of government or, more properly, cabinet instability in political science has a long and hallowed history. Michael Taylor and V. M. Herman in their 1971 article on "Party Systems and Government Stability" used contemporary political science methodology to test hypotheses developed on this question by A. Lawrence Lowell 80 years ago.[1] During the past 80 years, both the methodology for analyzing the causes of cabinet stability and instability and the range of countries and governments brought under analysis have changed dramatically, but the theoretical questions have remained largely unchanged. Whereas Lowell concentrated his attention on detailed descriptive accounts of five political systems, contemporary political scientists tend to present accumulations of data on the governments of the 19 "Western" parliamentary democracies, sometimes enlarged slightly by the inclusion of Israel, Japan, and/or the United States.[2] And, whereas Lowell was largely content to present his arguments in the form of axioms supported by the evidence from his descriptive accounts, contemporary political scientists have an array of measures, indexes, and statistical techniques to choose from to test their hypotheses. Even when contemporary political scientists concentrate their attention on a single political system, of which Fourth Republic France has received the lion's share, the methods of analysis nowadays tend to involve sophisticated statistical manipulations of large masses of data.[3]

Although recent methodological advances have produced some refinements in the ability of political scientists to measure more precisely the structure of legislatures and the composition of governments, answers supplied to the basic questions concerning the effects on government stability of the structure of the legislative party system, the composition of the cabinet, the characteristics of the opposition, and the role of ideological differences have so far fallen short of a general theory of cabinet

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[1] Michael Taylor and V. M. Herman, "Party Systems and Government Stability," *American Political Science Review*, 65 (March 1971), 28–37; A. Lawrence Lowell, *Governments and Parties in Continental Europe*, Vol. 1 (Cambridge, Mass.: Harvard University Press, 1896), pp. 70–74.

[2] See esp. Taylor and Herman; Jean Blondel, "Party Systems and Patterns of Government in Western Democracies," *Canadian Journal of Political Science*, 1 (June 1968), 180–203; and Lawrence C. Dodd, "Party Coalitions in Multiparty Parliaments: A Game-Theoretic Analysis," *American Political Science Review*, 68 (September 1974), 1093–1117.

[3] The methodologically most sophisticated analysis of cabinet instability in Fourth Republic France is, of course, that done by Duncan MacRae, Jr., *Parliament, Parties, and Society in France: 1946–1958* (New York: St. Martin's Press, 1967).

stability. Taylor and Herman found that most of Lowell's arguments were confirmed by the contemporary evidence, namely, that government instability was associated with fragmented multiparty systems and with coalition governments. They also found that governmental instability in the 19 parliamentary systems they studied was particularly associated with the size of the antisystem (principally the Communist) parties and the fragmentation of the prosystem parties, but otherwise that ideological differences among political parties were not of great significance in explaining cabinet instability.[4] Lawrence Dodd, in a recent analysis of data from 17 European and Commonwealth party systems, has modified Lowell's and Taylor and Herman's arguments somewhat by specifying the conditions under which some multiparty systems may produce durable coalition cabinets and has attempted to incorporate his findings into a general theory of government formation and persistence. His argument in brief is that durable coalition cabinets tend to be minimum winning coalitions and that such coalitions are most likely to form in multiparty parliamentary systems in which the degree of party system fractionalization is moderate and the party system relatively stable. Dodd's set of variables, like Taylor and Herman's, explains only approximately 25 to 30 percent of the variance in duration of the governments analyzed.[5] However, Dodd's treatment of coalition systems separately from one-party systems and his focus on the influence of party system stability in analyzing the causes of cabinet stability are desirable innovations consistent with the approach adopted in this article.[6]

My purpose in writing this article is to contribute to both the substantive and methodological discussion of these issues through analysis of data from India, a country not included in any of the broadly comparative studies so far carried out by contemporary political scientists. In doing so, I have three general points to make. The first is that political scientists ought not to be satisfied with inconclusive answers on questions concerning the causes of government stability and instability and with correlations which explain no more than 25 percent of the variance. When we are dealing with only 100 or 200 cases of governments, whose every moves have been documented in the press and whose participants often can still be interviewed, we should not be satisfied with correlations that leave most of the variance unexplained. To improve our explanatory capacities, however, political scientists will not be able to rely on only one methodological approach, but will have to combine quantitative analysis and case studies, the former to identify the evident patterns and regularities and to pinpoint the interesting anomalous cases and the latter to explain the anomalies through in-depth analysis of particular cases.

In social science, it is of little value to push aside unexplained variance in hopes that the all-comprehensive theory or the ultimate methodological tool will eventually be discovered. Therefore, my second point is that in analyzing a problem such as the causes of cabinet instability, political scientists ought not to do only "normal science," but must always seek explanations for the anomalous cases as well, by combining methodological approaches. Wherever quantitative analysis leaves a substantial portion of the variance in a dependent variable unexplained, it is incumbent on the researcher to propose an alternative research strategy for explaining the remaining variance through either further quantitative analysis of new kinds of data or through case studies that explore the anomalous cases in particular.

In this article, quantitative analysis of legislative and electoral variables provide an explanation for approximately 40 percent of the variance in the duration of coalition governments, but it proves to be much less useful in explaining the duration of one-party governments. Consequently, some examples, particularly anomalous cases, are extracted from the data file for examination at the end of this article, where the kinds of additional data and case studies that seem most appropriate to carry the explanation further are suggested.

My third point is that the range of observations in the study of cabinet government is still not broad enough, having been confined so far only to the Western and Commonwealth industrialized countries and Japan and, within those countries, only to the central cabinets. If we are looking for a general theory of cabinet instability, the data base ought to be extended to include contemporary governments outside the circle of the 19 or 20 industrialized democracies. In this article, the range of observations will be extended to include postindependence state governments in India.

India as a Research Site. The extension to

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[4] Taylor and Herman, pp. 34–37.

[5] Dodd, pp. 1111–12.

[6] However, the approach taken in this article derives exclusively from the Lowell, Blondel, Taylor-Herman tradition rather than from the game theory tradition which Dodd combines with the former in his study.

India of research on cabinet stability and instability adds not just one additional government to the comparison, but, by including observations on the provincial level, a whole new universe of data comparable in breadth and diversity to the entire range of cases previously brought into view in the study of Western parliamentary systems. The collection and interpretation of data on Indian state party systems and governments also offers the potential for opening the study of Indian politics to comparative methods of analysis in a manner similar to the development of the study of comparative state politics in the United States after V. O. Key began his systematic survey of American state politics and party systems.[7] And, just as Key pointed out that the description of the United States as a two-party system masked a remarkable degree of diversity of party systems at the state level, the conventional view of India as a one-party dominant system has concealed an even wider range of types at the provincial level. In broad descriptive terms, the range has included multiparty systems with one party dominant, dualized competitive systems (multiparty systems in which the principal conflict is between the two largest parties), segmented pluralist systems in which several institutionalized parties command significant political support in a sociocultural segment of society,[8] and fragmented multiparty systems in which no party has a strong and stable base of support in the electorate. The knowledge available so far on such systems in the Indian states has come nearly exclusively from descriptive analyses and case studies of particular states done by regional specialists.[9] Very little truly comparative work has been done on party systems and government stability in the Indian states and still less work has been done which sets out to advance theory on this subject by the explicit testing of carefully specified hypotheses.[10]

The data to be presented and analyzed below were drawn both from existing provincial units in India and from previously existing units that have been merged into or divided up among the present 21 states as a consequence of India's prolonged process of internal reorganization. The basis for including a provincial unit in the data base for this study was whether or not it had had experience with popularly elected legislatures and cabinet governments. There were 39 provinces in India that had such experience in the period between the First General Election of 1952 and March 27, 1974, the cutoff date for this study, and there were 127 governments in those units during that period. The data base clearly is large enough for the kinds of statistical analyses to be used in this article.

Throughout this article, the dependent variable whose variation is to be explained is the duration of Indian state governments in days. A government has been defined for purposes of this study as any administration whose chief minister has been sworn in by the governor of a state and who remains in office without an interelection resignation. In other words, a government that undergoes a formal resignation and a new swearing-in ceremony after a general election, but under the same chief minister, will be counted as one government. However, in a situation where a chief minister resigns between elections and either reforms the government with a new swearing-in ceremony or is replaced by another chief minister, the two administrations in such cases will be counted as two governments. This definition is not the same as that used both by Taylor and Herman and by Blondel, but it approximates their definition in practice while taking into account the constitutional and political features of Indian state politics. Their definition, which includes, in addition to continuity of leadership, continuity of party support in the legislature,[11] could not be used in India because it is not always clear which parties in fact support the government; this is a determination that is frequently made by the governor of the state rather than by a formal test in the legislature.

The descriptive-analytical portion of this article is divided into five sections. In the first part, some of the hypotheses drawn from the

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[7] V. O. Key, Jr., *Southern Politics in State and Nation* (New York: Knopf, 1949) and *American State Politics: An Introduction* (New York: Knopf, 1956).

[8] The term "segmented pluralism" comes from Val R. Lorwin, "Segmented Pluralism: Ideological Cleavages and Political Cohesion in the Smaller European Democracies," *Comparative Politics,* 3 (January 1971), 141–75.

[9] Two important collections of such studies are in Myron Weiner, ed., *State Politics in India* (Princeton, N.J.: Princeton University Press, 1968), and Myron Weiner and John O. Field, eds., *Electoral Politics in the Indian States* in four volumes, of which see esp. Vol. 4, *Party Systems and Cleavages* (Delhi: Manohar Book Service, 1975).

[10] However, see Paul R. Brass, "Political Participation, Institutionalization, and Stability in India," *Government and Opposition,* 4 (Winter 1969), 23–53.

[11] Taylor and Herman, p. 29, and Blondel, p. 190.

Taylor and Herman study have been tested with the replicated Indian data. It is demonstrated here that these hypotheses, though confirmed, do not have much explanatory power in the Indian case particularly when Indian state governments are broken up into different types. Different explanations are required for coalition than for one-party majority governments and for majority and minority governments. Consequently, these different types of governments are analyzed separately in the three sections that follow. In these three analytical sections, the principal independent variables are various measures (some derived from other studies, others used here for the first time) of governmental, legislative, and party system fragmentation, cohesion, and institutionalization.[12] In addition, the data are used to test hypotheses concerning the impact of "antisystem" opposition and of ideology and opportunism in determining the relative endurance of governments. In the final analytical section, brief consideration is given to the broader question of the conditions that produce different types of party systems and to the relationship between theories of party system development and theories of cabinet stability.

Legislature and Government

The Legislative Party System. Taylor and Herman used two measures of fragmentation of the parliamentary party system—the number of parties holding seats in parliament (N_p) and Rae's index of parliamentary fractionalization (F_p). They found that government stability (measured in terms of duration of governments in days) correlated negatively with both measures in the 19 parliamentary systems they studied. Their correlations were $r = -.39$ and $R^2 = .152$ for N_p with government stability and $r = -.448$ and $R^2 = .201$ for F_p with government stability, so that N_p accounted for 15 percent and F_p for 20 percent of the variation in governmental stability, respectively.[13] In addition to these two measures, a third measure, the percentage of seats held by the largest party in the legislature (P_p), has been used here with the Indian data.[14] Although P_p is highly intercorrelated with F_p ($r = -.948$) and is, more or less, its obverse, it seemed appropriate to use P_p also because of the importance of one-party dominant systems in Indian state politics. The hypothesis tested with this measure is that government stability is correlated positively with P_p—the reasoning being that the higher the P_p the more likely it is that one party will be able to form a majority government; in addition it is known that one-party majority governments on the average tend to persist longer than coalition or minority governments.

The two hypotheses derived from the Taylor and Herman studies and the third just presented were tested for the Indian state governments, using the data on individual governments for computation of the product-moment correlations. The Pearson correlation coefficients for the three variables with government stability are as follows: $-.272$ with N_p, $-.333$ with F_p, and $.371$ with P_p. Thus, the negative correlations found between government stability and N_p and F_p with the data from the 19 parliamentary systems are confirmed with the data from Indian state governments, but the correlations are smaller and the amount of variation explained by these variables in the Indian states is only 7.39 and 11.08 percent, respectively. F_p explains slightly less of the variance in the Indian data than the more simple measure of percent seats held by the largest party in the legislature, the R^2 for P_p with government stability being .1378 (13.78 percent).

The Role of "Antisystem" Opposition. Taylor and Herman's leading finding was that cabinet stability correlated negatively with the percentage of seats held in the legislature by antisystem parties, principally the Communist parties.[15] This finding conforms well with what is known about the difficulties posed for cabinet construction and endurance in the two most unstable systems in Europe after World War II, France and Italy, where the Communist parties were excluded from government. India has two strong Communist parties, whose strength varies from state to state, but their "antisystem" character has not been as pronounced or as much feared as in Europe because they have both acted within the rules of the game of Indian politics, and they have not always been excluded from alliances with other parties or from participation in coalition governments.[16] However, because there are some parties in Indian politics whose leaders prefer to avoid coalitions with one or the other or both of the Communist parties, a correlation was run for government duration and the combined Communist party seat shares (CP) in the Indian state legislative assemblies. Once again, the Taylor and Herman hypothesis was supported, but at a much lower level, the correlation coefficient being only –.157 compared to –.450 in their study.

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[12] As in the Taylor and Herman study, all legislative structure variables are held constant during the entire period of a legislature between elections even though more than one government may form and terminate in the life of a single legislature and even though conditions inside the legislature itself may change between elections. In the case of governments that endure through more than one election, the value assigned to each independent variable is "a weighted average of its values in the periods before and after the election, the weights corresponding to the lengths of those periods." Taylor and Herman, p. 29.

[13] Ibid., pp. 30–31.

[14] All three of these variables were first used systematically as measures of different types of party systems by Douglas W. Rae, *The Political Consequences of Electoral Laws* (New Haven: Yale University Press, 1967), Ch. 3.

[15] Taylor and Herman, pp. 35–36.

The Composition of the Government. Taylor and Herman looked for the sources of cabinet instability in the structure of the cabinet as well as in the degree of fragmentation in the legislative party system. They found that one-party governments were more stable than coalition governments, that majority governments were more stable than minority governments, and that government stability correlated negatively both with the number of parties in the cabinet and with governmental fragmentation. All these relationships are confirmed by the Indian data.

Most noteworthy are the relationships shown in Tables 1 and 2 between the average duration of Indian state governments in days and the type of government. The difference between the mean duration in days for one-party governments compared with coalition governments is substantial and significant at the .001 level. (The *t*-statistic is 4.71 with *df* of 125, and the *r*, using dummy variables, is .388.) The difference between the mean duration of majority and minority governments is less substantial but still considerable, with a *t*-statistic of 3.75, which also is significant at the .001 level. The principal line of demarcation, however, is clearly between one-party governments (particularly one-party majority governments) and coalition governments (particularly minority coalition governments). Moreover, this distinction is even sharper with the Indian data than it is with the data from the Western parliamentary systems. Indian one-party governments last longer on the average than Western parliamentary systems (1187.5 days for Indian governments compared to 1107.9 days for Western governments), but Indian coalition governments are far more short-lived than their Western counterparts (378.3 days for Indian governments, 624.5 days for Western governments).[17]

The importance of the distinction between one-party governments and coalition governments is demonstrated further in Table 2, where the data on average duration of Indian state governments are broken up into four duration categories and then cross-tabulated with type of government. All governments that lasted longer than five years in India were

Table 1. Mean Duration of Indian State Governments in Days by Type of Government

Type of Government	(N = 127)[a]	Mean Duration[b]	Standard Deviation	Range
One-party majority	(76)	1307.9	1022.2	12–4685
One-party minority	(16)	615.3	546.2	19–1750
All one party	(92)	1187.5	990.0	12–4685
Majority coalition	(22)	495.4	400.2	12–1405
Minority coalition	(13)	180.1	139.2	7– 411
All coalition	(35)	378.3	360.1	12–1537
All majority	(98)	1125.5	979.1	12–4685
All minority	(29)	420.2	465.5	7–1750
All governments	(127)	964.5	935.2	7–4685

[a] Some of the categories overlap. Consequently, the totals do not add up to 127. However, the Ns for rows 1, 2, 4, and 5, for 3 and 6, and for 7 and 8, respectively, form discrete subsets of the data that do add up to 127.

[b] All the means are unweighted averages.

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[16] It can be argued that the West European Communist parties, despite their revolutionary rhetoric, have also operated in terms of the rules of the parliamentary game and that they have been kept out of governments because of the real or alleged fears of their opponents concerning the dangers of Communist participation. Whatever the true explanation of the European situation, my point here is that the Indian Communist parties have played the parliamentary game *and* have not been totally excluded as coalition partners in state governments.

[17] Taylor and Herman, p. 31.

one-party majority governments. Moreover, there is a monotonic decline in the proportion of one-party majority governments as duration in days declines from one category to another. The relationship is precisely opposite for coalition governments. No coalition government lasted longer than five years, only two lasted longer than three years, and the proportion of coalition governments to other types increases as the category of duration in days declines. Of the governments that lasted less than one year, 56.4 percent were coalition governments and 74.3 percent were either coalition governments or one-party minority governments.

These data clearly establish some evident regularities and general patterns in the data of which the most prominent are the demonstration that cabinet instability is associated with particular types of government and that coalition governments are the most unstable type of government in the Indian states. However, the demonstration of these regularities constitutes only the beginning of the inquiry. The data in Table 1 showing the standard deviation and the range in duration for different types of governments suggest the extent of the problems remaining to be resolved. The standard deviations for all types of governments are high and the range within each type is very broad. What remains to be explained, therefore, are the reasons for the differences in the duration of the different types of governments and for the considerable range in duration within types.

While it is commonly known that one-party governments tend to endure longer than coalition governments, there is far from unanimous agreement in the theoretical literature on why this pattern should hold. Two types of rival explanations have been prominent. One is that coalition governments tend to arise in political systems where the society is divided by multiple ideological and/or ethnic cleavages which so fragment the support of parties in the electorate that coalition governments are necessary, but also cannot be sustained for very long because of the irreconcilability of the ideological differences among coalition partners. The second type of explanation also assumes that multiparty systems arise in political-social orders divided by multiple cleavages, but argues that it is not the ideological differences among the principal parties in a multiparty system that matter so much as the opportunities given to the nonideological parties, groups, and individuals to exploit the differences among the parties by bargaining for ministerial positions with the highest bidders from competing coalitions. In more homogeneous societies where political culture and social structure sustain either two-party or one-party dominant systems, it is commonly assumed that neither ideological nor pragmatic problems prevent the formation of stable governments.

The same set of issues arises in interpreting the relative stability of governments within types as between types. Sartori's distinction among moderate, polarized, and atomized multiparty systems,[18] for example, has been modified by Dodd, who argues that the type of

Table 2. Cross-Tabulation of Duration of Indian State Governments in Days with Type of Government

	Duration in Days							
	1826+ (N = 17)		1001–1825 (N = 28)		366–1000 (N = 43)		1–365 (N = 39)	
Type of Government[a]	N	%	N	%	N	%	N	%
One-party majority	17	100.0	23	82.1	26	60.5	10	25.6
One-party minority	0	0.0	3	10.7	6	14.0	7	17.9
All one party	17	100.0	26	92.9	32	74.4	17	43.6
Majority coalition	0	0.0	2	7.1	9	20.9	11	28.2
Minority coalition	0	0.0	0	0.0	2	4.7	11	28.2
All coalition	0	0.0	2	7.1	11	25.6	22	56.4
Majority	17	100.0	25	89.3	35	81.4	21	53.8
Minority	0	0.0	3	10.7	8	18.6	18	46.2

[a] As in Table 1, the categories overlap (see footnote a to Table 1). All percentages are based on the total *N* for each column. The percentages for rows 1, 2, 4, and 5, for rows 3 and 6, and for rows 7 and 8, respectively, add to 100.00 in each column.

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[18] Giovanni Sartori, "European Political Parties: The Case of Polarized Pluralism," in *Political Parties and Political Development,* ed. Joseph La Palombara and Myron Weiner (Princeton, N.J.: Princeton University Press, 1966), pp. 137–76, and "The Typology of Party Systems–Proposals for Improvement," in *Mass Politics: Studies in Political Sociology,* ed. Erik Allardt and Stein Rokkan (New York: Free Press, 1970), pp. 322–52.

cabinet coalition formed in such systems—"oversized," "undersized," or minimum winning coalition—is influenced by the degree of "party system fractionalization, instability and cleavage conflict" and that the durability of the cabinet coalition in turn depends upon its "coalitional status," minimum winning coalitions being the most durable. This explanation takes account of ideological factors, measured in the "cleavage conflict" variable, but places as much emphasis on structural factors in the party system as determinants of coalitional status and more emphasis on structural than ideological differences in the cabinet coalition as determinants of its durability. For, in contrast to Sartori who emphasizes the instability and immobilism of "polarized pluralism," Dodd argues that stable pluralist systems tend to produce minimum winning coalitions, hence more durable coalitions, irrespective of whether or not the party system is "depolarized" or "polarized" ideologically.[19]

These alternative explanations, one emphasizing structural and the other ideological factors, will be explored below insofar as possible with the available data. It will be argued that in the Indian cases structural factors, such as the relative dominance of larger and more cohesive political parties in the legislature and the degree of party system institutionalization, have been more important conditions for stable government than the presence or absence of ideological differences between parties. However, it must be conceded at the outset that the way in which the data were distributed provided more satisfactory measures of structural than of ideological variables.

Coalition Governments

Three types of independent variables were used with the Indian data in an attempt to explain the variation in the stability of the 35 coalition governments. One set of variables measured different aspects of the structure and composition of the legislature, a second set measured aspects of the structure and character of the parties and persons in the government, and a third (comprising only one variable) measured the degree of institutionalization of the party system in the electorate. No attempt was made in this article to relate government stability or instability to conditions external to the government, legislature, or party system. An analysis of external conditions such as public demonstrations and agitations, riots, and civil disorders would be of relevance and importance, but is better handled through case studies than through the kinds of quantitative methods used here. For purposes of this article, I am concerned principally with ascertaining how much of the variance in the stability of different types of governments can be explained by using measures on which comparable data can fairly easily be obtained or generated for all the Indian states, and which facilitate comparisons with the results obtained in other studies of cabinet instability.

The Structure and Composition of the Legislature. In the previous section, it was shown that government duration correlated positively with P_p and negatively with N_p, F_p, and the combined proportion of seats in the legislature held by the Communist parties (CP). However, these correlations all change markedly when done for only the coalition governments, as shown in Table 3. The negative association between F_p and duration practically disappears, that between P_p and duration is similarly reduced and changes sign, the correlation for CP is reduced and changes sign, and only the correlation for N_p increases and retains its sign and significance level. A comparison of the correlations for these variables for one-party and coalition governments indicates clearly that P_p and F_p are not explanatory variables at all for either type of government, but are merely descriptive indicators of the structure of the legislature. The positive correlation of .371 for P_p and the negative correlation of $-.333$ for F_p with duration for all governments merely indicated that one-party governments are in general more stable than coalition governments, but the absence of any association between P_p or F_p and duration for one-party or coalition governments considered separately suggests that the proportion of seats held by the governing party and the degree of fragmentation in the legislature are not critical factors in explaining duration for either form of government. Interestingly enough, however, N_p becomes the strongest explanatory variable (of the legislative structure variables) in relation to coalition governments, a fact that requires explanation since, on the face of it, N_p appears to be only a less precise measure of fractionalization than F_p and is intercorrelated with F_p at .606. Nevertheless, it is fairly easy to understand why N_p may, in situations involving coalition formation, be a more important variable than F_p. For example, an F_p of .74 or .75 may reflect a relatively even division of four political parties in a legislature, each holding approximately 25 percent of the seats, or it may reflect a seven-party division, with one party holding 45 percent of the seats, another 15 percent, three others 10 percent, and two others 5 percent. In the second case, there are more coalition possibilities, and a majority coalition without the leading party would be more complex and, therefore, less likely to be durable than any possible coalition that could be formed out of the first situation. Thus, the different correlations for duration and N_p with one-party majority and with coalition governments suggest that the number of parties in the legislature does not matter when one party is dominant, but does matter in multiparty systems where no single party has an overwhelming majority of seats in the legislature.

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[19] Dodd, p. 1110.

One other correlation in Table 3 that requires explanation is the positive correlation between duration and the Communist party seat share for coalition governments. Previously, Taylor and Herman's association of instability with a high Communist seat share was confirmed, though weakly, for all state governments in India. It now appears, however, that the Communist party vote, which is intercorrelated with F_p at .457 and with P_p at −.490, is merely one contribution to multipartyism and fragmentation in the legislature. In other words, a high Communist party representation in the legislature is associated with greater multipartyism, which in turn increases the likelihood that coalition governments will be required. However, the combined Communist seat share in the legislature has no effect on the stability of one-party governments and very little effect on the duration of coalition governments. There are two reasons why Communist representation may affect the durability of coalition governments in India quite differently from those in

Table 3. Correlation Coefficients for Duration with Variables Measuring the Structure of the Legislature, the Composition of the Government, and Party System Institutionalization in the Indian States

	Type of Government			
Variable[a]	All (N = 110, 113, 126, 127)	One-Party Majority (N = 67, 76)	Coalition (N = 18, 32, 34, 35)	One-Party Minority (N = 14, 15, 16)
F_p	−.333*	−.010	−.038	−.575*
N_p	−.272*	.096	−.381*	−.548*
CP	−.157*	.010	.074	.085
P_p	.371*	.012	−.007	.433*
TPR	.208*	.148	NC[b]	−.093
W_p	NC	NC	.051	.553*
F_o	NC	NC	NC	−.430*
N_{pG}[c]	−.219*	NC	−.221	NC
F_G	−.360*	NC	−.219	NC
IC	.205*	NC	.237	NC
IDG	.290*	NC	.277*	NC
MINMAJCO	.318*	NC	.428*	NC
CONGNON	.195*	NC	.264	−.163
PSI	.370*	.228*	.534*	.408*

*Significance level of .05 or better.

[a] F_p is Rae's index of parliamentary fractionalization; N_p is the number of parties in the legislature at the time of election; CP is the combined seat share of the Communist parties in the legislature; P_p is the seat share of the largest party in the legislature; TPR is the ratio of seats held by the governing party to those held by the next largest party; W_p is the seat share of the two largest parties; F_o is the fractionalization of seat shares among opposition parties only, using Rae's formula; N_{pG} is the number of parties in the government; F_G is the fractionalization of party representation in the government; IC is a dummy variable for ideologically polarized and nonpolarized governments; IDG is a dummy variable for presence or absence of independents or defectors in the government; MINMAJCO is a dummy variable for majority and minority governments; CONGNON is a dummy variable for Congress and non-Congress governments; PSI is a measure of party system institutionalization defined in the text of this article.

Rae's formula for F_p is: $F_p = 1 - (\sum_{i=1} T_i^2)$ where T_i is any party's decimal share of seats in the legislature.

[b] NC = not calculated.

[c] The correlations reported in this row exclude governments that contained independents or defectors because of the ambiguities involved in deciding whether or not to classify each independent and/or defector as one party or certain groupings of independents and/or defectors as one party. Moreover, excluding such governments from this variable avoids overlapping with variable IDG.

Europe. The first reason has already been noted, namely, that, in contrast to the situation in Europe, Communist parties often have been acceptable coalition partners in the Indian states. Consequently, a large Communist party presence does not, as in Europe, *ipso facto* reduce the coalition possibilities. Secondly, the Communist parties in India have been more effective than most parties in maintaining internal cohesion and discipline in their legislative parties and in preventing defections to other parties. The importance of indiscipline and defections in general in explaining government instability in India will be explored presently. As far as the Communist parties are concerned, however, their internal cohesion means that once the party decides either to participate in or to support a coalition government, its support can be counted upon and is not subject to sudden change because of individual or group defections.

In general, this analysis of the impact of four variables measuring the structure of the legislature on the duration of coalition governments indicates clearly the critical importance of treating one-party and coalition governments entirely separately. Correlations done for the two types of governments combined mask the fact that associations between an independent variable with duration for all governments may reflect only the differences between the two types. A second point that is suggested by this analysis is the uniqueness of the European experience of Communist involvement in the parliamentary system. In India, in contrast to Europe, Communist strength in the legislature is correlated positively with the stability of coalition governments. Third, the multiple correlation for three of the four variables used so far (N_p, CP, and P_p, eliminating F_p because of its high intercorrelation with P_p) is .416, with an R^2 of .173. Thus, the combined effect of the variables used here measuring the structure of the legislature accounts for only 17 percent of the variance in the stability of coalition governments. Clearly, we must look to other factors to explain further the varying duration of coalition governments in the Indian states.

The Composition of the Government. Six variables were selected to measure various aspects of the composition and character of coalition governments and to test several hypotheses concerning the relationship between government composition and duration. The first variable concerned the majority or minority status of the government. It is evident from Tables 1 and 2 that minority coalition governments were the most unstable form of government in the Indian states in the period under review and that the mean duration and range of such governments was considerably smaller than the corresponding figures for majority coalition governments. There are two evident reasons why minority coalition governments should be expected to be less durable than majority coalition governments. If the minority government has been formed because ideological disagreements prevent possible coalition partners from joining together, it is likely that controversial issues will soon arise that will precipitate a successful no-confidence vote. If a minority coalition government has been formed because the demands for ministerial payoffs by other potential coalition partners are too high, then it is likely that an alternative coalition will form before long around the disaffected groups. The correlation coefficient for minority and majority coalition governments (MINMAJCO) with duration, using dummy variables, is .428 and the R^2 is .183. Thus, the dichotomous distinction between minority and majority coalition governments explains over 18 percent of the variance in duration.

Two variables used by Taylor and Herman to measure the internal structure of the government—the number of parties in the government (N_{pG}) and governmental fractionalization (F_G)—also were used with the Indian data to test the hypotheses and their findings that duration is negatively correlated with both variables. The correlations reported by Taylor and Herman were $r = -.307$ for N_{pG} with duration and $r = -.302$ for F_G with duration. However, these correlations were for all governments, including both one-party and coalition governments and probably reflect in large part the difference between the two types. The more interesting question is whether or not and to what extent the stability of coalition governments is associated with the number of parties in the government and the degree of governmental fractionalization. The correlation coefficients for duration of Indian coalition governments with N_{pG} and F_G respectively, are $-.221$ and $-.219$. These correlations hold up also for majority coalition governments treated separately, for which the coefficients are $-.247$ for duration with N_{pG} and $-.299$ for duration with F_G. All the correlations are in the expected direction.

In an earlier descriptive article on coalition governments in the north Indian states, two other variables were considered in explaining the instability of cabinet coalitions in those states, namely, the extent of ideological differences among the parties in the government and the degree of internal coherence within the parties. It was argued then that, although the cabinet coalitions in north India were ideologically diverse, a lack of coherence within parties was a more important cause of the fall of those governments than interparty ideological differences. Instability threatened most when the balance between opposing coalitions was held by defectors from political parties and nonparty legislators (independents) who shifted their support from one coalition to another.[20]

In order to test this argument with the larger data base, all the state coalition governments were divided into two sets of dichotomized categories: ideologically nonpolarized and polarized, on the one hand, and coalitions comprised only of parties versus coalitions containing nonparty ministers (independents or defectors), on the other hand. The distinction between polarized and nonpolarized cabinets was a very rough one. Only those cabinets that contained ministers both Left and Right of the Center (that is, on both sides of the Indian National Congress)[21] were classified as polarized, whereas all other cabinets were classified as nonpolarized no matter how diverse they were in other respects. This criterion yielded only 8 cabinets that were classified as ideologically polarized and 26 that were classified as ideologically nonpolarized (Table 4). A comparison of the mean duration of the two types of governments showed that the ideologically nonpolarized governments lasted longer on the average (435.8 days) than the ideologically polarized governments (237.6 days). The *r* with duration, using dummy variables, was .237. Unfortunately, this division of cabinets into polarized and nonpolarized does not provide a satisfactory independent test of the role of ideology because of the fact that seven of the eight polarized governments also contained independents and defectors.

The division of coalition governments into party governments and governments containing independents and defectors yields a more symmetrical division into 18 party governments and 16 governments containing independents and defectors. A government was classified in the latter category no matter how many independents or defectors it contained, the range being from 8 to 100 percent independents and/or defectors in the cabinet. The assumption here is that the presence of even a single independent or defector in a cabinet usually reflects a precarious balance of legislative forces which would allow opportunists to be arbiters of the fates of governments. A comparison of the mean duration of the two types of governments supports the hypothesis that such governments are considerably more unstable than party governments and that a lack of coherence in legislative parties is a major source of instability in coalition governments. The mean duration of coalition party governments is 473.9 days, that of coalition governments containing independents and defectors is only 274.7 days. The correlation coefficient for duration of coalition governments in days, with dummy variables coded 1 for party governments and 0 for governments containing independents and/or defectors, is .277—a slightly stronger correlation than that obtained for

Table 4. Mean Duration of Coalition Governments by Ideological Composition and Party and Nonparty Composition

Type of Government	*N*	Mean Duration
Nonpolarized	26	435.8
Polarized[a]	8	237.6
Parties only	18	473.9
Containing independents and/or defectors	16	274.7

[a] Polarized governments are defined as cabinets that contained parties both Left and Right of the Center. A broader definition of polarized governments that included Left-Center coalitions also as polarized *reduced* the difference of means between polarized governments and nonpolarized governments to 294.7 days and 442.5 days, respectively.

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[20] Paul R. Brass, "Coalition Politics in North India," *American Political Science Review*, 62 (December 1968), 1174–91.

[21] For example, the five-party coalition government of Govind Narayan Singh in Madhya Pradesh in 1967–69 was classified as ideologically polarized because it contained Center elements (the BKD and defectors from the Congress) and parties on the Right (Jan Sangh) and on the Left (SSP) as well as a regional party, the KVD of the Rajmata of Gwalior. However, the 13-party coalition government of Ajoy Kumar Mukherjee in West Bengal in 1969–70 was classified as ideologically nonpolarized because all parties in the coalition were Center, Left, or regional parties, but none were parties of the Right.

polarized and nonpolarized governments.

The importance of opportunism as a source of cabinet instability in India is supported further by a comparison of the data on duration of party governments versus governments containing defectors and/or independents, with minority governments excluded. The results are similar to those obtained with minority governments included. The mean duration for 13 majority coalition governments that did not contain defectors or independents was 560.7 days, that for 8 majority governments that did contain defectors or independents was only 389 days, and the r with duration, using dummy variables, is .265. In other words, opportunism is an important source of cabinet instability in India for majority as well as minority coalition governments.

In considering the role of ideology versus opportunism and the effect of party cohesion or the lack of it in explaining cabinet instability, we are dealing with nontrivial issues on which argument has long raged in comparative politics, particularly with reference to explaining the greater instability of French cabinets in the Third and Fourth Republics as compared with other European multiparty systems.[22] While the data from India do not permit any comment on the debate concerning the relative roles of ideology and opportunism in determining the fates of French coalition cabinets, they do lend support to the broad argument that coalition governments face particular difficulties in multiparty systems in which political parties do not monopolize access to power and in which party cohesion is not firmly maintained by all parties. In modern Europe, only Fourth Republic France, whose governments were the least stable on the Continent in the postwar period, operated a parliamentary system with uncohesive parties.[23] And, in the Indian states, nonparty legislators and defectors from uncohesive parties play critical roles in the fall of coalition governments.

So far, I have been analyzing government stability and instability in the Indian states as if it were uninfluenced by external forces. In fact, that is far from the case. The central cabinet has the power to terminate any state government in India when the state governor—who is a central appointee—reports that no stable government can be formed. But even more important, the central leaders of the Indian National Congress and of other parties make decisions that influence whether or not a government will be formed and which political parties will participate in it. Moreover, party splits originating in leadership conflict at the national level may precipitate party splits in the states that will in turn lead to a loss of support for particular state governments. The national leadership of the Indian National Congress also often worked actively in a variety of ways to undermine the stability of state governments, both those controlled by opposition parties and those controlled by opposing factions in the Congress itself.

Two noteworthy examples of interference by the central leadership of the Congress in state government affairs occurred in 1963 and then again in the period after the split in the Congress in 1969. In 1963, when the so-called Kamaraj Plan was implemented, all the Congress chief ministers in all the states submitted their resignations to Prime Minister Nehru, who then decided which resignations to accept and which to decline to accept. In this way six state governments were terminated and their chief ministers replaced at one stroke. In the period between 1969 and 1974, Mrs. Gandhi and her political emissaries involved themselves directly and often successfully in the affairs of several states in order to replace hostile chief ministers—whether in Congress or out—with chief ministers favorable to her and her party.

It was commonly charged by opposition party leaders in those years that the national leadership of the Indian National Congress frequently sent emissaries to state capitals when non-Congress governments were in power to persuade, wheedle, cajole, and bribe opposition politicians to defect to the Congress and thus bring down non-Congress regimes. It also was often charged that the state governors acted in a manner prejudicial to the interests of non-Congress governments and recommended their termination at the bidding of the central government when internal events did not justify it. Since external intervention was used to bring down Congress as well as non-Congress governments, it is difficult to devise a measure to convert this charge into a testable hypothesis. The coalition governments were, however, divided into Congress and non-Congress governments to test the hypothesis that non-Congress coalition governments, most of them lacking the support of the central government and often incurring its hostility, were likely to have been less stable than coalitions in which the Congress was a partner. In addition, coalition governments allied with the Congress were likely to have been more stable because the Congress in all but a few cases was the largest party in the legislatures. Non-Congress coalition governments were, in fact, less stable on the average, with a mean duration of 312.1 days, than Congress coalition governments, which lasted an average of 522.6 days. The correlation coefficient for duration, with dummy variables for Congress and non-Congress governments, is .264. Thus, the hypothesis that non-Congress coalition governments were likely to have been less stable than Congress coalition governments is supported by the data.

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[22] On the Fourth Republic, see esp. MacRae, Chs. 4–7, and the interesting critique of MacRae's argument in David M. Wood, "Responsibility for the Fall of Cabinets in the French Fourth Republic, 1951–1955," *American Journal of Political Science*, 17 (November 1973), 767–80.

[23] Leon D. Epstein, *Political Parties in Western Democracies* (New York: Praeger, 1967), 334–40.

Regression equations were run in order to calculate the combined effect, first, of the variables measuring governmental structure and, second, of both sets of legislative and governmental variables. The regression equations established clearly that the governmental composition variables were more important than the legislative structure variables in explaining the variance in duration of coalition governments. In an equation run with only governmental variables, both MINMAJCO and CONGNON entered the equation at a significance level of better than .05, with an adjusted R^2 of .247. The ideological character of the government contributed only marginally to the R^2 on the third step and IDG (the dummy variable for presence or absence of independents or defectors in the government) had no effect on the equation. In the equation with both sets of variables entered, MINMAJCO, CONGNON, and N_p entered the equation in that order at significance levels better than .05, with an adjusted R^2 of .347. The ideological character of the government again contributed only marginally—this time on the fourth step—to the cumulative R^2 and again IDG had no effect.

These results, however, were influenced strongly by one case in the data, the government of Mohan Lal Sukhadia of Rajasthan which endured for 1537 days from 1967 to 1971. This government began as a Congress minority government, but was later expanded to a majority government by the inclusion of several defectors in it. Because it was classified in the data as a Congress majority coalition government containing independents and defectors, its inclusion enhanced the importance of the variables, MINMAJCO and CONGNON, and decreased that of IDG. In the regression equations run with this case excluded from the data, MINMAJCO and N_p again contributed significantly to the R^2, but IDG replaced CONGNON in the equation and contributed more than the other variables to the cumulative adjusted R^2 of .365. Consequently, it would not be correct on the basis of the results of the previous regression equations to dismiss the importance of independents and defectors as precipitants of cabinet instability in coalition governments.

In summary, the following conclusions appear justified by the data from the regressions. Of the two sets of variables, it is clear that the composition of the government is more important than the structure of the legislature at the time of the election. However, the number of parties in the legislature also influences the prospects for cabinet stability and instability such that the greater the number of parties in the legislature the less likely are its governments to endure for long. Of the governmental variables, the majority or minority status of the government is of undoubted importance in explaining duration. The case of the Sukhadia government indicates that where a single party has a near-majority of seats in the legislature, the induction of independents and defectors into the government may contribute to its endurance; but, otherwise, independents and defectors play a critical role in undermining governments. Finally, the limited evidence available also suggests that the ideological composition of the government has played at best only a marginal role in the duration of Indian state coalition governments.

Party System Institutionalization and Stability in Coalition Governments. A suitable indicator was sought for a third type of variable that would measure the degree to which the party system as a whole had become institutionalized. The hypothesis tested here is that cabinet instability results in situations where the parties lack stable bases of support in the electorate—that is, they undergo dramatic fluctuations in support—and do not persist over time to the extent of winning seats in the legislature within a predictable minimum and maximum range, or where new parties arise and make inroads into the established bases of support of the principal parties. The assumption behind this hypothesis is that the more dramatically party membership in the legislature fluctuates the more likely it is either that the parties are dependent upon the support ot local notables, political entrepreneurs, and opportunists or that new parties have been created that draw on such support to achieve quick success. Such parties are likely to be less able to maintain internal cohesion in their legislative groups than more institutionalized, stabler parties that return a larger core of persons dependent on the party for their election (rather than vice-versa) and a larger percentage of incumbents.

A rough measure of party system institutionalization that takes into account these features of a state's party system is provided in the following formula:

$$\text{PSI} = \frac{W}{C} - \frac{L}{C}$$

In this formula, party system institutionalization (PSI) for each election equals the ratio of seats won by all parties that have won at least one seat in the state's legislature in any election since 1952 (W) to the number of seats contested by such parties (C) minus the ratio of the number of security deposits forfeited by the candidates of those parties (L) to C.[24] This measure, therefore, ignores independents and completely unsuccessful parties and records only the scores of the successful parties. However, it is sensitive to the rise and fall of parties. For example, a party that was successful in an early election, but is on the decline and loses all seats contested and many of its deposits in a later election will influence PSI in a downward direction for that election. Similarly, a new party that enters an election for the first time and wins one seat, but loses several security deposits also will reduce the size of PSI. If, however, the same parties in a state contest every election and win more seats than they lose deposits, the overall PSI for each election in that state will be positive and relatively high. Therefore, states whose party systems are dominated by parties that persist over time, that regularly win a number of seats, and that know their areas of strength well enough not to field candidates who lose their security deposits will score higher on this measure than states where parties fluctuate in strength, where new parties are rising to challenge older parties, and where such challenges leave few parties with persistent areas of stable support. The maximum score on this index is +1.0 and the minimum is –1.0, but the effective range in genuinely competitive party systems will for the most part move between +.50 and –.50. (Perfect two-party competition where only two parties contest all seats, where each party wins half the seats, and where neither party fields any candidates who lose their deposits will result in a score of +.50.)

The correlation coefficient for duration of coalition governments with PSI is .534. Thus, although PSI is not a direct measure of the structure of the legislature and tells nothing about the composition of particular governments, it is a better explanatory variable for duration of coalition governments than any of the direct measures used before. A regression equation was run which included all the independent variables not highly intercorrelated (Table 5a). Six of the independent variables entered the equation at a significance level of .274 or better. The amount of variance explained by these variables, adjusted for the number of variables and the number of cases, is 42.7 percent, but it is apparent that PSI makes by far the largest single contribution to the equation and dwarfs the importance of all other independent variables.

In this equation as in the previous equations, the results have been influenced strongly by the single case of the Sukhadia government. With the Sukhadia government removed from the data, IDG enters the equation at the second step and CONGNON drops out of it altogether (Table 5b). Otherwise, the relative importance of the other variables does not change significantly. IC, the variable measuring ideological polarization, could not be entered into either of these equations. The strong correlation between duration and PSI and its importance in the regression equation is supportive of the general arguments that have been advanced above and adds a further element to it so that the argument can now be restated as follows: The stability of coalition governments in India depends principally upon the degree to which the legislature and the government are dominated by the larger, more cohesive, and more institutionalized political parties. Coalition governments are likely to be short-lived (1) either when the existing parties have not institutionalized their support adequately or when new parties arise to challenge the institutionalized support of the existing parties; (2) when the parties in the governing coalition either lack internal cohesion or do not have a majority of seats in the legislature; or (3) when there are a large number of small parties in the legislature. Under these conditions, the uncertainty of support and the weakness of parties provide an atmosphere in which political opportunism and party defections flourish and in which the fates of governments depend upon the shifting support of political entrepreneurs.

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[24] Candidates in Indian elections are required to pay a security deposit which they forfeit if they poll less than one-sixth of the vote in single-member constituencies and less than one-twelfth in double-member constituencies.

Table 5a. Multiple Correlation of Coalition Government Duration with Variables Measuring Legislative Structure, Government Composition, and Party System Institutionalization ($N = 29$)

Variable Entered	b	Significance	Multiple R	R^2	R^2 Change	Adjusted R^2
PSI	699.8	.002	.547	.300	.300	.300
CONGNON	195.7	.098	.609	.371	.071	.348
MINMAJCO	187.3	.268	.634	.401	.031	.355
N_p	−29.5	.274	.657	.431	.030	.363
F_p	−23.2	.095	.705	.498	.066	.414
CP	− 6.8	.238	.727	.529	.031	.427

Table 5b. Multiple Correlation of Coalition Government Duration with Variables Measuring Legislative Structure, Government Composition, and Party System Institutionalization, Sukhadia Government Excluded ($N = 28$)

Variable Entered	b	Significance	Multiple R	R^2	R^2 Change	Adjusted R^2
PSI	553.4	.002	.554	.307	.307	.307
IDG	188.3	.016	.673	.453	.146	.432
N_p	−20.6	.351	.688	.473	.020	.431
F_p	−16.9	.193	.715	.511	.038	.450
CP	− 5.1	.323	.730	.533	.022	.452
MINMAJCO	118.0	.305	.746	.556	.023	.455

One-Party Minority Governments

One-party minority governments operate under quite different constraints from those that affect the stability of coalition governments. Minority governments exist and endure because some parties and individuals in the legislature, though unwilling to join the minority government, prefer to support or at least not oppose it rather than try to replace it with an alternative coalition or force a breakdown and a new election. In contrast to coalition governments, the endurance of one-party minority governments should logically be expected to depend more on the degree of cohesion and fragmentation in the legislature. Whereas a majority coalition government can maintain itself in power as long as the parties in power can retain the loyalty of their following in the legislature, the ability of a one-party minority government to endure depends upon both the loyalty and discipline of the governing party and the loyalty and discipline within the parties supporting the government. A split in a large supporting party may cause a loss of support for the government. A highly fragmented legislature may require the government to depend upon the internal discipline of several smaller parties. The greater the number of such parties, the greater the danger that one or more of them may withdraw its support or may divide internally on the question of support for the existing government.

In fact, the data on one-party minority governments in India show that the stability of this form of government is more dependent on the structure of the legislature than are either coalition governments or one-party majority governments. This adds further support to the argument that the prospects for governmental stability increase the more the legislature is dominated by parties and decrease the more the legislature is composed of fragmented groups and independents. All the correlations for one-party minority governments between duration and measures of party dominance are positive–.296 with percent seats held by the governing party, .433 with P_p, and an even stronger .553 with percent seats held by the two largest parties in the legislature (W_p). In contrast, three measures of legislative fragmentation–N_p, F_p, and F_o (fractionalization of the opposition)–correlated negatively with duration: −.548 for N_p, −.575 for F_p, and −.430 for F_o. The multiple correlation for two of these variables, N_p and F_p, was .695, with an adjusted R^2 of .437, explaining 43.7 percent of the variance.

It is also noteworthy that the dominance of large parties in the legislatures is more important for stability than the competitive balance between them. It was hypothesized that one-party minority governments would endure longer the greater the distance between the percent seats held by the governing party in the legislature as compared with its nearest rival and that, conversely, the more even the balance between the two largest parties in the legislature the quicker a one-party minority government would fall. In fact, these hypotheses were not confirmed. The correlation coefficient for duration in days with the ratio between the percentage of seats held by the governing party and the percentage of seats held by the nearest rival (TPR) was –.093.

One-Party Majority Governments

The hardest part of the task remains, namely, to find some explanation for the great range in duration of one-party majority governments —the largest category of governments (76) in the universe of Indian state governments and the category that contains the broadest range (12 to 4685 days). However, as already noted, the relationships and correlations that seemed to explain the duration of Indian state governments when all governments were taken together do not hold up when one-party majority governments are considered separately. None of the legislative structure variables correlate strongly with duration for one-party majority governments. The governmental composition variables used so far are, of course, not relevant. It would be desirable to construct measures of governmental composition that would take into account the factional composition of one-party governments, but such data are not readily available.

The highest correlation found between duration of one-party majority governments and any of the measures used so far in this article was that with PSI which was .228. It should be remembered, however, that PSI is not a direct measure of the structure of the legislature, but an indirect measure of the degree of party institutionalization in the electorate. It suggests something about the character of the parties that hold seats in the legislature, but it has nothing to do with the degree of fragmentation/cohesion in the legislature as measured by the percentage of seats held by the parties. Moreover, the correlation for PSI with duration of one-party majority governments is not nearly as high as it was for coalition governments.

Only three of the variables discussed in this section, PSI, N_p, and TPR entered a regression equation at a significance level of .25 or better (Table 6). Altogether, these variables explain only 7.6 percent of the variance in the duration of one-party majority governments. P_p and CP were eliminated from the regression equation because they did not make any additional contribution to the adjusted R^2 after these three variables were entered. Thus, there are no evident and strong patterns of relationship between the structure of the legislature or the competitive balance among the parties in the legislature that explain much of the variance in the stability of one-party governments.

The absence of any clear relationship between government stability and the competitive relations among parties in the legislature means clearly that we must look elsewhere to explain the relative duration of one-party majority governments. Two broad possibilities may be suggested. One is that factors external to the legislative system may be at work. The other is that the internal relations between factions and between leaders and rank-and-file in the dominant party may be more important than relations between the dominant party and its rivals.

Table 7 provides a breakdown of the one-party majority governments by duration in days and reasons for termination (eight categories) for those governments where data were available. It is immediately apparent from this table that the principal type of termination of one-party majority governments in India has been an interelection change of the chief minister without a change in party. Such changes, which almost always involve some intraparty factional conflict,[25] have accounted

Table 6. Multiple Correlation of Duration with Three Variables for One-Party Majority Governments (N = 76)

Variable Entered	Significance	Multiple R	R^2	R^2 Change	Adjusted R^2
PSI	.063	.228	.052	.052	.052
N_p	.150	.287	.083	.030	.071
TPR	.254	.318	.101	.012	.076

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[25] It would, of course, be desirable to develop a formal indicator of intraparty factionalism in the Congress legislature parties and in Congress governments to correlate with duration, but the fluidity of factional alignments in the Congress is such that an indicator of this sort could be constructed only in

for 30 of the 74 terminations or 40.54 percent. The important question for our purposes is whether such changes in the chief ministership have been distributed unevenly. One hypothesis might be that the longer-lived governments have been led by more skillful chief ministers who were better able to prevent factional conflict, whereas the shorter-lived governments have been led by less able leaders whose governments were more readily brought down by internecine conflict. In fact, the available data do not support this hypothesis at all. A *higher* proportion of the longer-lived governments, 17 out of 40 or 42.50 percent that have lasted more than three years, have been terminated by an internal change of leadership, whereas a smaller proportion of the shorter-lived governments, 13 out of 34 or 38.24 percent, have been so terminated. It should be stressed here, however, that these data do not disconfirm the hypothesis that skillful management of internal factional conflict may provide the principal explanation for the relative endurance of one-party majority governments. It may still be that internal factional conflicts are endemic in most Indian states and that some chief ministers simply manage to control and balance such conflicts more effectively and for longer periods than others, but that ultimately even the most skillful leaders may be forced out by them.

Before taking up this last point, the question of leadership should be placed in a broader context by considering all the types of government terminations. It makes sense to divide these types into two broad categories: (1) those that end in a procedurally regular way (general election) or by direct intervention of the central government (either by dissolving or changing the status of the political unit in question or by inducting the chief minister into the central government) or by accidental circumstances (death of the chief minister); and (2) those that terminate through changes in the legislature leading to resignation of the chief minister and/or a change in the party composition of the government or a complete breakdown in stability and the consequent imposition of President's Rule. The rough distinction being made here is between forces that are more under the control of a state political leader and those that are less so. The hypothesis to be tested is basically the same as that tested for interelection changes in the chief minister, namely, that governments that endure longer will terminate for reasons less under the control of the leader, whereas short-lived governments will terminate for reasons that reflect at least in part upon the leadership skills of the chief minister. In fact, there is not much support for this hypothesis either. Half the governments that lasted for more than three years terminated for "external" reasons, half for "internal" reasons, whereas 47.06 percent of the governments that lasted less than three years terminated for "external" reasons and 52.84 percent for "internal" reasons. Clearly, there are hardly any differences between longer-lived and shorter-lived governments in their forms of termination when the data are broken up in this way.

A further test of this hypothesis was carried out through correlation analysis by assigning points to each form of termination on a scale of 0.0 to 1.0, with the "external" factors assigned a score of 1.0, a resignation followed by a

Table 7. Cross-Tabulation of Duration of One-Party Majority Governments in Days with Reasons for Termination

	Duration in Days				
Reasons for Termination	1826+ $N = 17$	1001–1825 $N = 23$	366–1000 $N = 26$	1–365 $N = 8$	Total $N = 74$
General election	3	5	3	0	11
Dissolution, reconstitution, or change in status of state	0	5	6	1	12
Death of chief minister	4	1	4	1	10
Transfer of chief minister to union government	1	1	1	0	3
Resignation of government followed by reformation	1	0	0	0	1
Interelection change of chief minister	8	9	10	3	30
Change of chief minister/party composition	0	1	0	2	3
Imposition of President's Rule	0	1	2	1	4

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approximate form and only after extensive documentary research.

reformation under the same chief minister a score of 0.50, an interelection change of chief minister a score of 0.25, and a change of both the chief minister and the party composition of the government or a breakdown leading to President's Rule each assigned a score of 0.0.[26] The principle behind the assignment of points for each termination is that the higher scores are assigned to terminations less under the control of the chief minister and the lower scores to terminations that reflect more on the chief minister's leadership abilities, with the lowest scores being assigned to situations involving the greatest deterioration in the dominance of both the leader and/or the governing party. Two correlations were run, one for all terminations and one in which terminations of governments caused by dissolutions, reconstitutions, and changes in the status of the states were excluded. The correlation between duration and form of termination for all one-party majority governments was .115 and for governments with status changes in the states excluded was .204. Thus, there is only modest support for the hypothesis that longer-lived one-party majority governments terminate under conditions that reflect more favorably on the leadership capacities of the chief minister, whereas shorter-lived governments terminate under conditions suggesting less skillful political leadership.

Leadership and Factionalism in One-Party Dominant Systems. Although the correlations just noted are weak, they are at least in the hypothesized direction and suggest that it is worth probing deeper into the events surrounding the termination of those governments whose ends were brought about by "internal" causes. A closer look at the events surrounding 38 internally caused terminations suggests some further distinctions of relevance to the leadership question. For example, some chief ministers have resigned for "health reasons" that do not appear to be reasons of political health, others were asked to resign by the central leadership of the Congress even though they continued to command a firm majority in the state legislative party, some resigned to make way for rivals in the state party after being clearly defeated either in the Congress legislature party or in the organizational wing of the party, the Pradesh Congress Committee (PCC), and some have had to resign because party members defected to the opposition.

Clearly, resignations on legitimate grounds of health do not reflect upon the ability of state leaders to maintain the support of their followers. It is less clear how to assess a forced resignation by the national leadership of the party in comparison with a local defeat of the leader in the legislative party or in the state party organization because of the fact that state and national pressures often reinforce each other. That is, the central leadership of the Congress sometimes intervenes to remove a chief minister who has lost support in state politics. Conversely, a local contest for leadership is sometimes permitted or not opposed by the national leadership when it is hoped that an unpopular leader will be removed in this way without involving the central leadership. Sometimes also, weak and unpopular chief ministers are able to maintain themselves in power solely because the national leadership does not wish them to be removed.

Keeping these reservations in mind, it is nevertheless worth noting that only one-fifth—8 out of the 40 governments that lasted three years or more—terminated in decisive defeats of incumbent chief ministers in either their own party or in the legislature, whereas 11 of the 34 governments or nearly one-third that lasted less than three years terminated in this way. These results again suggest that the question of leadership in the dominant party is worth pursuing further as an explanation for the relative stability of one-party majority governments. However, proper treatment of this question will require qualitative and detailed comparisons of leader-follower and factional relationships in several state parties.

A few examples drawn from the data used in this study will suggest the kinds of questions that need to be pursued in this way. Consider, for example, the case of the second longest consecutive chief ministership on record, the government of Mr. Mohan Lal Sukhadia of Rajasthan that lasted for 4500 days from November 13, 1954, until March 13, 1967. If one counts Mr. Sukhadia's second tenure as chief minister—mentioned above in the discussion of coalition governments—which was separated from the first only by a month of President's Rule in March-April, 1967, Mr. Sukhadia's period in office comprised a total of

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[26] In assigning points to government terminations in this way, I have adapted an idea developed by Leon Hurwitz for classifying types of government formation in "An Index of Democratic Political Stability: A Methodological Note," *Comparative Political Studies*, 4 (April 1971), 41–68. In the larger study of which this article is a part, I plan to apply Hurwitz's Index of Government Persistence to the Indian states as an alternative measure of government stability to that of duration in days.

6037 days (4500 days between November 13, 1964, and March 13, 1967, and 1537 days between April 24, 1967, and July 9, 1971). Sukhadia's period in office spanned the reconstitution of the state of Rajasthan in 1956 (with the merger of the old state of Rajasthan and the princely state of Ajmer) and three general elections. Sukhadia had to control internal factionalism and had to deal at times with a precarious balance of forces in the legislature. He maintained his position in a fragmented legislature (F_p of .634 for the first tenure and .681 for the second) in which Congress dominance was often not secure. The average Congress majority in Sukhadia's first tenure was 57.36 percent, but the Congress secured only 50.00 percent of the seats in the 1962 legislature. Sukhadia's second span of office began with a minority government, the Congress having secured only 48.37 percent of the seats in the elections of 1967. The average ratio between the Congress vote and that of the next largest party was 4.64 in Sukhadia's first tenure and 1.85 in his second. PSI for the first tenure was .0842 and for the second was .1540. The question raised by Sukhadia's two terms in office is how was he able to maintain power in the face of factional conflicts in the Congress, a fragmented legislature, a not very strongly institutionalized party system, and at times less than a majority for his party in the legislature?

At the other end of the spectrum, similar questions occur in reverse. For example, how can one explain the fact that the very first government in the new state of Haryana, that of Mr. Bhagwat Dayal Sharma, ensconced in power with 59.26 percent of the seats in the legislature and with a TPR of 4.00 was brought down by defections from the ruling party after only 12 days in office? Or, at the other end of the subcontinent at an earlier period of time, how does one explain the fact that the government of Mr. S. R. Kanthi in Mysore, formed after the General Elections of 1962 with a strong Congress majority of 66.35 percent of the seats, was replaced after only 104 days in office by a change in leadership to Mr. S. Nijalingappa? Scholars and journalists familiar with the politics of these states probably have ready answers to these questions, but to be theoretically satisfying, the answers will have to be placed in a comparative context. It will not be sufficient to say merely that one government was brought down by defections, another by internal factional conflict. The questions that need answering must be framed comparatively: for example, what is it about the leadership of Mohan Lal Sukhadia that enabled him to turn minorities into majorities in comparison to the leadership of Bhagwat Dayal Sharma whose majority disintegrated in 12 days? Or, were there differences in the structure and composition of intraparty factions that explain the differences in the fates of these two governments rather than differences in the skills of the two leaders?

Until now, questions concerning leadership and factionalism in Indian politics have been treated only in monographic studies of particular leaders or particular states. Such studies constitute a resource that can be used in looking for theoretical answers to comparative questions concerning the conditions which facilitate or impede long tenure in office and stability or instability in Indian state governments. This article demonstrates not that further case studies of leadership and factionalism in Indian politics are no longer needed, but that the cases must be selected carefully and the questions framed precisely so that the results can be integrated into a general theoretical explanation that specifies the relationships among leadership and factionalism, legislative fragmentation and cohesion, and party system institutionalization in determining the fates of governments.

On the Conditions for Coalition and One-Party Systems in the Indian States

This analysis of the causes of cabinet stability and instability in coalition and one-party systems in the Indian states suggests another larger, but related question concerning the conditions under which different types of party systems arise. It may be, in fact, that the narrower question cannot be answered satisfactorily until the larger question is addressed. The importance of party system institutionalization as an explanatory variable for all types of governments in India suggests as much. For party system institutionalization depends upon the ability of the parties to establish strong, secure, and stable bases of support in the electorate. How successful the parties are in doing so in turn is likely to depend upon the degree of fragmentation and cohesion that exists in the social structure and the ways in which the parties relate to social and cultural cleavages.

Although it is beyond the scope of this article to pursue this question in detail here, some of the problems posed by the Indian data and some hypotheses for further research are suggested by the distribution of coalition and one-party minority governments, as shown in

Table 8. Fifteen of the 39 provincial units that have had some experience with cabinet government in India also have experienced either coalition or one-party minority governments. However, some of those provinces no longer exist and others have had experience with only one or two coalition or minority cabinets. More than 80 percent (42 out of 51) of the coalition and minority governments that have been formed in the Indian states were concentrated in eight states–Bihar (8), Kerala (8), Orissa (6), Uttar Pradesh (6), Punjab (4), West Bengal (4), Madhya Pradesh (3), and Manipur (3). Leaving aside the tiny hill state of Manipur (about whose social structure and political process very little is known), the seven larger states can be grouped broadly into three types–the extremely large (in population or in area), extraordinarily heterogeneous, and socially fragmented states of Bihar, Uttar Pradesh (U.P.), and Madhya Pradesh (M.P.); the smaller states of Kerala, Orissa, and Punjab that are divided socially and culturally by two or three sharp cleavages;[27] and West Bengal which is intermediate in size and cultural heterogeneity between these two groups of states. In the first group of states, social and political fragmentation have gone together. Throughout most of their postindependence political history, the Congress has been dominant in these states, but when the Congress failed to win a safe majority of seats, the opposition was too fragmented to provide coherent and stable alternative governments. In two of the smaller states, Kerala and Orissa, the Congress has never been dominant and several political parties have organized successfully and maintained stable support within distinct subcultural segments of the society. Punjab is similar to Kerala and Orissa with regard to the relationship between political parties and social cleavages, but is different from these two states since the Congress has had a stronger position there. West Bengal, like the states in the first group, was a Congress-dominated state until 1967. When the Congress lost its majority there after the 1967 election, the opposition was fragmented, but a distinct ideological-political polarization developed in the competition between the Congress and the Communist Party of India (Marxist).

The states that had no experience with coalition or minority governments in the period under review were Assam, Maharashtra, Tamil Nadu, Mysore, and Gujarat. While there are no evident similarities among these states that distinguish them clearly from the states that have experienced coalition and minority governments, they are all smaller than the three

Table 8. Distribution by State and by Duration in Days of Coalition and One-Party Minority Governments

	Duration in Days					
	1001–1825		366–1000		1–365	
State	Coalition *N*	One-Party Minority *N*	Coalition *N*	One-Party Minority *N*	Coalition *N*	One-Party Minority *N*
Andhra			1			
Bihar					8	
Goa		1				
Haryana					1	
Kerala			3	3	1	1
Madhya Pradesh			1		1	1
Madras			1			
Manipur			1		2	
Orissa	1	1	2	1		1
Pepsu					1	1
Pondicherry		1			1	
Punjab			1	1	2	
Rajasthan		1				
Uttar Pradesh				1	2	3
West Bengal	–	–	1	–	3	–
Total (51)	1	4	11	6	22	7

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[27] For analyses of the relationships between party systems and social structures in these three states, see F. G. Bailey, "Politics and Society in Orissa (India)," *Advancement of Science*, 19 (May 1962), 25–28; Paul R. Brass, "Ethnic Cleavages and the Punjab Party System, 1952–1972," in *Party Systems and Cleavages*, pp. 7–69; and Robert L. Hardgrave, Jr., "The Kerala Communists: Contradictions of Power," in *Radical Politics in South Asia*, ed. Paul R. Brass and Marcus F. Franda (Cambridge: MIT Press, 1973), pp. 134–38.

huge and heterogeneous states of Bihar, U.P., and M.P. None of the states except Assam has developed a pattern of political organization of subcultural cleavages that characterizes the states of Kerala, Orissa, and Punjab. Finally, one of the states, Tamil Nadu, is distinctive in the extent to which it has developed a cohesive political culture based on Tamil nationalism.

While these broad distinctions are only suggestive, they do point the way to two hypotheses concerning the conditions under which coalition and minority regimes are likely to arise and concerning the stability of such regimes. One hypothesis that clearly needs to be explored further is that such systems arise in social orders characterized either by extreme heterogeneity and fragmentation or by a more or less sharp social-political organization of two or three subcultural cleavages. A closer examination of the data in Table 8, which shows that half the coalition and one-party minority governments that have lasted less than a year have been in the two states of Kerala and Orissa and that 13 of the 29 coalition and one-party minority governments that have lasted less than a year have been in the two states of Bihar and Uttar Pradesh, suggests a second hypothesis. The differences between these two groups of states suggests that systems that are characterized by the political organization of a few distinctive subcultures are likely to produce more stable patterns of coalition and minority government than extremely fragmented and heterogeneous systems. This second hypothesis links up with the relationship established in this article between cabinet stability and party system institutionalization. The party systems in Kerala and Orissa are more institutionalized than those in Bihar and Uttar Pradesh in the sense that the principal parties in the two smaller states have persisted longer, have undergone less dramatic fluctuations in popular support, and have resisted more effectively the inroads of new parties. (These differences are reflected in the mean PSI indexes for those states for all elections held between 1952 and 1972, which are as follows: +.066 for five elections in Orissa, +.244 for seven elections in Kerala, −.150 for six elections in Bihar, and −.218 for five elections in U.P.)

It also deserves to be noted that Kerala and Orissa are quite distinguishable from Bihar and U.P. with regard to the roles played by party defectors in government formation and termination. Although defectors played roles in the formation and termination of three governments in Orissa and Kerala,[28] they were not as significant a force as in Bihar and U.P. between 1967 and 1972 when nearly every government either contained defectors or depended upon the shifting support of defectors for its endurance. Moreover, in the post-1967 period when most governments formed in all four states were either coalition or one-party minority governments, there were numerous defections affecting nearly all parties in Bihar and U.P. but no defections in either Orissa or Kerala.[29] These associations between social organization, party institutionalization, opportunism, and government instability suggest a possible causal sequence, namely, that a segmented social structure produces party institutionalization (or vice versa), which in turn reduces opportunism and enhances the prospects for stable government. In contrast, a fragmented society may restrict the ability of parties to become institutionalized, leaving opportunities for political entrepreneurs whose ceaseless maneuverings decrease the prospects for stable government. On the other hand, and by the same logic, it may be hypothesized that social orders with either a dominant social segment—such as a caste or a class—or a homogeneous political culture are more likely to provide the conditions for one-party dominant or competitive two-party systems. These linkages and possible causal relationships cannot be explored further in this article, for they require the development and comparative analysis of a whole new set of data on the internal social and cultural fragmentation and cohesion of the Indian states and on the relationships between social structure and party organization. The extension of the study of cabinet instability into this broader area of social and political structure does, however, appear to be necessary if a more complete theoretical explanation of its causes is to be developed.

Conclusions

This article began with the purpose of replicating, with data drawn from the universe of Indian state governments, hypotheses derived from Western parliamentary experience concerning the causes of cabinet instability. Several of the principal hypotheses from the

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[28] Government of India, Ministry of Home Affairs, *Committee on Defections: Proceedings of the Committee and Papers Circulated to the Members* (New Delhi: Government of India Press, 1969), pp. 48–49.

[29] Ibid., esp. pp. 67–73. See also Subhash C. Kashyap, *The Politics of Defection: A Study of State Politics in India* (Delhi: National Publishing House, 1969).

Taylor and Herman study were tested with the Indian data. All their hypotheses tested here were supported by the data from the entire universe of Indian state governments, but in all cases at lower levels of explanation than in their study. Moreover, the correlations differed markedly when the different types of governments—coalition, one-party minority, and one-party majority—were considered separately. In addition, there remained a great range of variability in the duration of all three types of governments.

Consequently, correlations were run separately for each type of government. It proved easier to arrive at some general conclusions concerning the stability of coalition and one-party minority governments on the basis of available quantitative data than to do so for one-party majority governments. It was clear that much of the variance in the stability of governments in India was explainable first in terms of the generally much longer average duration of one-party majority governments. It also was clear that a part of the variance in the duration of coalition governments was attributable to the distinction between majority and minority coalition governments.

The less evident and more theoretically important findings on coalition and one-party minority governments concerned the relative roles of ideology, opportunism, party cohesion, and party system institutionalization. Coalition governments in general were the most unstable form of government in India in the period under review—more unstable even than one-party minority governments. Even so, there was a substantial variability in the duration of such governments. Among the factors considered in seeking an explanation for that variability, it was found that ideological polarization as defined in this article did not explain much of the variance, but that a lack of party cohesion in the legislatures and the presence of opportunism were more critical factors. Opportunism and a lack of party discipline in the legislature in turn were related more broadly to a low degree of party system institutionalization in the electorate. Instability promoted by opportunism and party defections was most prominent among coalition and one-party minority governments when a state party system as a whole was undergoing realignment, when old parties were losing support and new parties were rising to challenge the old, or when party dominance itself was challenged by increasing numbers of successful nonparty candidates.

Variability in the duration of one-party majority governments was harder to explain. None of the quantitative measures alone explained more than 5 percent of the variance in the duration of one-party majority governments, and three variables entered into a regression equation together explained no more than 7.6 percent of the variance. Leadership and the relationship between leadership and factions in the dominant party appear to be the critical factors in the relative stability of these governments, but leadership is difficult to express in quantitative terms. The indirect measure of leadership skill used in this article provided only very weak support to the hypothesis that leadership skill is the critical independent variable in explaining the relative duration of one-party majority governments; further probing of the data, however, suggested that the problem may lie in the absence of good measures rather than in the lack of importance of the leadership variable. Questions raised by consideration of a few concrete examples suggested that careful selection of cases—particularly the apparently anomalous cases of long and short tenures of chief ministers—for documentary and field research is indispensable for the further development of theory on the causes of government instability in one-party dominant systems.

The tentative findings and hypotheses preesented above on the issue of leadership in one-party dominant systems suggest also a methodological argument, namely, that the development of theory in comparative politics depends upon maintaining a close relationship between quantitative data analysis and qualitative case studies. If the indiscriminate proliferation of unsystematically selected case studies in comparative politics, particularly in developing countries, is no longer considered methodologically useful, neither should the indiscriminate proliferation of correlation and regression analyses that leave us with most of the variance still to be explained. What this study suggests is that case studies continue to be necessary in comparative politics, but that they should be selected only after evident regularities have been demonstrated, the amount of unexplained variance specified, and the anomalous cases that hold promise of completing the explanation identified.

A brief review of the types of states in which coalition and minority governments have been most common also suggested a possible link between theories of cabinet stability and theories relating party systems to different types of social structures. A causal relationship was hypothesized in which the relationship between political parties and social and cultural segments of the society affects party system institutionalization which in turn affects party cohesion in the legislature, the scope available for opportunists, and hence the stability or instability of governments. In this model, the emphasis has been placed on weaknesses of organization and of institutionalization of party systems rather than upon the immobilizing effects of ideological differences between parties as the principal causes of government instability in the Indian states. Although the evidence from the data does not provide a clear demonstration that ideological differences between parties are or are not obstacles to stable government, it does indicate strongly that, in the Indian states, political fragmentation, weak institutionalization, and personal opportunism are more critical factors.

National Elite and Local Politician in Taiwan*

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In Taiwan, a national elite socialized to measure the behavior of others (but not its own behavior) against extremely high standards of morality finds itself offended by the apparently immoral behavior of local electoral politicians. With reference to similar situations in other developing countries, this article describes how the attitudes of Taiwan's national elite toward the political morality of others developed and why the electoral politicians act in a manner offensive to the elite's attitudes. As a postscript, the article discusses why the national elite has checked its desire to do away with Taiwan's offensive fledgling electoral system and expanded its scope instead.

In 1965, Myron Weiner published an article entitled "India: Two Political Cultures."[1] In this article, Weiner described an elite political culture centered around India's bureaucracy and national rulers, and a mass political culture centered around India's state and local politicians. According to Weiner, these two political cultures represented very different views of the way politics should be carried on in India, and if political statesmen capable of bridging the gap which separated them did not come forward, a potential conflict between the two was likely to develop. Such a conflict, Weiner feared, would endanger the continuation of democratic politics in India.

Weiner noted that these two political cultures and the gap between them were not unique to India, but at one time represented a general condition throughout much of the developing world. Today this condition still exists in those few countries in which the national elites have not yet completely stifled local level democracy in an effort to rid their polity of the rival political culture.

Taiwan is one of those few developing countries in which the political culture of the national elite and the mass political culture still confront one another. In this article, I will describe how these two political cultures came to exist on Taiwan and the trend of Taiwan's politics under the influence of their conflicting relationship. Unlike the pessimistic prognosis for democracy in most of the developing world, however, my conclusion will be that, given the political isolation of Taiwan's national elite in the world today, there is reason to hope that the trend of Taiwan's politics will be toward an accommodation by the national elite with the mass political culture.

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*This article summarizes my research findings concerning provincial and local politics in Taiwan and integrates these findings with related findings concerning Taiwan and from the general field of comparative politics. My own findings are based on the reading of materials published in Taiwan and on interviews conducted in Taiwan between the summers of 1966 and 1968. Interviews, taking the form of informal conversations, were guided by a standardized series of questions, in combination with a feel for the most productive line of questioning at a given moment. Interviews ranged from single hour-long sessions in the relatively formal atmosphere of an interviewee's office, to repeated informal contacts in a variety of surroundings, often including the interviewee's own home. In all, hundreds of hours were spent in Taiwan's various counties and cities, conversing with provincial, county, and city assemblymen; provincial, county, and city assembly staff workers; citizens active in local political campaigns; local newspaper reporters; government officials from the central, provincial, and county-city levels; local and foreign social scientists; and foreign officials stationed in Taiwan. Many hours were also spent in talks with members of Taipei's mainlander community.

Based on long and repeated talks with informants, my findings do not lend themselves to statistical statements. The sources which I use to bolster and expand upon my interview evidence–newspapers, magazines, books, and official documents published on Taiwan and research reports about Taiwan published abroad–are also rarely of a statistical nature. However, in footnotes I do include sources based on statistical research when they apply. Note that my interview evidence stands behind most statements in this article. Footnote citations, therefore, are limited to those sources which bolster and expand upon this evidence.

I am indebted to J. Bruce Jacobs for his careful reading and his criticisms of the original draft of this article. Of course, only I am responsible for all mistakes which remain uncorrected.

An earlier version of this article was presented as a paper at the 26th Annual Meeting of the Association for Asian Studies, Boston, Massachusetts, April 1-3, 1974.

[1] Myron Weiner, "India: Two Political Cultures," in *Political Culture and Political Development*, ed. Lucian W. Pye and Sidney Verba (Princeton: Princeton University Press, 1965). See esp. pp. 227–28, 243–44.

While reading my description, the reader should bear in mind that although the text stays close to the reality of Taiwan, most aspects of the description are congruent with the experience which other societies have had concerning the development of the two political cultures and their subsequent conflict. This congruency is not complete, however. While it does include aspects that are often regarded as uniquely Chinese, the aspects of land reform and the possible amelioration of the conflict between the two political cultures are not to be found in many other societies.

A Note on Comparative Methodology. Since I will be describing the conflict between the behavior of local politicians and the ideals of Taiwan's national elite, the reader may be disturbed by a lack of parallelism. Should not behavior be contrasted with behavior and ideals with ideals? I persist in this lack of parallelism because the relevant conflict on Taiwan—as it is (or was) in many other developing countries—is between the actual behavior of electoral politicians and the ideals of the national elite.

The political behavior of the national elite is relevant to this discussion only as a dependent variable, dependent on the perception by the national elite that its ideals are being violated by the behavior of local politicians. It is this perception which motivates the national elite to behave in such a way as to restrict the behavior of local politicians.

The political ideals of the local politicians are even less relevant to this discussion. If investigated, one will find some of Taiwan's local politicians holding ideals quite inconsistent with their behavior and rather in agreement with the ideals of the national elite. However, as will be noted below, other local politicians are well aware of what their behavior is and hold a set of ideals which supports this behavior. Whatever a local politician's ideals are, however, is not relevant to the main thrust of this paper, since these ideals simply do not enter into the *local politician behavior in conflict with national elite ideals leads to elite behavioral reaction* equation.

A second comparative problem is that this paper is, strictly speaking, discussing aspects of three, not two, political cultures. In addition to the political culture of Taiwan's national elite (primarily represented in this article by the aspect of elite ideals and secondarily represented by one aspect of elite behavior), the "mass political culture" under discussion is divisible into the political culture of Taiwan's ordinary voters and the political culture of Taiwan's local politicians (both represented in this article primarily by aspects of political behavior). Although closely related, I have taken pains to treat voters and local politicians as distinct groupings.

Some Definitions. Note that for the pre-1946 period (the year in which local elections were instituted), I use the term "local politician" to refer to landlords, heads of large lineages, and any other Taiwanese who were close enough to ordinary individuals living in the countryside to serve as patrons or otherwise importantly influence their lives. For the post-1946 period, the term "local politician" refers to any candidates for elective office from any election district in Taiwan, as well as to their active supporters. This definition is meant to include candidates for national political bodies (elections for which were instituted in 1969) as well as for lower level offices.

I use the term "traditional" to refer to the totality of those sociopolitical practices on Taiwan which, at any given point in time, have not yet felt the influences of: (a) post-1945 economic change, (b) the 1949–1953 land reform program, and (c) the institution of elections in 1946 and important electoral reforms such as those taking place in 1950 and 1961. Note that because the influences of these phenomena made themselves felt at different points in time, because different regions felt these influences in different ways, and because the residual effects of these influences are still being absorbed by the sociopolitical system, my use of the term "traditional" should not be understood as referring to the totality of Taiwan's society at any given point in time. Instead, it should be understood as referring to only those sociopolitical practices which had not yet felt the influences of these phenomena at the time in question.

The World of Taiwan's National Elite and Its Effect on the National Elite's Political Culture

In order to limit the scope of this article to the main points under discussion and to restrict the text to a manageable length, the following description of the world of Taiwan's national elite and the effect of this world on the national elite's political culture will not be comprehensive. It will be confined to the images the national elite holds in its own mind of (a) what a good political system should be, (b) the benefits such a good political system would bring to society, and (c) the role the national elite should play in such a political system. This description will not deal with the images which individuals outside the Taiwan national elite hold in their own minds of the actual political systems which Taiwan's national elite has been identified with or of the national elite's actual role in these systems. These less "subjective" images of the national elite's politics have been extensively treated in other works and need not concern us here.

Note that I am using the term "Taiwan's national elite" to refer to individuals who were born in mainland China, who came to Taiwan in the years after it was returned to China in 1945, and who now occupy national and lower level positions in the government or in the Kuomintang (the ruling political party). It is my strong impression that a large proportion of the middle class mainland Chinese citizens who accompanied this elite to Taiwan, as well as many of their children, share the elite's political culture. Since the mainland Chinese citizens are so close to the national elite and since they are the national elite's strongest supporters in Taiwan, it is conceivable to expand the definition of the national elite to include them.

"Democracy" as a Crucial Element in the World of the National Elite. As it was for the elite of many of the developing nations, democracy was one of the most important goals of Taiwan's national elite. The meaning of democracy for members of the elite, however, is a basic cause of the gap between them and the politicians rising out of the local level electoral system.

The major problem in the minds of national elite members such as those on Taiwan was how to meet the challenge of the European world. The failure of their nation to meet this challenge was causing them much humiliation. Like their counterparts in similarly challenged nations, members of Taiwan's national elite felt—even while growing up on mainland China—that it was an urgent task to restore "wealth and power" to their nation, thus erasing their humiliation.

In the search for means to the end of wealth and power, many elite members turned away from solutions suggested by their own traditional cultures as having already failed to cope with the European challenge. Instead, they attempted to discover the secrets behind European wealth and power in the hope of adopting those secrets for their own use.

The threatening Europeans encouraged this attempt, missionaries and political ideologists presenting one or another of their own favorite notions as a means to wealth and power. In China, Western style electoral democracy was at first accepted by many as one important means to wealth and power. But as time went on, those who saw Western style democracy as important became fewer and fewer. It was among that portion of the Chinese national elite which retreated to Taiwan (thus becoming Taiwan's national elite) that a faith in Western style democracy appears to have been retained—or to have been reborn—after 1945.

Given a need to restore prestige to its nation (minimally to that portion of the nation still controlled by the national elite, i.e., Taiwan; ideally, of course, to all of China as a consequence of the elite's long-awaited triumphant return to the mainland), thus restoring its own self-esteem, the national elite felt a deep commitment to democracy. But these needs also shaped the content of the elite's definition of democracy. Among the elements which the elite saw as crucial to the definition of democracy were:

1. *Liberating the energies of the people and channeling them into public affairs.* The major goal of the elite was to restore pride and self-esteem. Pride and self-esteem in the world in which China was being challenged came from the possession of wealth and power. According to the elite's understanding, democracy would liberate the energies of the people which had been repressed by Confucian society. Once liberated, these energies by democracy would be channeled into public affairs. Once in the arena of public affairs, the vast energies of the Chinese people could then be tapped for a national drive to wealth and power.[2]

2. *Disciplining the energies of the people.* Once the energies of the people are in the arena of public affairs, what is to prevent them from

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[2] For historical background concerning this discussion see Benjamin Schwartz, *Chinese Communism and the Rise of Mao* (Cambridge, Mass.: Harvard University Press, 1951), p. 9; Benjamin Schwartz, *In Search of Wealth and Power* (Cambridge, Mass.: Harvard University Press, Belknap Press, 1964), pp. 237–47; and George Paloczi-Horvath, *Mao Tse-tung: Emperor of the Blue Ants* (Garden City, N.Y.: Doubleday, 1963), pp. 30, 32, 46. For examples of the relationships of "wealth and power" and democracy in the writings of Taiwan's national elite, see Hsueh Jen-yang, "Hsu," in *T'ai-wan Sheng Ti-fang Tzu-chih Chih-yao,* ed. Hou Ch'ang (T'ai-pai: T'ai-wan Sheng Ti-fang Tzu-chih Chih-yao Pien-chi Wei-yuan-hui and Chung Hua Ta-Tien Pien-yin-hui, 1965), preface, p. 2; "Message of Chiang Kai-shek," in Fred W. Riggs, *Formosa Under Chinese Nationalist Rule* (New York: Macmillan, 1952), pp. 154–55; and Yang I-feng, *Chung-kuo Ti-fang Tzu-chih Hsin-lun* (T'ai-pei: Cheng-chung Shu-chu, 1962), pp. 17, 39–40, 57–60.

flowing in the wrong direction? Or worse, what is to prevent them from flowing in many directions at once, producing conflict and chaos? A modern day American liberal might see in a situation of free-flowing energies possibilities for great creativity, as was manifested during such periods of Chinese history as the late Chou, the Six Dynasties, or the Republican period. But for the elites of the developing world, such creativity was always accompanied by great disunity, disorder, and disharmony—three attributes which guaranteed a lack of wealth and power for their nations as a whole. These periods, therefore, do not reflect the workings of effective solutions to their contemporary problems. The energies of the people must be disciplined into a unified flow if wealth and power are to be achieved.[3]

Concern for unity, order, and harmony became significant to the Chinese much earlier than the appearance of the European challenge. Much traditional Chinese political philosophy was concerned with how to counter the nation's tendencies toward disunity, disorder, and disharmony.

Nor was this concern limited to the traditional elite philosophers. Ordinary Chinese peasants knew well the dangers of social disorder. They had learned of the great misery social disorder can cause, both from listening to stories of social breakdown in other times and places and from their own personal experience.

Recently, a number of scholars (Lucian W. Pye and Richard H. Solomon, for example) have speculated that Chinese-felt needs for unity, order, and harmony may additionally result from the use of such psychological defense mechanisms as repression, projection, and displacement. According to these scholars, the Chinese call on these mechanisms in order to deal with the severe anxiety which arises from immersion in traditional-style Chinese interpersonal relations—a style which, they claim, persists in the interpersonal relations of Chinese today.[4]

The necessity to channel the liberated energies of the people in unity, order, and harmony in turn required adding to the definition of democracy a further specification.

3. *Orderly discussion in search of a unified general will.* Elites such as those now on Taiwan came to believe that democracy, because it originated in the "advanced" European world, offers something akin to a scientifically correct method for discovering the best policies for attaining the public good. According to the elites' understanding of democracy, in order to discover the best policy to deal with a specific problem, the citizens and their representatives hold a marketplace of ideas. Ideas are traded in a calm, considered, rational manner, as all sincerely aid each other in a search for the correct method. Since it is a matter of science, the result will be a method proven best for all to see. Once found, therefore, the citizens will naturally all support the implementation of this manifestation of the general will.[5]

As with unity, order, and harmony, this Rousseauian concern for orderly discussion in search of a unified general will probably had correlates in Chinese culture long before it came in contact with European political concepts. The specific correlate which I have in mind is the Chinese ideal of social decision making in accordance with a high degree of wisdom, tranquility, moderation, and consensus.[6] Of course, just as with the Rousseauian ideal of democracy, actual practice was generally far less calm, considered, and rational than the ideal would suggest. In reality, for both Chinese and Western practice, the frequency, content, and enforcement of decisions was usually the reflection of the changing balance of concrete force among society's power holders.[7] But, psychologically, Chinese proclivity for the ideals of wisdom, tranquility, moderation, and consensus in decision making may well have encouraged their receptivity to the Rousseauian ideal of democracy.

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[3] For example, the importance of "organized democracy and disciplined freedom" is stressed by Huang Chieh, former chairman of the Taiwan provincial government, in *T'ai-wan Kuang-fu Erh-shih-nien*, ed. T'ai-wan Sheng Hsin-wen-ch'u (T'ai-pei: Chung-hua Ta-tien Pien-yin-hui, 1966), preface, p. 6. For other examples bearing on this point, see Ch'en Li-fu, *Kuo-fu-chih Ch'uang-chien yu Chung-hua Wen-hua-chih Pi-jan Fu-hsing* (T'ai-pei: Chiao-yu-pu, Wen-hua-chu, 1971), pp. 8, 12–14; Fu Ch'i-hsueh, *Chung-kuo Cheng-fu* (T'ai-pei: T'ai-ta Fa-hsueh-yuan Shih-wu-tsu and T'ai-pei Shih San Min Shu-chu, 1964), pp. 76–78, 83–84, 108–10; and "Ts'ung 'Luo-pen' Nao-chu Lun Tzu-yu yu Chih-hsu," editorial in *Chung-yang Jih-pao* (Hang-k'ung-pan), March 11, 1974. See Weiner, pp. 230-31, 235-37, for the experience of India's national elite in terms of the need for unity.

[4] In this regard, see especially Lucian W. Pye, *The Spirit of Chinese Politics* (Cambridge, Mass.: MIT Press, 1968); and Richard H. Solomon, *Mao's Revolution and the Chinese Political Culture* (Berkeley: University of California Press, 1971).

[5] Pao Ch'ing-chiu, *T'ai-wan Sheng I-hui* (Mu-shan, T'ai-wan: Kuo-li Cheng-chih Ta-hsueh Kung-kung Hsing-cheng Ch'i-yeh Kuan-li Chung-hsin, 1964). Pao's discussion exemplifies this conception of democracy (p. 98). For the stress on nonconflictive politics in general, see Yang I-feng, *Chung-kuo Ti-fang Tzu-chieh Hsin-lun*, pp. 17, 148–49; Ch'en, *Kuo-fu-chih Ch'uang-chien*, p. 8; and Ho Ming-chung, *Chung-hua Wen-hua Tui Shih-chieh Wen-hua-te Kung-hsien* (T'ai-pei: Chiao-yu-pu Wen-hua-chu, 1971), p. 30. For the attitude of India's national elite in regard to democracy as a rational process, see Weiner, p. 229.

[6] Descriptions of this ideal may be found in such works as: Hsiao Kung-chuan, *Rural China: Imperial Control in the Nineteenth Century* (Seattle: University of Washington Press, 1960), p. 291; H. G. Creel, *Chinese Thought from Confucius to Mao Tse-tung* (Chicago: University of Chicago Press, Mentor Books, 1953), p. 197; Francis L. K. Hsu, *Americans and Chinese* (Garden City, N.Y.: Doubleday Natural History Press, American Museum Science Books, 1970),

"The Movement" as a Crucial Element in the World of the National Elite. Such values as unity, order, and harmony were also fostered by another set of experiences of Taiwan's national elite and its counterparts in other countries. These elites were not only humiliated by the inability of their countries to meet the European challenge, were not only attracted to European ideologies as methods of countering the challenge, but also joined movements to propagate the European ideologies within their own societies.

These movements fostered a complex of values rooted in the necessities of a prolonged and difficult struggle. Unity, order, and harmony were among these values. Closely related were the values of party loyalty, selflessness, and willingness to sacrifice for the general good, and the importance of duty and obligations over rights.

Two points are worthy of note here. First, it is not necessary for believers in these values to actually live up to them. They may consistently violate them. But their image of themselves and the standards by which they judge other people are defined by these values. Secondly, the complex of values fostered by the movement experience may become prime values in themselves. In addition to meeting the European challenge, participants come to see it as important to be unified, loyal, selfless, steadfast in duty, etc. They will likely also attempt to institutionalize these values and the general inspirational atmosphere of the movement into their society.[8]

A cadre ethic may also develop out of the movement. Holders of this ethic will believe that those who struggled for so long have been tested and proven to be especially qualified in morality and in knowledge to be society's leaders. The ordinary citizen should pay heed to their words and copy their deeds.[9]

In countries like Taiwan, the cadre ethic is held in combination with an "expert ethic." This ethic is not a product of the movement experience. It is a result of the general recognition of the importance of "science" as an additional method for the attaining of wealth and power. According to this ethic, many government officials, given experience and/or special schooling, are considered scientific experts. These experts are thought to be especially qualified to make decisions concerning social policy.[10]

A major conclusion is drawn by elites which fits in closely with the cadre-expert ethic. Under the influence of their definition of democracy as involving calm and considered decision making, the elites assume that democratic citizens will be rational and that this rationality will be expressed by the choice of the best people and the best policies with which to run society. And since the elites are convinced that it is the cadres and experts within their own ranks who are the best people, and that the policies of these cadres and experts are also the best policies, they naturally conclude

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pp. 369–70, 380–81; and Bernard Gallin, "Political Factionalism and Its Impact on Chinese Village Social Organization in Taiwan," in *Local-Level Politics*, ed. Marc J. Swartz (Chicago: Aldine, 1968), p. 385. For expressions of this ideal among Taiwan's national elite, see Ho, *Chung-hua Wen-hua*, pp. 10–14, 33, 40; Ku I-ch'un, *Ts'ung Tui Shih-chu-te Kuan-ch'a Lai Chien-t'ao Chung-hua Wen-hua Ching-shen* (T'ai-pei: Chiao-yu-pu Wen-hua-chu, 1971), p. 12; and Ts'ao Min, *Lun Fu-hsing Chung-hua Wen-hua Chi-ko Szu Shih Erh Fei-te Kuan-nien chi Ch'i Fu-hsing-chih Tao* (T'ai-pei: Chiao-yu-pu Wen-hua-chu, 1971), pp. 16–24, 27–28, 30–32.

[7] For descriptions of actual (as opposed to ideal) social decision making in Chinese society, see Gallin, "Political Factionalism," p. 385; Burton Pasternak, *Kinship and Community in Two Chinese Villages* (Stanford, Calif.: Stanford University Press, 1972), p. 99; and Hsiao, pp. 262–64, 290–92.

[8] On the influence of "the national movement" on such values as unity and sacrifice, see Weiner, pp. 230–33. On Taiwan the importance of "duty and obligations over rights" is stressed by Fu, p. 77.

[9] The importance of defining oneself as an "old revolutionary" (lao ko-ming) on Taiwan, is noted by Kao Chun, in "T'ai-wan Ch'uan-li Mao-tun chi Ch'i Chieh-chueh-chih Tao," *Ming-pao Yueh-k'an*, no. 109 (January 1975), 80.

[10] That such an "expert ethic" exists on Taiwan is attested to by Assemblyman Yeh Ping-huang, quoted in *T'ai-wan Sheng I-hui Ti-san-chieh Ti-pa-tz'u Ta-hui Chuan-chi* (Wu-feng, T'ai-wan: T'ai-wan Sheng I-hui Mi-shu-ch'u, 1967), vol. 2, p. 2574; by Kao, "T'ai-wan Ch'uan-li Mao-tun," p. 80; by Felix Tardio, *Mr. Tardio Sees Taiwan* (Taiwan: By the Author, 1966), pp. 109, 196; and by Liu Yen-fu, *Ti-fang Tzu-chih Lun-chi* (T'ai-pei: Min-chien Chih-shih-she, 1964), preface, p. 2.

that the populace will support these people and accept their policies.

The World of the Local Politician and Its Effect on the Local Politician's Political Culture

The politicians who represent the "mass" political culture have been formed in a very different world from that of these national elites. Below, I will describe the world of Taiwan's local politicians—as well as the world of the ordinary individuals to which the local politicians must respond—in terms of the basic goals of local politics, the way these goals have been achieved during Taiwan's traditional period, the way society has been changing, and the way in which these changes have been affecting methods of goal attainment. This description will demonstrate how local politicians such as those in Taiwan are constrained by their environment to behave in ways which directly conflict with the just described political ideals of their respective national elites. (See above section on *Comparative Methodology* for an explanation of the lack of parallelism implied in opposing the *behavior* of local politicians to the *ideals* of Taiwan's national elite.)

As in national politics, the main goals of local politics were physical security, wealth, and prestige; national politicians conceived of themselves as pursuing these goals on behalf of the nation as a whole. Individuals at the local level, however, were much more likely to conceive of themselves as pursuing these goals on behalf of themselves and their immediate relations.

For traditional societies such as Taiwan's, the major political methods for achieving these goals at the local level involved reliance on small family, larger lineage, and in-law relationships. If these proved insufficient, reliance was also placed on extra-familial vertical (patron-client) and horizontal mutual exchange relationships. For Taiwan, these extra-familial relationships were often manifested by village groups encompassing some or all members of a single village. Manifestations involving some or all members of a single village in intervillage relationships were also known.[11]

In Taiwan, as in Chinese society as a whole, the character of the relationships that the individual relied upon during the traditional period was often founded on the ideal model of the family in combination with the actual necessities of bartering for crucial values.[12] Underlying these relationships was the necessity to satisfy felt-needs of the individual or projected needs of units that the individual identified with (e.g., a family's need for prestige and perpetuation). Individuals learned through the socialization process that one of the main methods for satisfying needs was by the manipulation of human relationships. But they also learned that coldly to calculate human relationships for personal advantage was wrong. Therefore, in Taiwan, mainland China, and in traditional societies in general, the individual was taught to clothe such manipulations in terminology associated with the trust and warmth of ideal family relationships.[13] Partially for this reason, in traditional societies such as Taiwan's the instrumental coercive elements of protective relationships were heavily overlaid by feelings of voluntarism and affection. Patterns of deference to authority were, therefore, very strong.[14]

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[11] For discussions of ordinary individuals and the social structures they have relied upon for goal achievement in traditional Chinese society, see Marion J. Levy, Jr., "Contrasting Factors in the Modernization of China and Japan," *Economic Development and Cultural Change*, 2 (1953), 165–66; Pye, p. 180; Morton H. Fried, *Fabric of Chinese Society* (New York: Praeger, 1953), throughout, but esp. pp. 123, 214, 218–22; and C. K. Yang, *A Chinese Village in Early Communist Transition* (Cambridge, Mass.: MIT Press, 1959), pp. 99, 109–18.

The same subject is discussed for traditional Taiwanese society by Gallin, in "Political Factionalism," pp. 380–81, 386–88; Martin M. C. Yang, *Socio-Economic Results of Land Reform in Taiwan* (Honolulu: East-West Center Press, 1970), pp. 428–44; Pasternak, esp. pp. 144–46; and David K. Jordan, *Gods, Ghosts, and Ancestors: Folk Religion in a Taiwanese Village* (Berkeley: University of California Press, 1972), esp. pp. 12–26.

For general discussions of ordinary individuals and the social structures they have relied upon for goal achievement in traditional society, see James C. Scott, "Patron-Client Politics and Political Change in Southeast Asia," *American Political Science Review*, 66 (March 1972), 91–113; and Eric R. Wolf, *Peasants* (Englewood Cliffs, N.J.: Prentice-Hall, 1966), esp. pp. 77–95.

[12] That this was often the case in Taiwan is noted by Yang, *Socio-Economic Results*, pp. 420–21. That this was often, but not always, the case in Chinese society in general is noted by Fried, pp. 136, 142, 151, 218, 220; and by Levy, "Modernization of China and Japan," pp. 165–66.

[13] Levy, "Modernization of China and Japan," p. 166; Pye, pp. 172, 180–81; and Richard H. Solomon, "Mao's Effort to Reintegrate the Chinese Polity: Problems of Authority and Conflict in Chinese Social Process," in *Chinese Communist Politics in Action*, ed. A. Doak Barnett (Seattle: University of Washington Press, 1969), p. 301. Specifically in relationship to use of kinship terminology on Taiwan, see Yang, *Socio-Economic Results*, pp. 436–37. For a discussion of the use of kinship terminology as a general phenomenon not limited to Chinese society, see Scott, "Patron-Client Politics," p. 95.

In these traditional societies, the small family or larger lineage was occasionally strong enough to insure individuals an adequate supply of values, and it was not necessary to go beyond these units for protection. Protective ties that did go beyond the family often displayed the additional characteristics of being "based on individual ties to a leader rather than on shared characteristics or horizontal ties among followers."[15] These leaders based their behavior on traditional moral and normative rules.[16] They held power because of a personally controlled local resource base, such as land, lineage position, and/or a locally based government office.[17] Given such resource control, they were generally accepted as the natural representatives of a constituency. Often they monopolized the resources of their area, and persons tied to them as clients had individually little power with which to bargain with them.[18]

However, during the traditional period patrons were somewhat dependent on the good will of their clients in terms of their individual actions, and were virtually forced to give up some of their wealth to clients in return for prestige in their eyes, for fear that clients' collective actions would threaten a patron's physical existence.[19]

Given this balance of power and the affective elements noted above the patron-client tie was in essence an "instrumental friendship," based on an unbalanced reciprocal exchange of values. It follows, therefore, that these ties were, in James C. Scott's terms, persistent, face to face, personal relationships dealing in terms of the needs of the whole person in a diffuse, multiplex fashion.[20]

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[14] According to James C. Scott, "material, particularistic inducements to cooperation play a minor role" during the traditional period, "except among a limited number of local power-holders." See James C. Scott, "Corruption, Machine Politics, and Political Change," *American Political Science Review*, 63 (December 1969), 1146. My contention is that even during this period, major inducements were of both the material and nonmaterial kind. Evidence for my contention is available in Levy, "Modernization of China and Japan," pp. 164–66, 171, 172, 176; Marion J. Levy, Jr., *The Family Revolution in Modern China* (Cambridge, Mass.: Harvard University Press, 1949), pp. 241, 242, 245; and Yang, *Socio-Economic Results*, pp. 437–44.

[15] Scott, "Patron-Client Politics," p. 97.

[16] Gallin, "Political Factionalism," p. 386.

[17] In the Chinese case, nationally appointed government positions were also an important resource through which power came to local families. On this point, see Ch'u T'ung-tsu, *Local Government in China Under the Ch'ing* (Stanford, Calif.: Stanford University Press, 1962), pp. 168–92. On the general point of local resource bases, see Scott, "Patron-Client Politics," pp. 97–98.

[18] Scott, "Patron-Client Politics," pp. 93, 96, 99–100. Specifically in terms of Taiwan, see Yang, *Socio-Economic Results*, pp. 420–22, 437–44, 476–80; Pasternak, *Kinship and Community*, p. 57; and Gallin, "Political Factionalism," pp. 379–80, 384–85. According to these sources, heads of large lineages and landlords were the most typical patrons during the traditional period on Taiwan. For reasons why the landlord-tenant relationship may be regarded "as the prototype of patron-client ties," see Scott, "Patron-Client Politics," p. 93.

[19] Yang, *Socio-Economic Results*, pp. 423, 439–44; Scott, "Patron-Client Politics," pp. 93, 108; and Fried, pp. 104–05, 195–97, 219–21, 224, all argue against the thesis, so strongly stated in Communist Chinese publications as well as in Western scholarly works, that in traditional societies "peasants only give and cannot control what they give or demand anything in return and aristocrats only take and have no reason to return anything of what they take. . . . [They expect] nothing from them and [do] not feel that [they have] rights or duties toward them." [From John H. Kautsky, *The Political Consequences of Modernization* (New York: Wiley, 1972), pp. 27, 35.]

Of course, where patrons concentrated in walled towns or where they were so well endowed with material resources and personal relations that they were able to raise their own military forces, or where modern state power brought police protection into the countryside, the patrons were freed from the threat of their clients' collective physical force. (For example, see Fried, pp. 195–98, 224.)

The appearance of police protection in the countryside—which was often accompanied by support by the police for the patrons as a stabilizing force in the society—was a common occurrence during the colonial period for many colonized societies throughout the world. As a consequence, Scott argues that the weakening of the physical threat which the clients were able to bring against their patrons caused a great strengthening of the patrons' power and a weakening of their motivation to be of service to their clients.

That the position of traditional patrons was in this manner strengthened in Taiwan during the Japanese period (although no reduction in service motivation is indicated) is argued by Bernard Gallin in "Land Reform in Taiwan: Its Effect on Rural Social Organization and Leadership," *Human Organization*, 22 (Summer 1963), 111.

Evidence that before the Japanese period fear of the collective actions of clients was a factor in convincing patrons to be of some service to their clients on Taiwan is based partially on the general Chinese experience of the possibility of peasant rebellion in areas in which landlords lived in the countryside far from the immediate protection of their own or government police forces. For this evidence, see Hsiao, pp. 426–31, and Fried, pp. 195–96, 219–21, 224. Specifically in terms of Taiwan, evidence that at least some of Taiwan's patrons lived under similar conditions before the Japanese period is found in Lawrence W. Crissman, "Each for His Own: Taiwanese Political Response to KMT Local Administration" (paper prepared for the London-Cornell Project for East and Southeast Asian Studies, June, 1969), pp. 19–20; Johanna Menzel Meskill, "The Lins of Wufeng: The Rise of a Taiwanese Gentry Family," in *Taiwan: Studies in Chinese Local History*, ed. Leonard H. D. Gordon (New York: Columbia University Press, 1970), p. 7; and Pasternak, *Kinship and Community*, pp. 116–20.

Social Change and Its Effects on Goal Attainment

A number of forces impinged on Taiwan's traditional society after 1945, causing drastic change. Below, three of these forces—economic development, land reform, and the institution of elections—will be discussed.

Economic Development.[21] At least since 1945, and increasingly each year thereafter, economic development has had an important influence on Taiwan's society. Other economic changes, especially "very rapid population growth and accompanying scarcity of land," have also been important.[22] The following discussion will be limited to describing economic development on Taiwan as it is related to the rise of new interest groups and to the widening geographical area of the ordinary individual's economic activity.

Here it may be useful to remind the reader that such limitations as these are made to restrict the scope of this article to the main points under discussion and keep the text at a manageable length. It also may be useful to remind the reader that most aspects of the following description of economic development and its effects on goal attainment in Taiwan are congruent with the experience which other societies have had concerning the development of the "mass political culture."

The Rise of New Interest Groups. Economic development began to stimulate the appearance of groups with interests in new areas of activity. With the institution of elections in 1946 these new groups began to seek out or send out politicians, in order to insure their protection amidst the vagaries of both the market and the many decisions being made by the burgeoning bureaucracy, which was expanding along with the growing economy.

Such groups often consisted of one person and all those dependent on that person's enterprises. These people not only sought out politicians or became politicians themselves for protection of their enterprises, but often used their enterprises as a political base for further economic and/or political aggrandizement.[23]

Whatever the course of action taken, new political interests and political bases were gradually becoming a part of the polity and competing with or supporting older politicians, patrons, village groups, lineages, and small families.

Competition was for more than economic and political power. The new political entities were also an alternative source of status and deference to the older units. Successful competitors, upon attaining economic and political power, were almost automatically granted status in recognition of their success.[24] Moreover, during the competition new political entities often vanquished those whose status was based on older arrangements.[25] Whether as alternatives or as direct competitors, a threat to traditional patterns of status and deference as well as to traditional patterns of power arrangements thus appeared.

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[20] Scott describes the patron-client relationship as an "instrumental friendship" in "Patron-Client Politics," p. 92. He explains his use of this term on pp. 92–95 and 106–07. According to Pye and Solomon, aspects of Chinese political culture which closely fit in with Scott's model of the patron-client relationship as an "instrumental friendship" include: (a) manipulation of human relationships as the chief means of satisfying felt-needs, (b) necessity to disguise such manipulation with feelings of trust and warmth, (c) strongly felt needs for order and security, seen as satisfiable only within hierarchically organized personal relationships, (d) personal relationship hierarchies seen as functioning successfully only when they are headed by a single authority figure. See Solomon, "Mao's Effort to Reintegrate the Chinese Polity," pp. 298–306; Pye, pp. 103, 172, 180–81; and Arthur J. Lerman, "Political, Traditional, and Modern Economic Groups, and the Taiwan Provincial Assembly," (Ph.D. dissertation, Princeton University, 1972), pp. 267–75.

Evidence that the model of patron-client relations as an instrumental friendship is applicable to landlord-tenant relationships in traditional China can be found in Hsiao, pp. 427–28. Evidence that the model is applicable to traditional Taiwan is available in Yang, *Socio-Economic Results,* pp. 438–42, 460–65.

I recognize that there are scholars who argue in favor of using other terms in place of "instrumental friendship." See Fried, p. 103; and Eric R. Wolf, "Kinship, Friendship, and Patron-Client Relations," in *The Social Anthropology of Complex Societies,* ed. Michael Banton (New York: Praeger, 1966), pp. 10–13, 16.

[21] In addition to interviews, the major source for this discussion of the effects of economic development on goal attainment in Taiwan is Gallin, "Political Factionalism," esp. pp. 380–81, 399. Also see Yang, *Socio-Economic Results,* pp. 455, 457. For a discussion of the effects of economic development on goal attainment in other societies, see Scott, "Patron-Client Politics," pp. 103, 105–08.

[22] Gallin, "Political Factionalism," p. 380.

[23] For a detailed discussion of my characterization of new interest groups which "seek out or send out politicians" see Lerman, pp. 345–80.

[24] Gallin, "Political Factionalism," p. 386, argues that prestige did not automatically come with the attainment of real power. Prestige still had to be earned through traditional respectable behavior. Crissman, p. 26, Yang, *Socio-Economic Results,* p. 485, and my own research indicates that the process is more automatic.

For the ordinary individual, the rise of these new interest groups had an unsettling effect. In the first place, their power was so great that the traditionally protective patron, village group, lineage, and/or small family could not protect the individual from their aggressions.[26]

Secondly, specifically in regard to the village group and lineage, their power was not only deteriorating as a result of attack from these externally based interest groups; the rise of new interests meant that the interests of the individuals within the village group and lineage were becoming more varied. This loss of solidarity was an internal force which caused a marked deterioration in both the ability and willingness of the village group and lineage to guarantee the security, wealth, and status of the individual.[27]

Thirdly, given the increasing number of groups and the increasing amount of competition between them, the original fears of the ordinary Chinese concerning unpredictability and chaos were also increasing.

Widening Area of Economic Activity. A related result of economic development was the widening of the geographic area relevant to each individual's and small family's economic livelihood. No longer was it enough to have good relations with neighbors and kin living within the confines of a small market town and its satellite villages. Products originating from all over Taiwan and even from overseas had now to be purchased. In turn, the individual's own markets were being heavily influenced by far away forces. New "quasi-official" economic organizations were also influencing individuals from beyond their local areas.[28] As a result, the individual and small family came to be increasingly involved with many individuals in terms of very specific transactions and decreasingly involved with only a few individuals in terms of comprehensive, whole person relationships.

Under these conditions, the relatively small village group and lineage no longer had sufficient power to protect individuals and their families, even if these units had remained united and unthreatened by powerful new interest groups. Nor could the traditional patrons with their locally based resources be of much aid. Indeed, the possibility of maintaining any "whole person" patron-client relationship, whether based on traditional and/or nontraditional resources, was becoming increasingly difficult.

Moreover, even if the increasing differentiation in interests were not considered, the larger number of people now in contact with each other and the wider geographic area over which they were spread gave rise to a looser, more complex, less controllable situation. Thus, another cause for the deepening of ingrained fears of conflict and unpredictability appeared.

Land Reform.[29] Taiwan is one of the only developing countries which has successfully instituted an effective land reform program. Therefore, unlike economic development and the institution of elections which have made comparable contributions to the emergence of the mass political culture in both Taiwan and other developing countries, the contribution of land reform to the emergence of the mass political culture is more specific to Taiwan.

Land reform on Taiwan contributed heavily to the rupturing of traditional power and status arrangements in the countryside. Previously it was the landlord, or those members of the lineage who controlled the lineage lands, who had power and status in the countryside. The individual and small family rented land, borrowed money, and/or in other ways received services from these personages in return for whatever services they themselves could offer.

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[25] Gallin, "Political Factionalism," pp. 386–87; Bernard Gallin, "Rural Development in Taiwan: The Role of the Government," *Rural Sociology*, 29 (September 1964), 318; and Yang, *Socio-Economic Results*, pp. 487–500.

[26] Gallin, "Political Factionalism," pp. 394, 396, 398–99.

[27] Note that solidarity is still manifested by many village and lineage organizations on Taiwan. What we are discussing is a gradual tendency toward breakdown, not a completed process. For fuller descriptions of this process, see Gallin, "Political Factionalism," pp. 387–88, 392, 394–97; Pasternak, *Kinship and Community*, pp. 72–78, 86–113, 116, 120–27; and Yang, *Socio-Economic Results*, pp. 455, 457.

[28] Gallin, "Political Factionalism," p. 381, and Gallin, "Rural Development in Taiwan," p. 314.

[29] In addition to interviews, the major sources for this discussion of the effects of land reform on goal attainment in Taiwan are Gallin, "Political Factionalism," pp. 380–87, "Land Reform in Taiwan," pp. 109–12, "Rural Development in Taiwan," pp. 314–18; Yang, *Socio-Economic Results*, pp. 230–59, 458–66, 480–505; and Pasternak, *Kinship and Community*, pp. 25, 100, 115.

With the end of large landholding in 1953, landlords and lineage leaders no longer had a strong material base with which to serve clients.

For this same reason, landlords and lineage heads now had much less of a material power base with which to sustain themselves in leading positions in the community. As will be seen below, reliance on their traditional prestige along with the remainder of their original material power base was inadequate for this purpose. Instead, landlords and lineage heads had to work hard to find new material power bases, or they had to accept the role of rank and file citizen.

As for ordinary peasants, in spite of the land that they received as a result of land reform the loss of the services of the landlords and lineage heads was another factor increasing their feelings of insecurity.

Institution of Elections. Perhaps the greatest influence on Taiwan's society has come from the institution of elections for local public offices. In conjunction with the influences already discussed above, as well as independently, elections have caused many changes in the world of the local politician.

The most significant aspects of the elections in terms of political change were: (a) virtually any adult could run for office; (b) virtually every adult could vote; (c) the winners obtained real power at any one of a number of levels of government;[30] (d) along with winning came great prestige; and (e) many important elections were held in districts much larger than the village community.

The Institution of Elections and the Growth of Conflict. It has already been demonstrated that because of larger areas of economic activity and an increase in the kinds of interests among the people, conflict was growing. As in other developing countries which instituted elections where anyone could run and with election districts including large areas containing many more voters than that of the small village community, possibilities for conflict in Taiwan were further increased. Moreover, the power available to the winner of an election encouraged those already involved in conflict to seek after that power as a necessary resource. Even those not yet involved in conflict were motivated to join the competition for a share as a hedge against future needs, if not for immediate advantage.

Since anyone could run and since the material rewards could be great, even individuals without an original economic base could borrow money in the hope of a large return for their investment. Those who began with resources, obtained either by selling the bonds received in land reform or by relying on the economic resources and manpower of their new enterprises, were, of course, in a better position.[31]

The Institution of Elections and Taiwan's Former Landlords. Because Taiwan had undergone an effective land reform, Taiwan's landlords were in a different situation from their counterparts in countries which are otherwise comparable to Taiwan. Faced with the developing electoral system, each of Taiwan's former landlords displayed one of two responses. The first response was to withdraw from competition for a local leadership position. Those who chose this response did so for one or more of the following reasons: (a) financial inability to compete, (b) unwillingness to take the necessary financial risks, (c) rejection of the extremely opportunistic behavior increasingly necessary for electoral success, (d) diversion of interests into farming or nonfarm business, and (e) diversion of interests away from the local community.[32]

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[30] That many posts at many levels have real power is testified to by Gallin, "Political Factionalism," p. 395; Crissman, pp. 8, 30, 37–38, 42, 44, 47, 54, 57; Yang, *Socio-Economic Results*, pp. 408–11, 490–91; "Chueh Pu Jung Chu 'Hsuan' Hai Kung," in *Lien-ho Pao* (Chia-nan Ti-fang-pan), January 12, 1968, p. 7; and *T'ai-wan Sheng I-hui Ti-san-chieh Ti-pa-tz'u Ta-hui I-yuan Chih-hsun yu Pen Fu Ta-an Hui-pien* (Chung Hsing Hsin Ts'un, T'ai-wan: T'ai-wan Sheng Cheng-fu Mi-shu-ch'u, 1967), p. 179.

[31] For the institution of elections and the growth of conflict on Taiwan, see Gallin, "Political Factionalism," pp. 386–88, 391; Ch'en Ming-t'ieh, "Ch'e-ti Hsiao-ch'u Hsuan-chu P'ai-hsi Fen-cheng," *Ti-fang Tzu-chih*, no. 176 (February 15, 1968), p. 15. Pao, p. 69; Liu, pp. 33–34; *T'ai-wan Sheng I-hui Ti-san-chieh Ti-pa-tz'u Ta-hui I-yuan Chih-hsun yu Pen Fu Ta-an Hui-pien*, p. 179. For comparable phenomena in India and Southeast Asia, see Weiner, pp. 208–13 and Scott, "Patron-Client Politics," pp. 109–11. Note that material rewards are likely to be great only for higher level elected office. Motivation for lower level office is usually based on the quest for prestige. See Gallin, "Political Factionalism," p. 386, and Crissman, p. 40.

[32] Yang, *Socio-Economic Results*, concludes that "land reform removed most of the large landlords from rural communities and into businesses or politics. Some did stay in rural towns but changed their occupations. Small landlords had, in most cases, become real owner-farmers" (p. 497; also see p. 362). I am not sure that Yang's evidence permits us to make such a definitive statement.

Evidence concerning landlords who withdrew from local leadership competition is found in Yang, *Socio-Economic Results*, pp. 230–60, and in Gallin, "Political Factionalism," pp. 386–87, and Gallin, "Rural Development in Taiwan," pp. 316–18.

Note that the choice to stay out of local leadership competition was not irrevocable. This was especially true for former landlords who became successful in nonfarm businesses. Nevertheless, both Gallin and Yang offer evidence to suggest that the number of former landlords who choose to stay out of local political competition is growing, not diminishing.[33]

Many former landlords, however, chose a second response—doing everything in their power to compete for local leadership positions. And while some went bankrupt for their efforts, evidence indicates that many were successful, especially in the early years of the electoral system.[34] Just as, or more, motivated than anyone else to obtain the rewards now being offered by the new electoral system, many former landlords managed to successfully establish a new political base.

The Institution of Elections and Status Arrangements. One important cause of many former landlords staying in politics arose from the rupture of traditional status arrangements and deference patterns noted earlier. Traditional status arrangements, buffeted by economic development and land reform, were receiving their coup de grace from politicians using "nontraditional" forms of political behavior. These politicians, many from humble origins, were using "nontraditional" forms to nudge aside those traditional leaders who were not adapting their behavior to the new political system.[35]

Since other arrangements were in decline, elections, with their clear proof of popular esteem, became one of the few methods available for affirming, or reaffirming, one's reputation. As had happened in many other developing countries during the rupture of old status arrangements, it became difficult to clearly discern who the status elites really were. And since status remained highly valued, both to previous and potential power holders alike, elections were seized upon as the new proof that one was indeed of high status.[36]

The Institution of Elections and the Plight of the Ordinary Individual. Given the above noted social changes, the situation of the ordinary individual in Taiwan, as in similar developing countries, was becoming increasingly difficult. The individual was faced with unprecedented problems of schooling, employment, markets, and health care, as well as a general need for security from the chaos which an increasingly conflictive environment seemed to portend. At the same time, the traditionally protective lineage and village groups were becoming too weak and/or disunited to be effective. Traditional patrons, depending on what remained of their locally based resources, were also unable to offer meaningful protection.[37]

The alternative of organizing with peers in horizontal pressure groups also did not offer much hope. A number of reasons may be cited for this:

1. In traditional societies there had been much conflict and suspicion at the local level, especially between ethnic groups, between villages, and between lineages within villages. Intergroup relations in mainland China and in Taiwan, for example, often took the form of international relations rather than relations between civic organizations of the same state.[38]

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[33] Yang, *Socio-Economic Results*, pp. 492–94, has figures which indicate a possible slow decline in the rate of landlord participation and/or success in competition for local leadership positions over time. If these indications are correct, the decline may be attributable to the aging of the landlords as a group and not to the other causes noted above. The decline may also be attributable to the weakening of traditional status as an electoral asset as time goes by. On this latter point, see Yang, *Socio-Economic Results*, pp. 480, 484, 501, and Gallin, "Rural Development in Taiwan," pp. 317–18. On the point of a decline in landlord political participation over time see also Gallin, "Political Factionalism," pp. 386–87, and "Rural Development in Taiwan," pp. 316–18.

[34] Early landlord success in local leadership competition is also indicated by Yang's figures in *Socio-Economic Results*, pp. 492–94. A generally high rate of landlord participation and success in politics is indicated by Yang, pp. 483–86, 488, 494. Gallin, in "Political Factionalism," p. 389, and in "Rural Development in Taiwan," pp. 317–18, also offers evidence on these points.

[35] That many new politicians were from humble (i.e., not traditional leadership) origins is indicated by the evidence of Gallin, "Political Factionalism," pp. 386–87, and Gallin, "Rural Development in Taiwan," p. 318; Yang, *Socio-Economic Results*, pp. 487–88, 492–98, 503; and my own interviews.

[36] The importance of elections "when other criteria for status become fuzzy" is stated as a general proposition by Weiner, pp. 208–09. For manifestations of this phenomenon on Taiwan, see Yang, *Socio-Economic Results*, p. 485; and Crissman, p. 26.

[37] Scott, "Patron-Client Politics," pp. 103–04. In terms of Taiwan, see Gallin, "Political Factionalism," pp. 378, 380–81, 391, 394–95, 399; Crissman, pp. 30–31; and Yang, *Socio-Economic Results*, p. 475.

[38] This was especially true of southeastern China, the area in which most of Taiwan's society originated. See Maurice Freedman, *Chinese Lineage and Society: Fukien and Kwangtung* (London: Athlone, 1966), pp. 104–17; C. K. Yang, *A Chinese Village*, pp. 109–17; and Hsiao, pp. 361–70, 419–27. Open conflict also

2. If the recent speculation of such scholars as Pye and Solomon concerning Chinese personality is sound, a reason for nonreliance on horizontal peer organization specific to Taiwan would be the individual Chinese's lack of faith in cooperation between peers and tendency to see cooperation as possible only in terms of hierarchically organized personal relationships led by social superiors.

3. In terms of transitional societies in general, if traditional forms of hierarchical relationships (especially the patron-client form, though with a new breed of patrons) could adequately service the felt-needs of the individual, they would continue to prosper. The "naturalness" and lack of strain felt in continuing to use traditional forms of hierarchical relationships, prejudice in favor of these relationships, prejudice against horizontal peer group relationships, and lack of knowledge about many kinds of horizontal peer group relationships, all would act to keep traditional forms of hierarchical relationships prospering.

4. Previously united villages and lineages—two hierarchically organized groups with potential for transformation into more equalitarian organizations—were already breaking up along the lines of newly developing economic interests.

5. The individual feared that there were not enough resources to be adequately distributed among potential members of a horizontal organization.

6. In spite of the above reasons, even if individuals did attempt to organize their peers into horizontal pressure groups, the national elite on Taiwan—like its counterparts in many other developing countries—would not allow such independent organizations, dedicated to the desires of their members, to become powerful. That would be too threatening to the national elite's own power. Instead, the national elite actively discouraged spontaneous organizing and sponsored its own organizations, encouraging individuals to join. But these organizations dedicated to furthering the desires of the elite and not the desires of the ordinary members were not seen as aids in solving the problems of the individual.[39]

Reliance on formal government institutions was another solution which did not appeal to the ordinary individual. The reason for its lack of appeal is also relevant to the lack of reliance on horizontally organized groups. In Taiwan, as in many developing societies, the individual has been socialized not to trust impersonal institutions. Security, the individual has learned, comes from dependence on personal relations. Therefore, ordinary individuals avoided reliance on impersonal government programs.

Even if individuals were able to bring themselves to depend on impersonal programs, moreover, problems remained. Those who ran these programs, influenced by the same socialization concerning impersonal and personal relationships, acted in such a manner as to undermine their impersonal quality.[40]

The Institution of Elections and Security for the Ordinary Individual. Given this inability to rely on horizontal peer organization or on impersonal governmental programs, individuals in Taiwan, paralleling the response of individuals in comparable situations in other developing countries, increasingly conceived of their needs in terms of their own particular problems unrelated to the problems of those outside their immediate circle. Consequently, individuals began making demands with an eye to causing particular power holders to act on behalf of the individuals themselves, members of their families, or on behalf of others close to them.[41]

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manifested itself on Taiwan, especially before the establishment of firm police control in the countryside. In this regard, see Meskill, pp. 7, 11–12; Jordan, pp. 9, 20–26, 42, 48–51, 86, 135–37, 163; and Burton Pasternak, "The Role of the Frontier in Chinese Lineage Development," *The Journal of Asian Studies*, 28 (May 1969), 554–55, 559–61. Indications that underlying currents of these old conflicts did not disappear with the imposition of police control are found in Crissman, pp. 20–23; Bernard Gallin, "A Case for Intervention in the Field," *Practical Anthropology*, 10 (March-April 1963), 58; Jordan, pp. 22, 48–49, 135–37; 163–64; and Pasternak, *Kinship and Community*, pp. 103–05.

[39] For the national elite's attitude toward nongovernmental organizations, see statements in *T'ai-wan Sheng Ti-faug Tzu-chih Chih-yao*, pp. 896, 872–73, 876, 879, 891.

[40] See n. 13 and 20 above. Also see Scott, "Patron-Client Politics," pp. 102–04, and Bernard Gallin and Rita S. Gallin, "Sociopolitical Power and Sworn Brother Groups in Chinese Society: A Taiwanese Case," in *The Anthropology of Power*, ed. R. N. Adams and R. D. Fogelson (New York: Academic Press, 1977), p. 95.

Note that the impersonal administration of the land reform program and the strict adherence to a merit system in the treatment of students on all levels of schooling in Taiwan are two important exceptions to this rule.

[41] "Historically, the expansion of the suffrage together with the rupture of traditional economic and status arrangements have signalled the rise of particularistic, material inducements" (p. 1148). "The sense of community was especially weak, and . . . social fragmentation made particularistic ties virtually the

Fortunately for the individual, social change had provided new resources which made these demands more effective. Given the rupture of traditional status arrangements and deference patterns, for example, it was now difficult to maintain the support of an individual by heavy reliance on the feelings of voluntarism and affection which traditionally supported patron-client relations. Greater material reciprocity was necessary. More crucial, however, was that the individual now had something that people of power and potential power needed very badly—a vote.

In earlier times, it was often the distance of the rich from the immediate protection of police power which constrained them to accept the ideology that the wealthy should serve the people in return for a good name. Now it is the value of the vote which motivates politicians to aid as many individuals as possible in expending their resources to win election. Moreover, since there are a large number of competitors for their votes, individuals can often shop around for the politician with access to the economic, social, and political resources most relevant to their needs.

As a result, candidates in Taiwan, like candidates in similar developing countries, are placed under great pressure to establish, either personally or through intermediaries, as many personal relationships as possible with potential voters. Within the context of each relationship, candidates must offer the voter an adequate number of particular favors, from whatever resources—public or private—at their command. At least in Taiwan (if the Pye-Solomon speculation is valid), these favors must serve not only the voter's material needs, but also such nonmaterial needs as the provision of a figure of stability in a world of potential chaos. It is to the candidate who offers the most advantageous mix of needed services that ordinary individuals give their votes.[42]

The Institution of Elections and the "Unsavory" Appearance of Local Politics. Since politicians must service voters individually in order to establish the necessary personal relationships on which reciprocity is based, their services frequently take the form of bending or breaking general rules on behalf of the voter. But because these general rules are usually legally binding, it follows that a politician's services often involve activities which are corrupt, according to the strict definition of the term.[43]

In other cases, while politicians' services may not be legally corrupt, their need to attract votes on a particularistic basis will often cause them to act on behalf of a very small section of the community, disregarding other sections of the community or the community as a whole.[44]

In all cases, given the great competition between candidates and the necessity to obtain votes through personal relationships, candidates begin to gather around them as many voters as possible, and mutually opposed factions form. The growing competition of these factions—a competition which includes the exacerbation of the traditional and modern social divisions already mentioned above—creates the appearance of great divisiveness and lack of consensus at the local level.[45]

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only feasible means of cooperation" (p. 1150). In these two statements, Scott, "Corruption, Machine Politics, and Political Change," generalizes on the type of situation facing the ordinary individual in Taiwan. Evidence to indicate that this is the case for Taiwan is found in Lerman, pp. 387–401.

[42] In terms of the voters' material needs, see Scott, "Corruption, Machine Politics, and Political Change," pp. 1148, 1157–58, and Scott, "Patron-Client Politics," pp. 108–13. My statements concerning the voter's nonmaterial needs are based on the evidence noted in fns. 4 and 20. That politician-voter relationships on Taiwan are actually of this nature is indicated by Gallin, "Political Factionalism," pp. 391, 395; Crissman, pp. 30, 32, 37; and Lerman, pp. 203–13. Note that a number of politicians on Taiwan echoed the statement made to me by one provincial assemblyman that: "the people recognize money, not candidates." ("Jen-min jen ch'ien, pu jen jen.")

It should be noted, however, that two factors make it appear that traditional values still guide individuals when they shop for a politician. First, individuals are still likely to insist upon establishing "personal" relationships rather than treating their dealings with a politician as a coldly calculated transaction. Second, the individual uses traditional guides to determine which politician is likely to offer the most rewarding relationship. See n. 13 above and Crissman, p. 37.

[43] Scott, in "Corruption, Machine Politics, and Political Change," pp. 1143–45, states the general case. For specific examples in Taiwan, see "Hsin-ying Ko-chia Yin-hang, Shih-yuan Ch'ao-p'iao Huan-kuang," in *Lien-ho Pao* (Chia-nan Ti-fang-pan), January 21, 1968, p. 7; "Chuch Pu Jung Chu 'Hsuan' Hai King," in *Lien-ho Pao*, p. 7; Crissman, pp. 35–36; and Gallin, "Political Factionalism," p. 386.

[44] For examples of particularism on Taiwan, see Lerman, pp. 218–25, and Pao, pp. 67, 69. For similar examples in other societies, see Scott, "Corruption, Machine Politics, and Political Change," p. 1144, and Weiner, pp. 209–13, 220.

[45] Often there is a consensus within which the competition is taking place, but this is not necessarily apparent to the outsider. On this point see Andrew J. Nathan, "A Factionalism Model for CCP Politics," *The China Quarterly*, 53 (January-March 1973), 46, 50–51, and Lerman, pp. 300, 309–10. Sources for the rise of factionalism are in fn. 31.

Moreover, with the desires of all those in local politics centered on personal security, wealth, and status, causing voters and politicians alike to form and break up relationships solely on the basis of individual interest, lack of principle joins corruption, particularism, and divisiveness on the list of "unsavory" appearances at the local level.[46]

The Institution of Elections and the National Elite. Even the national elite is forced to participate in this system. The national elite must insure that a great majority of the candidates who win office are identified with its party. Otherwise, it would lose prestige which means so much in terms of a continuation of legitimacy.

In order to win the national elite must find candidates who, at a minimum, are already enough a part of their local societies so that the voters can conceive of establishing personal relationships with them. Usually, Taiwan's national elite–like the national elites in similar developing countries–is faced with individuals in the various election districts who are so well endowed with personal relationships and other resources that the national elite must work with them. Attempts to impose candidates from the outside, in disregard of the will of these local notables can only lead to defeat for the national political party.[47]

Taiwan's national elite is not completely at the mercy of the local notables, however. Firstly, the local notables are disunited, since they are the product of the conflict between factions and between other social divisions discussed above. Secondly, the national elite, through its willingness and ability to manipulate and bend electoral rules, its ability to revalue virtually all electorally meaningful resources–public or private–through governmental action, its control of all non-word-of-mouth information about candidates which the public receives, its active discouragement of any efforts to seriously organize an opposition party, and its control of a loyal block of votes in many election districts, can have a great influence on the outcome of elections.

In response to the elite's great influence over elections, Taiwan's local level factions have most of their active members join the elite's political party. Only in the event that the elite's party nomination does not go to one's own faction and no adequate compensation is provided, will the non-nominated faction run a serious campaign in opposition.[48]

It appears that an additional elite tactic in Taiwan is to insure that at least two viable factions remain in competition in each election district. In order to attain this end, the elite parcels out nominations and other resources to competing factions accordingly. What the elite seems to fear is that, if one faction became dominant, the whole area perhaps allying itself with other united areas might begin to make demands on the national elite, instead of expending its energies in local factional competition. Except for an occasional failure, the result of this policy has been a two-faction system in many of Taiwan's election districts.[49]

As a whole, Taiwan's political system appears somewhat coercive, but in other respects resembles a boss machine of the type found in noncoercive electoral systems. The national elite acts as the boss, using its control of resources to induce most potentially threatening vote-getters to join its machine.

This boss machine sits on top of the local factional systems. The boss keeps the bought-off local level politicians divided by fostering a balance of power between rival faction leaders. Each leader is kept satisfied, but no leader is allowed to control resources to the extent of driving rival factions out of existence.

Note that in terms of this nationally based boss-machine system, the familiar distributive requirements still prevail. It is not simply a matter of buying off the faction leader. Faction leaders must be assured of enough resources to pass down to their lieutenants, who in turn pass them down, until voters receive enough to be convinced that it is in their interest to support the entire system by voting.[50]

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[46] James C. Scott argues that this concentration on individual self-interest may be a blessing in disguise. In a divided polity with few other bases of consensus, self-interest can serve as the one motive with which to bind otherwise opposed individuals together. See Scott, "Corruption, Machine Politics, and Political Change," pp. 1151–54. Weiner, pp. 212, 242, and Lerman, p. 310, make the same point. For an example of lack of principled behavior by Taiwan's local politicians, see "Man T'an Cheng-chih yu Tao-te," in *Min-sheng Jih-pao,* January 24, 1968, p. 2. For similar behavior in India, see Weiner, p. 212.

[47] Scott, "Patron-Client Politics," p. 110, and "Corruption, Machine Politics, and Political Change," p. 1154. For examples of the national political party going down to defeat in Taiwan, see articles in *Tzu Chih,* no. Ko Hsin 181 (May 1, 1968), pp. 4, 5, 10.

[48] For an example of this strategy on Taiwan, see Chao Chun-wu, "Miao-li Hsien Chang Hsuan-chu, Liu Huang Liang P'ai Ta Chueh-chan," *Liao-wang Chou-k'an,* no. 31 (October 14, 1967), p. 26.

[49] Crissman, pp. 33, 38, and Douglas Mendel, *The Politics of Formosan Nationalism* (Berkeley: University of California Press, 1970), p. 95. For a general statement describing the elite's influence over elections and the response to this influence by local level factions on Taiwan, see Gallin and Gallin, "Socio-political Power and Sworn Brother Groups," p. 95.

Conflict Between the Two Cultures

There are political scientists—as well as local politicians in Taiwan and elsewhere—who argue that the type of political system described above, with all its formal corruption and other unseemly characteristics, actually works well in terms of the needs of ordinary individuals whose society is in a transitional stage of political and economic development. Politicians in control of resources are under extremely heavy pressure to expend these resources in helping ordinary individuals in exactly the manner in which ordinary individuals demand.

With the patience and forbearance of the national elite, the political scientists add, this system will indeed develop into a model of democracy somewhat close to the original intentions of the national elite. The economy will make people feel more secure. People will develop trust in impersonal institutional guarantees. They will begin to see their interests in occupational and class terms, form effective organizations with which to pursue them, and abandon their unilateral attempts to gain security by making personal deals with a patron. At that point they will begin to pursue their interests in terms of support for national policies which affect all individuals in a given category in the same way.[51]

But it is difficult for a national elite such as Taiwan's to be patient and forbear. This is true in spite of the national elite's complicity in the system, as outlined above. Perhaps this complicity, forced on the national elite by the nature of the local political system and tarnishing its superior image of itself, is an additional reason why the national elite cannot be patient and forbear.

Indeed, such a national elite would probably find it difficult to be patient and forbear, even if the local political system was exactly like the model of democracy described above as somewhat close to its original intentions. The "somewhat close" model is also deficient, since it suggests occupational and class organizations freely competing for their own conceptions of their interests, in disregard for the elite's conception of the good of the whole political body. How much more difficult is it, therefore, to accept the transitional system, with its stress on the self-centered particularistic needs of individuals, small families, and small localities for security, wealth, and prestige; its strong tendency to cater to these needs through the distribution of great amounts of materials and services according to unscientific, seemingly haphazard, political criteria; its stress on personal relations and personal favors; and its manifestation (at least in the mind of the national elite) in a chaos of factional conflict noted for opportunistic, anything-goes tactics.

All this is very distressing to an elite which conceives of democracy as a process which arrives at the best choices of policies and leaders through a rational discussion of alternatives and which results in rational, scientific allocations of resources according to plans based on what is best for the general welfare. This conception of democracy in combination with the elite's participation in "The Movement" leads the elite to expect citizens and politicians in a democracy to be motivated to do their duty and fulfill their obligations, with less stress on their privileges and rights. Selflessness and a willingness to sacrifice for the greater good are central to the elite's view of actually functioning democratic systems. For the elite, such standards plus the unity and order according to which the process should run will give the country both the moral and physical strength necessary to insure its security, wealth, and status among the nations of the world.

At this point it may be helpful to remind the reader that it is not necessary for the elite to live up to these standards in order to believe in them. The elite may consistently violate them. But the elite's image of itself and the measures it uses to judge other people are defined by these standards.

Given the elite's belief in these standards, it is faced with a great dilemma. It has learned that what is correct according to the scientific criteria of trained and experienced experts and according to the moral judgments of seasoned cadres (i.e., the experts and cadres within its own ranks) coincides with what is democratic. But its experience had been just the opposite. It can only draw the conclusion that something is very wrong.

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[50] For general descriptions of the boss machine model, see Scott, "Corruption, Machine Politics, and Political Change," pp. 1154–58, and Scott, "Patron-Client Politics," pp. 96, 110–12. I prefer not to refer to the local factions as boss machines, since, according to Scott's definition, the term *boss machine* "connotes the reliable and repetitive control it exercises within its jurisdiction" (Scott, "Corruption, Machine Politics, and Political Change," p. 1143). Most factions, however, appear to share control of their jurisdiction with at least one other faction.

[51] Scott, "Patron-Client Politics," pp. 108–12, Scott, "Corruption, Machine Politics, and Political Change," pp. 1144–45, 1157–58, and Weiner, pp. 241–44. That such developments may be taking place on Taiwan, especially among younger, educated voters, is indicated in "Hsuan-chu Chien-t'ao Tso-t'an-hui," *Ta-hsueh Tsa-chih*, no. 62 (February 1973), p. 18.

Perhaps the people are not yet mature enough for democracy. Perhaps the politicians and their factions are evil and are corrupting the electoral system.[52] Certainly the interest groups arising out of the new economy or reflecting the prejudices of small localities are too selfish.[53] And perhaps the electoral system itself, in bestowing power on such selfishness, is deficient.[54]

Fortunately, however—the elite is convinced—there is a force in society, its government, and party which understands what true democratic standards are. Moreover, this force is powerful enough to correct the evils of the local political system.[55] The tendency on Taiwan has been for the national elite to use its government and party to disallow "evil" activities and organizations, progressively restricting electoral activities until behavior conforms to the ideal. These restrictions, once established, are designed to remain in effect until the people mature.[56]

Unfortunately, the standards of the elite are so high that it is unlikely that the people will ever mature sufficiently.[57] It appears that not even a small number of imperfections are acceptable to the national elite as the price of an unrestricted electoral system.[58]

As for many local politicians—both in Taiwan and elsewhere—imperfect as they know the local electoral system to be, they are used to its imperfections, identifying them with normal human behavior and taking them all in their stride. Moreover, they feel that their actions, whether formally legal or not, have truly been earning the votes of the electorate. The national elite may complain that they are acting against the general will of the people, but they are in daily contact with many persons, if not "the people," and feel that they are in a much better position than the national elite to determine what the will of the people is. On rare occasions one or another of Taiwan's local politicians will put aside a fear of repression and give voice to these feelings, publicly opposing the legitimacy of the national elite with his or her own electoral legitimacy.[59]

Amelioration of the Conflict on Taiwan

In many countries, the above conflict has led to the national elite despairing of electoral politics altogether, and replacing elections with forms of democracy based on the national elite's "unique capacity" to rely on its own background and skills to determine what the will and needs of the people are. A number of constraints are likely to check the tendency of Taiwan's national elite to move in this direction.

With all its unseemliness, the local political system has brought tangible rewards to the voters of Taiwan. Local politicians have also benefited. Moreover, the system has tied the politicians close to the voters and has made them leaders of opinion and action. If the national elite tried to do away with this system, it would be very difficult to convince the voters and politicians that any substitute would be more in their interests.

The problem is compounded because the national elite is identified as a nonelected group which did not even originate in the majority community. For the elite to repress the real elected leaders of the majority community and claim that somehow it is a better representative than they would be very difficult for the majority to accept.

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[52] See n. 31 above.

[53] For examples of the elite's attitudes concerning interest groups, see Pao, pp. 67, 69, and Ho, p. 30.

[54] For examples of elite attitudes concerning the deficiency of the electoral system, see Liu, preface, p. 4, and pp. 1, 33–34, and Yang I-feng, p. 178.

[55] For examples of the national elite's faith in the government, see Pao, pp. 67–69, 107, and Liu, p. 34.

[56] For the elite's attitude on what should be done about the local political system, see Pao, pp. 50–52, 68–69, 74, 99, Yang I-feng, pp. 117–79, and Ch'en, p. 14.

[57] In terms of the over-high standards which the elite insists that democracy must live up to, see Liu, preface, pp. 1–4, and Pao, p. 98.

[58] In terms of the national elite's unwillingness to accept even a small number of imperfections in the electoral system (and for further examples of what the elite thinks should be done about the local political system), see Pao, pp. 52, 69, 74, 99, and Chien Ho, "Ch'i Kao I Chao," in *Chung-yang Jih-pao* (Hang-k'ung-pan), September 14, 1971, p. 4. For further examples of the Taiwan national elite's attitudes toward the local political system, see material introduced in fns. 5, 31, 43, 44, and 46 above. For the reactions of India's national elite to their local political system, see Weiner, pp. 228–44. For the reactions of other national elites to their local political systems, see Scott, "Corruption, Machine Politics, and Political Change," pp. 1155, 1157.

[59] For examples of the attitudes of local politicians, see *T'ai-wan Sheng I-hui Ti-san-chieh Ti-pa-tz'u Ta-hui Chuan-chi,* vol. 2, pp. 2525–26, 2534, 2569, 2574, 2579. These and other statements of local politicians are translated in Lerman, pp. 142–85. For the attitudes of India's local politicians, see Weiner, pp. 231, 241.

Constraints such as the above, however, have not discouraged other national elites from doing away with electoral systems. The reason why the feelings of the majority community and its leaders are so important for the Taiwan national elite stems from the recent widespread rejection of the elite by the world's other national elites along with their respective national communities. Taiwan's national elite could probably not survive if its own majority community joined this general withdrawal of legitimacy.

Indeed, one of the only remaining supporters of the Taiwan elite, the United States, is far more tolerant of imperfect democracy and would likely become less supportive of the Taiwan elite if the latter did away with its fledgling democratic system.

Thus, in spite of its distaste for the local political system, the Taiwan national elite can only deepen its own complicity in electoral politics, even committing itself to a slow expansion of the scope of elections into national politics if it is to have a chance at remaining the governing elite of Taiwan.[60]

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[60] It hardly seems necessary to add that this is exactly the direction in which the national elite appears to be moving (though with great hesitation). See, for example, J. Bruce Jacobs, "Taiwan 1972: Political Season," *Asian Survey*, 13 (January 1973), 106–07, and J. Bruce Jacobs, "Taiwan 1973: Consolidation of the Succession," *Asian Survey*, 14 (January 1974), 26–29.

The Socio-Economic Determinants of Popular-Authoritarian Electoral Behavior: The Case of Peronism*

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In several highly mobilized Third World societies, rising levels of working-class political activism seem to have encouraged the development of political movements which are both popular and authoritarian. This popular authoritarianism melds intensive political mobilization of previously excluded social sectors with political structures which severely limit these groups' ability to affect public policy. Much of the research on popular authoritarianism has attempted to explain the phenomenon by identifying the socioeconomic determinants of popular-authoritarian electoral behavior. In an effort to clarify the relative merit of contending explanations, this study uses data from the prototypic case of Argentine Peronism to test six common hypotheses and then to construct a model which optimizes the explanatory ability of five major socioeconomic variables. The results indicate that an area's rate of industrial growth and the size of its working-class population account for more than four-fifths of the variation in Peronist electoral behavior that can be attributed to socioeconomic variables.

Most of the earliest analyses of Peronism's electoral popularity by United States diplomats, journalists, and social scientists were distorted by an ideological predisposition to discover a fascist mind beneath the cap of South American military officers trained in prewar Germany or Italy. Possibly for this reason alone it was once considered acceptable to explain Juan Perón's popular success by noting his demogogic ability to hoodwink Argentina's rapidly expanding urban proletariat into exchanging its political freedom for evanescent economic and psychic gratifications.[1] Today, when an academic emphasis upon rational decision making often is combined with a renewed faith in the Third World working class's autonomous role as an agent of social change, a new generation of social scientists has attempted to redefine our understanding of the relationship between the Peronist movement and the masses it purports to represent.[2]

The rationale for such a major intellectual expenditure rests at least in part on the assumption that Peronism is prototypic of a prominent political phenomenon found in several Third World societies exhibiting advanced levels of social mobilization and highly differentiated roles and institutions. In these

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[1] Spruille Braden, *Diplomats and Demagogues: The Memoirs of Spruille Braden* (New Rochelle, N.Y.: Arlington, 1971), pp. 340, 356–58; Fleur Cowles, *Bloody Precedent* (New York: Random House, 1952), p. 149; Ray Josephs, *Argentine Diary: The Inside Story of the Coming of Fascism* (New York: Random House, 1944), pp. xi–xlvi; Robert J. Alexander, *The Perón Era* (New York: Columbia University Press, 1951), p. 12; Arthur P. Whitaker, *The United States and Argentina* (Cambridge, Mass.: Harvard University Press, 1954), pp. 118–21.

[2] Aggregate data studies include Gino Germani, *Estructura social de la Argentina. Análisis estadístico* (Buenos Aires: Raigal, 1955), Ch. 16; Pedro Huerta Palau, *Análisis electoral de una ciudad en desarrollo, Córdoba, 1929–1957–1963* (Córdoba: Universidad Nacional de Córdoba, 1963); Peter G. Snow, "The Class Basis of Argentina Political Parties," *American Political Science Review*, 63 (March 1969), 163–67; Floreal H. Forni and Pedro D. Weinberg, "Reflexiones sobre la relación entre clases sociales y partidos políticos en la Argentina," *Desarrollo Económico,* 12 (July-September 1972), 421–36; Paul H. Lewis, "The Female Vote in Argentina, 1958–1965," *Comparative Political Studies*, 3 (January 1971), 425–41; Peter H. Smith, "The Social Base of Peronism," *Hispanic American Historical Review*, 52 (February 1972), 55–73; Walter Little, "Electoral Aspects of Peronism, 1946–1954," *Journal of Inter-American Studies and World Affairs,* 15 (August 1973), 267–84; E. Spencer Wellhofer, "The Mobilization of the Periphery: Perón's 1946 Triumph," *Comparative Political Studies,* 7 (July 1974), 239–51. To date the only major work on Peronism utilizing survey data is Jeane Kirkpatrick's important *Leader and Vanguard in Mass Society: A Study of Peronist Argentina* (Cambridge, Mass.: MIT Press, 1971).

nations, rising levels of working-class political activism have encouraged a type of government which is both popular and authoritarian. This popular authoritarianism melds intensive political mobilization of previously excluded social sectors with political structures which severely limit these groups' ability to affect public policy.[3] Popular authoritarianism is an appropriate label not only for Peronism but also for the Vargas regime in Brazil, the Velasco Alvarado regime in Perú, and the Cárdenas regime in Mexico. As opposed to bureaucratic-authoritarian governments which attempt to exclude the mobilized popular sectors of society from political participation, these popular-authoritarian governments seek to activate and incorporate major segments of the popular sector into national political activity. As O'Donnell and others have noted, however, incorporation is accompanied by stringent restraints upon autonomous political activity.[4]

The ubiquitous nature of social and political mobilization suggests that popular authoritarianism may become increasingly common, providing the principal alternative to both bureaucratic authoritarianism and social revolution. For this reason, political researchers are now struggling to accommodate the needs of policy makers who, lacking alternative explanations, might continue to label (and relate to) the Third World's Peróns and Cárdenases as fascist and/or communist demagogues. But popular authoritarian regimes contain paradoxical elements—mobilization and repression—which make their ideologies and public policies appear vague and often contradictory to United States social scientists. Perhaps for this reason scholarly interest has centered upon a more easily understood aspect of popular authoritarianism: the attitudes and characteristics of popular authoritarian mass support. Given social scientists' belief that voters are capable of defining their needs and aspirations and then of rationally choosing to support candidates most willing to fulfill them, the analysis of popular-authoritarian electoral behavior has become a method for understanding the social base of such movements. With this knowledge at hand, if we can assume that popular-authoritarian elites are willing to respond to their constituents' needs, then it should be possible to explain or, perhaps, to anticipate some of the broader policies of future popular-authoritarian regimes.

While tenuous assumptions concerning voter rationality, elite responsiveness, and the political implications of social mobilization in the Third World continue to cry out for attention, concurrent questions must also be addressed. The answer to one of the most important of these questions involves determining the nature of popular-authoritarian mass support. Given the general absence of survey data in the Third World, for this task social scientists have tended to rely upon the comparative analysis of aggregate electoral behavior and socioeconomic-demographic variables. In the prototypic case of Argentine Peronism, more than 20 years have now passed since Germani's initial inquiry into the relationship between a central socioeconomic variable, social class, and Peronist electoral behavior. Since 1955 other students of Argentine politics have contributed analyses of additional variables and offered alternative explanations which have greatly refined our explanatory capability. By now virtually every socioeconomic variable has been mustered into the great explanatory fray, each accompanied by batteries of convincing arguments to protect its intellectual flanks.

The pages that follow identify the major hypotheses which have been offered to explain the socioeconomic nature of Peronist electoral behavior and then test these hypotheses with new, more sensitive data from the precincts and wards of the Federal Capital (Buenos Aires) and from the surrounding counties which, together with the Federal Capital, comprise Greater Buenos Aires. The analysis covers the entire 27-year electoral history of Peronism from 1946 to 1973. (The data and the manner in which they have been manipulated are described in the appendices.) The purpose of these tests is to assess the relative value of each hypothesis by constructing an optimum model to explain the socioeconomic determinants of Peronist electoral behavior.

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[3] As employed here, social mobilization refers to the process described by Karl W. Deutsch in "Social Mobilization and Political Development," *American Political Science Review*, 55 (September 1961), 494. Two studies of Argentine politics that employ Deutsch's perspective are Gino Germani, *Política y sociedad en una época de transición. De la sociedad tradicional a la sociedad de masas,* 4th ed., rev. (Buenos Aires: Editorial Paidos, 1971); and Peter H. Smith, "Social Mobilization, Political Participation, and the Rise of Juan Perón," *Political Science Quarterly,* 84 (March 1969), 30–49. Political mobilization is the term used by Huntington to describe the political participation of previously passive social sectors. Samuel P. Huntington, *Political Order in Changing Societies* (New Haven, Conn.: Yale University Press, 1969), pp. 266–70, 354–55.

[4] Guillermo A. O'Donnell, *Modernization and Bureaucratic Authoritarianism: Studies in South American Politics* (Berkeley: Institute of International Studies, University of California Press, 1973), pp. 89–95, 111; James M. Malloy, "Authoritarianism and Corporatism in Latin America: The Modal Pattern," in *Authoritarianism and Corporatism in Latin America,* ed. James M. Malloy (Pittsburgh: University of Pittsburgh Press, 1977), p. 14.

H_1: A Socially Mobilized Environment and a Plural Political Party Structure Encouraged Class-Oriented Electoral Behavior Before the Advent of Peronism

This initial hypothesis merits consideration because political mobilization has the potential to influence profoundly the nature of group consciousness.[5] Unless it can be demonstrated that previously mobilized groups switched their allegiance when a more attractive actor appeared to champion their interests, social scientists will never be able to ascertain whether these groups' electoral behavior is a product of the aggregated social characteristics of a movement's supporters or the result of a mobilization-induced consciousness. This fact, and the disparate social physiognomy of the original Peronist coalition–organized urban labor, a nationalistic military, and, perhaps, the "new" industrialists[6]–should suggest special care be taken when analyzing the socioeconomic correlates of the Peronist electorate.

Questions concerning the independent influence of political mobilization upon voting behavior have led to several studies of pre-Peronist electoral patterns. Far from reaching a consensus on the issue of prior political mobilization, two contradictory hypotheses currently vie for acceptance as the conventional wisdom. Peter Smith has marshaled an impressive array of historical data to demonstrate that the processes of social mobilization, political mobilization, and political integration were well advanced throughout the lower classes years before the 1943 coup ousted President Castillo. Smith is particularly careful to avoid equating political mobilization with political influence:

> In the late nineteen-thirties and early forties leaders ... sought to promote urban mass interests through established and constitutional political institutions. Before 1943 these leaders, mostly Socialists, were stopped at every turn by the ruling Concordancia; and the institutions through which they pressed their demands, particularly the national Congress, were devitalized and discredited. The critical point is that socially mobilized groups sought political participation, but were not given access to power.[7]

Gino Germani and Peter Snow, however, were unable to discern any significant pre-1943 lower-class political mobilization as indicated by class-oriented electoral behavior. Comparing the occupational structure and electoral statistics from each of the Federal Capital's 20 *secciones*, Germani found that "prior to 1946 the computed correlations do not reveal the existence of parties with homogeneous electorates from the point of view of their occupational composition."[8] Using Germani's data and a study of electoral behavior in the interior city of Córdoba, Peter Snow subsequently reported that "prior to the 1943 revolution ... correlations between social class and voting behavior were quite insignificant.... There is ... some evidence which indicates that occupation had little effect upon voting behavior during this period.... It also appears that occupation became an important factor in voting behavior after 1943 .... One should remember that just six years earlier such correlations virtually did not exist."[9] It is important to note that Snow's only interest was in comparing the conservative *Concordancia* with the centrist *Unión Cívica Radical* (UCR). For that reason the labor-oriented Socialists, the party which ran second to the UCR in 1940 and *first* in 1942, received no mention.

Data from the Federal Capital's electoral precincts (*circuitos*) offer a serious challenge to the Germani-Snow thesis that class-oriented electoral behavior first appeared in Argentina with Peronism's initial electoral victory. They also encourage the belief that lower-class political mobilization was well advanced by 1942. Table 1 presents the zero-order Pearson correlations between occupation and the vote for eight of the ten parties participating in the 1942 Federal Capital elections for national deputies.[10] Strongly conservative voting patterns appear restricted to areas with high percentages of professionals and students. Since the *Partido Conservador* received less than 1 percent of the total vote and the *Unión de Contribuyentes* scarcely fared better, the conservative vote is best indicated by results for the *Lista de Concordancia,* literally a list of candidates put forward by the semi-institutionalized coalition of Right and center-Right parties which governed Argentina throughout the 1930s and early 1940s. With over 21 percent of the popular vote, the *Concordancia* presumably provided conservative electors with an acceptable alternative to the centrist UCR and the three labor-oriented parties. These coefficients directly contradict Germani and Snow.

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[5] For a more deterministic view of the interaction between mobilization and group consciousness, see Gerald A. Heeger, *The Politics of Underdevelopment* (New York: St. Martin's Press, 1974), p. 7.

[6] Eldon Kenworthy has persistently questioned this early conventional wisdom. See his "Did the 'New Industrialists' Play a Significant Role in the Formation of Perón's Coalition, 1943–1946?" in *New Perspectives on Modern Argentina*, by Alberto Ciria et al. (Bloomington: Latin American Studies Program, Indiana University, 1972), pp. 15–28; and "The Formation of the Peronist Coalition" (Ph.D. dissertation, Yale University, 1970).

[7] Smith, "Social Mobilization, Political Participation, and the Rise of Juan Perón," p. 48.

[8] Germani, *Estructura social de la Argentina*, p. 260. [My translation.] The Spanish: "Así, mientras con anterioridad a 1946 las correlaciones computadas no revelan la existencia de partidos con electorado homogéneo desde el punto de vista de su composición ocupacional. . . ."

[9] Snow, "The Class Basis of Argentine Political Parties," pp. 163–64. The Córdoba study is that of Huerta Palau, *Análisis electoral de una ciudad en desarrollo*. Supporting the Germani-Snow thesis is Ezequiel Gallo (h.) and Silvia Sigal, "La formación de los partidos políticos contemporáneos: la UCR (1890–1916)," in *Argentina, sociedad de masas*, by Torcuato S. Di Tella et al. (Buenos Aires: Editorial Universitaria de Buenos Aires, 1965), p. 163.

The same cannot be said of the vote for the UCR, for although 29 percent of the electorate selected this option, no particular social group supplied a disproportionate share of the total UCR electoral strength. While similarly weak correlations are displayed by the very small *Partido Socialista Obrero,* the two principal labor-oriented parties, the *Partido Socialista* (PS) and the *Concentración Obrera,* exhibit rather strong social class relationships. Since the latter received slightly less than 8 percent of the popular vote, much greater attention should be accorded the PS, which actually won the election with one-third of all votes cast. By suggesting a strong linkage between the working class and the vote for the labor-oriented Socialists, these data also contradict Germani and Snow.

This assertion is placed in sharper relief by collapsing the eight parties into three categories: labor, center, and conservative. The coefficients in Table 2 indicate fairly strong tendencies toward conservativism among professionals and toward laborism among blue-collar workers. Support for the centrist parties continues to be of a multiclass nature. In short, these data from the 1942 election suggest that there is simply no justification for the hypothesis that class-conscious voting patterns first emerged with the initial Peronist election in 1946.

Although it is difficult to overlook the Socialists' consistently strong prolabor ideology, among Argentine social scientists there exists a tendency to deny the working-class orientation of the party.[11] But the fact that the PS adopted a parliamentary approach to reform and eschewed violent revolution should no more disqualify it as the working class' spokesperson in Argentina than it would the Labour party in Britain. Nor should the So-

Table 1. Social Class: Pearson Correlations between Occupation and Party Vote (Federal Capital, 1942; N = 209)

	Party							
Occupation	**PS**	**Concen-tración Obrera**	**Socialista Obrero**	**UCR**	**Radical**	**Unión de Contribu-yentes**	**Concor-dancia**	**Conser-vador**
% Professionals	–.39	–.36	–.20	–.20	–.02	.53	.55	.00
% Students	–.36	–.39	–.29	–.10	.00	.49	.50	–.08
% Businessmen	.11	–.12	–.20	–.04	–.08	.17	.00	.00
% White-collar employees	.13	–.08	.06	.13	.04	.09	–.21	.05
% Blue-collar workers	.29	.49	.15	.02	.03	–.62	–.39	–.08
% Unskilled workers	–.03	.21	.13	.02	.00	–.14	–.07	–.04
% of total vote	33.1	7.5	4.1	28.9	2.5	1.8	21.4	0.7

Source: Secretaría Electoral de la Capital Federal.

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[10] Two parties (the *Partido Salud Pública* and the *Unión Republicana*) received only nominal electoral support and have been excluded from all computations.

[11] Ernesto Sábato, *El otro rostro del peronismo, carta abierta a Mario Amadeo* (Buenos Aires: Impresa López, 1956), p. 20; José Luis de Imaz, *Los que mandan (Those Who Rule),* trans. with an introduction by Carlos A. Astiz with Mary F. McCarthy (Albany, N.Y.: State University of N.Y. Press, 1970), p. 208; Darío Cantón, *Party Alignments in Argentina between 1912 and 1955* (Buenos Aires: Instituto Torcuato Di Tella, 1967), p. 11; Carlos R. Melo, *Los partidos políticos argentinos,* 3rd ed. (Córdoba: Universidad Nacional de Córdoba, 1964), pp. 43–44.

Table 2. Social Class: Pearson Correlations between Occupation and Party Vote (Federal Capital, 1942; $N = 209$)

Occupation	Party Orientation Labor[a]	 Center[b]	 Conservative[c]
% Professionals	−.49	−.14	.58
% Students	−.49	−.05	.52
% Businessmen	.04	−.06	.01
% White-collar employees	.09	.17	−.21
% Blue-collar workers	.45	−.04	−.43
% Unskilled workers	.07	.01	−.08

[a] Labor: Concentración Obrera, Partido Socialista Obrero, PS.

[b] Center: Partido Radical, UCR.

[c] Conservative: Partido Conservador, Lista de Concordancia, Unión de Contribuyentes.

Source: Secretaría Electoral de la Capital Federal.

cialists' middle-class intellectual leadership be interpreted as an obstacle to working-class attraction by the same intellectuals who acknowledged Jorge Abelardo Ramos and Silvio Frondizi as leaders of working-class parties in the early 1970s. In the enforcement of legislation related to social welfare concerns, Peronism accomplished what the Socialists could have accomplished had they enjoyed Perón's monopoly on coercion. His initial years in power were characterized by the enthusiasm with which he enforced, as previous non-Socialist chief executives had ignored, a rich legacy of progressive labor and welfare legislation introduced by Socialist legislators.[12]

In any event, these and similar arguments concerning the commitment of the PS to the working class have been allowed to obscure clear evidence that prior to 1946 many members of the working class perceived the Socialists as their champion in Argentine politics. At the very least, the correlations in the preceding tables serve to support Smith's assertion that the working class was politically mobilized (and pro-Socialist) before the arrival of Peronism. Thus it is not unreasonable to conclude that the vote for the Peronist movement represents at least in part a response to perceived socioeconomic conditions rather than to a unique *conciencia peronista* produced during a hypothetical Perón-induced process of political mobilization.

H_2: The Proportion of an Area's Popular-Authoritarian Vote is Directly Related to the Nature of its Social Class Structure

This hypothesis is so nearly a truism that it would hardly merit further consideration were it not for the fact that most prior analyses have interspersed their discussions of social class, particularly the working class, with other independent variables. Thus Peronism attracted "a growing mass of politically unrepresented urban laborers newly migrated from the rural areas"[13] which may have become inconstant following "acclimatization of the workers to city life."[14] Even those studies that do isolate social class as a causal variable often find it necessary to employ unwieldy units of analysis or to offer subjective ordinal classifications of electoral districts that not only limit the nature of statistical inquiry but also are subject to the misperceptions which can accompany impressionistic classification.[15] Employing the smallest units available for aggregate analysis, the present discussion focuses exclusively upon social class as measured by occupation.

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[12] Partial listings of this legislation may be found in Adolfo Dorfman, *Historia de la industrial argentina* (Buenos Aires: Escuela de Estudios Argentinos, 1942), pp. 194–96; Carlos S. Fayt, *La naturaleza del peronismo* (Buenos Aires: Viracocha, 1967), p. 91. The PS was not the only party with an interest in labor and welfare legislation, for their minority status in Congress indicates that each bill became law only with the help of members of other political parties.

[13] Peter Ranis, "Electoral Systems and Party Competition: The Case of Argentina," unpublished manuscript, Department of Political Science, The State University of New York at Stony Brook, n.d., p. 10.

[14] David J. Butler, "Charisma, Migration, and Elite Coalescence: An Interpretation of Peronism," *Comparative Politics,* 1 (April 1969), 433. See also Torcuato S. Di Tella, *El sistema político argentino y la clase obrera* (Buenos Aires: Editorial Universitaria de Buenos Aires, 1964), pp. 10, 13; Alfredo Parera Dennis, "Apuntes para una historia del peronismo," *Fichas,* 2 (October 1965), p. 13; Pedro Geltman, "Mitos, símbolos, y héroes en el peronismo," in *El peronismo,* by Gonzalo Cárdenas et al. (Buenos Aires: Carlos Pérez, 1969), p. 123; Jean-Claude García-Zamor, "Justicialismo en Argentina: la ideología política de Juan Domingo Perón," unpublished manuscript, Department of Government, University of Texas at Austin, n.d., pp. 5–6; Gino Germani, *Política y sociedad en una época de transición,* p. 324; Fayt, *La naturaleza del peronismo,* pp. 70, 86; Roberto Carri, *Sindicatos y poder en la Argentina* (Buenos Aires: Editorial Sudestada, 1967), p. 48.

The social class orientation of Argentine politics is demonstrated most dramatically by the correlations between occupation and candidate preference in 1946. The Perón-Quijano coefficients in Table 3 indicate the existence of a tendency toward upper-class and working-class electoral polarization.[16] This is common knowledge, of course, and needs little emphasis here. The correlations between occupation and the Peronist vote in the 209 precincts of the Federal Capital between 1946 and 1973 are presented in Table 4; Table 5 contains similar data for the 39 wards and counties of Greater Buenos Aires. Unquestionably there exists a tendency for these aggregate electoral data and broad occupational categories to obscure fine distinctions and overemphasize the unidirectionality of upper-class and working-class electoral behavior. But it nevertheless remains apparent that areas of relatively large working-class social composition are those which strongly support the Peronist movement.

These data linking the working class to Peronism were confirmed by Jeane Kirk-

Table 3. Social Class: Pearson Correlations between Occupation and the Peronist Vote (Federal Capital, 1946; $N = 209$)

Occupation	Perón–Quijano[a]
% Professionals	–.70
% Students	–.67
% Businessmen	–.41
% White-collar employees	–.04
% Blue-collar workers	.79
% Unskilled workers	.24

[a] Endorsed by the *Partido Laborista*, UCR–*Junta Renovadora*, *Juventud Renovadora Argentina*, and the *Partido Patriótico 4 de Junio*.

Source: Secretaría Electoral de la Capital Federal.

Table 4. Social Class: Pearson Correlations between Occupation and the Peronist Vote (Federal Capital, 1946–1973; $N = 209$)

Occupation	PERON[a]	1946	1951	1957	1960	1962	1965	1973
% Professionals	–.71	–.70	–.66	–.61	–.67	–.66	–.65	–.71
% Students	–.72	–.67	–.68	–.63	–.69	–.64	–.69	–.71
% Businessmen	–.47	–.41	–.39	–.44	–.43	–.44	–.49	–.45
% White-collar employees	–.05	–.04	–.11	–.12	–.05	.01	–.06	.04
% Blue-collar workers	.81	.79	.80	.70	.76	.71	.74	.70
% Unskilled workers	.33	.24	.26	.33	.33	.28	.35	.34

[a] PERON is a composite variable representing the sum of the products of each election's standardized score and its factor score coefficient (see Appendix A).

Source: Secretaría Electoral de la Capital Federal.

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[15] An example of the former problem is found in Germani's "El surgimiento del peronismo: el rol de los obreros y de los migrantes internos," *Desarrollo Económico,* 13 (October-December 1973), 435–88. The methodological and theoretical content of Germani's article has already received substantial comment. See Peter H. Smith, "Las elecciones argentinas de 1946 y las inferencias ecológicas," *Desarrollo Económico,* 14 (July-September 1974), 385–98; Eldon Kenworthy, "Interpretaciones ortodoxas y revisionistas del apoyo inicial del peronismo," *Desarrollo Económico,* 14 (January-March 1975), pp. 749–63; Tulio Halperín Donghi, "Algunas observaciones sobre Germani, el surgimiento del peronismo y los migrantes internos," *Desarrollo Económico,* 14 (January-March 1975), pp. 765–81. The latter problem of subjective classification can be partially overcome by careful selection of a limited number of cases as, for example, in Snow's "The Class Basis of Argentine Political Parties," pp. 165–66.

[16] Special care must be taken when comparing these data with those from the 1942 election, for a radical realignment of parties occurred prior to the initial Peronist victory when virtually all of Argentina's established political parties confirmed Charles Dudley Warner's observation about strange bedfellows by uniting against Perón in the *Unión Democrática.*

patrick's 1965 survey analysis.[17] While she emphasized that Peronism is not an exclusively single-class movement, Kirkpatrick produced the data in Table 6 to support her conclusion that the Peronists are disproportionately associated with the Argentine working class. And, significantly, she also found that these "lower-class Peronists demonstrated a class consciousness unique among Argentines."[18]

The insignificant values registered by white-collar employees in the Federal Capital (Table 4) contradict similar data from Greater Buenos Aires (Table 5) and occasional assertions that the Argentine middle class was and remains vehemently anti-Peronist. Juan José Sebreli's influential work, for example, characterizes Peronism as "a challenge to petite bourgeois traditions, customs, established values, moral clichés, Philistine inhibitions, and hypocritical ideology of virtue. . . . The middle class reacted to this historical process which it did not understand with an hysterical anti-Peronism."[19] Recent empirical studies cast doubt upon Sebreli's interpretation and the negative correlations in Table 5. The more reliable data from the Federal Capital's 209 precincts (Table 4) produce directionless correlation coefficients. In addition, Kirkpatrick's survey discerned a tendency for the Peronist movement to attract a substantial middle-class following. "Middle-income respondents are well represented among Peronists, comprising approximately one-third of the persons who supported Perón for President and a larger portion of those with sympathy for the movement."[20] Both Kirkpatrick's analysis and the data in Table 4 suggest that very little faith should be placed in Sebreli's impressions or in the reliability of the middle-class coefficients in Table 5.

Table 5. Social Class: Pearson Correlations between Occupation and the Peronist Vote (Greater Buenos Aires, 1946–1973; $N = 39$)

Occupation	PERON	1946	1954	1957	1960	1962	1965	1973
% Professionals, 1960	–.96	–.83	–.95	–.90	–.89	–.94	–.84	–.96
% Students, 1960	–.80	–.60	–.80	–.71	–.75	–.81	–.72	–.86
% White-collar employees, 1960	–.72	–.47	–.74	–.68	–.73	–.74	–.64	–.77
% Blue-collar workers, 1960	.91	.76	.91	.88	.88	.92	.80	.90

Source: Ministerio del Interior, Dirección Nacional Electoral; Dirección Nacional de Estadística y Censos, *Censo nacional de población 1960.*

Table 6. Socioeconomic Characteristics and Orientations to Peronism, 1965

	Percent of Class[a]		
	Lower ($N = 721$)	Middle ($N = 960$)	Upper ($N = 157$)
Would vote for Perón	32.0	13.0	6.4
Generally approve of Peronist movement	38.7	18.4	12.7
Would support candidate supporting Perón	32.6	15.2	7.0
Perón did most to harm Argentina	19.4	39.5	65.0
Would hurt Argentina for Perón to return	34.8	56.7	77.7

[a] Class characterization based on interviewer appraisal.

Source: Kirkpatrick, *Leader and Vanguard in Mass Society,* 97.

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[17] Kirkpatrick, *Leader and Vanguard in Mass Society,* p. 94.

[18] Kirkpatrick, *Leader and Vanguard in Mass Society,* p. 96.

[19] Juan José Sebreli, *Buenos Aires, vida cotidiana y alienación,* 3rd ed. (Buenos Aires: Ediciones Siglo Veinte, 1965), pp. 98, 99, 102–03. Somewhat similar opinions of the middle class' attitude toward Peronism can be found in Torcuato S. Di Tella, "Populism and Reform in Latin America," in *Obstacles to Change in Latin America,* ed. Claudio Véliz (London: Oxford University Press, 1965), pp. 71–72; and in Carri, *Sindicatos y poder en la Argentina,* p. 61.

[20] Kirkpatrick, *Leader and Vanguard in Mass Society,* p. 97. Peter Snow has used aggregate data to confirm the existence of substantial Peronist support in the middle class. See "The Class Basis of Argentine Political Parties," p. 166.

H_3: The Proportion of an Area's Popular-Authoritarian Vote is Directly Related to its Population Growth Rate

Demographic change has always shared with social class the leading position on any list of variables most crucial to an adequate explanation of the development of the Peronist phenomenon. Here again, however, descriptive analyses have tended either to blur together discrete demographic characteristics—migrants "uprooted from their native environment and out of place in the new"[21]—or, more frequently, to place demographic variables alongside other socioeconomic variables without attempting to assess their separate impacts: "These displaced persons were not only poor and resentful; coming from the interior, where the Indian strain had been strong in colonial and early national times, they were also so swarthy that most of the older porteños referred to them contemptuously. . . ."[22]

Early in the data preparation a factor analysis of the demographic variables indicated the existence of two distinct demographic dimensions (see Appendix B, Table B2). One, urbanization, maintained a correlation of +.10 with the second, somewhat broader category of population growth (POPULATION GROWTH) which was defined as the percent average annual population increase and the percent average annual change in population density. Although migrants undoubtedly contributed to the area's population growth, no reliable data have ever been found to document their proportional share of the overall population increase.[23] Urbanization was simply defined as the percent of a county or ward's total population living in population clusters of 2,000 or more persons in the census years of 1947 and 1960.

Given the fact that in 1977 Greater Buenos Aires' eight million inhabitants are vying with those of Mexico City for the distinction of residing in the largest Spanish-speaking metropolis on earth, the area's negligible variation in percent urban population renders meaningless an analysis of the independent influence of urbanization upon the Peronist vote. While it is true that in 1947 and even in 1960 there was considerable variation in the percent urban population among Greater Buenos Aires' counties—the mean percent urban in 1947 was 92 percent with a range from 42 to 100 percent—social scientists nevertheless define urbanization broadly as the exposure to an urban environment through media, economic interaction, or physical proximity. By this definition the entire population of Greater Buenos Aires has long been as urban as any in the world. Given its insufficient variance, then, urbanization was eliminated from this study.

The literature on popular authoritarianism indicates that the second dimension of demographic change, population growth, should maintain an important positive relationship with the Peronist vote. With a zero-order POPULATION GROWTH—PERON correlation of +.69, the data in Table 7 tend to confirm the findings of earlier studies. Because much of Buenos Aires' demographic change may be attributed to working-class migration from the interior, it is possible that a more fundamental association between social class and Peronism underlies the POPULATION GROWTH—PERON relationship. Apparently population growth is more than just an intervening variable, however, for despite the +.35 WORKING CLASS—POPULATION GROWTH correlation, the first-order partial between POPULATION GROWTH and PERON controlling for WORKING CLASS remains a very high +.70. As Table B9 indicates, only when controlling for the composite variable of industrial growth (see Appendix B) is the POPULATION GROWTH—PERON correlation reduced from +.69 to +.08. And here, unfortunately, the intractable problem of multicollinearity (the POPULATION GROWTH—INDUSTRIAL GROWTH correlation is +.84) distorts the meaning of partial correlation coefficients.

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[21] Juan Carlos Rubenstein, "El peronismo y la vida argentina," *Fichas*, 2 (December 1965), p. 7.

[22] Arthur P. Whitaker, *Argentina* (Englewood Cliffs, N.J.: Prentice-Hall, 1964), p. 107. See also Tomás Roberto Fillol, *Social Factors in Economic Development: The Argentine Case* (Cambridge, Mass.: MIT Press, 1961), p. 83; Parera Dennis, "Apuntes para una historia del peronismo," p. 13; Geltman, "Mitos, símbolos, y héroes en el peronismo," p. 103; Germani, *Política y sociedad en una época de transición*, p. 324.

[23] It is important to note that this study specifically avoids any discussion of the political influence of rural-urban migration. There are no reliable data on migrants during the early Peronist years and, unfortunately, the more reliable data on migrants from later periods (as, for example, the individual-level sample from the 1960 census) do not provide information on political behavior or attitudes. Since the census contains only information on interprovincial migration, aggregate data correlations between migrants and Peronism are a matter of heroic guesswork.

H_4: The Proportion of an Area's Popular-Authoritarian Vote is Directly Related to its Level of Industrial Development

The literature on the origin and persistence of Peronism hypothesizes a direct linkage with an area's absolute level of industrialization, narrowly defined here in terms of factories and operationalized as the composite variable INDUSTRY[24] (see Appendix B, Table B8). Table 8 presents the zero-order Pearson correlations between INDUSTRY, its component variables, and the vote for the Peronist movement in Greater Buenos Aires.[25] As measured by the number of workers per industrial establishment, wages per industrial establishment, and value of production per industrial establishment, INDUSTRY seems to be nearly unrelated to Peronist voting patterns. In 1946 and to a lesser extent in 1957, a positive association exists between areas containing large factories (workers per industrial establishment) and those possessing substantial Peronist electoral strength, but over time this relationship becomes insignificant.

Table 7. Pearson Correlations between Population Growth and the Peronist Vote (Greater Buenos Aires, 1946–1973; N = 39)

	PERON	1946	1954	1957	1960	1962	1965	1973
POPULATION GROWTH	.69	.65	.76	.67	.76	.73	.66	.64
% Average annual population growth, 1914–1947	.67	.63	.71	.62	.66	.67	.62	.56
% Average annual population growth, 1947–1960	.70	.44	.71	.64	.72	.72	.71	.78
% Average annual change in population density, 1914–1947	.67	.63	.71	.63	.67	.67	.62	.56
% Average annual change in population density, 1947–1960	.70	.67	.70	.69	.68	.70	.63	.63

Source: See Table 5.

Table 8. Pearson Correlations between the Level of Industrialization and the Peronist Vote (Greater Buenos Aires, 1946–1973; N = 39)

	PERON	1946	1954	1957	1960	1962	1965	1973
INDUSTRY	.15	.24	.07	.25	.06	.10	.15	.12
Workers per industrial establishment, 1935	.06	.23	–.06	.16	–.01	.03	.02	.02
Workers per industrial establishment, 1941	.15	.34	.11	.22	.03	.10	.13	.04
Workers per industrial establishment, 1947	.23	.38	.15	.32	.17	.18	.18	.15
Workers per industrial establishment, 1963	–.08	–.01	–.12	–.04	–.09	–.13	–.10	–.05
Wages per industrial establishment, 1935	–.13	–.03	–.26	–.04	–.17	–.14	–.12	–.13
Wages per industrial establishment, 1941	–.13	.01	–.19	–.05	–.22	–.16	–.11	–.17
Wages per industrial establishment, 1947	.04	.13	–.05	.12	.01	.01	.02	.01
Wages per industrial establishment, 1963	.00	.04	–.02	.02	–.02	–.03	.00	.02
Value of production per industrial establishment, 1947	.13	.26	.04	.22	.08	.11	.11	.07
Value of production per industrial establishment, 1963	.20	.27	.18	.19	.17	.18	.17	.21

Source: Ministerio del Interior, Dirección Nacional Electoral; Comisión Nacional del Censo Industrial, *Censo industrial de 1935;* Dirección General de Estadística y Censos, *Estadística industrial de 1941;* Ministerio de Asuntos Técnicos, *IV censo general de la Nación;* Instituto Nacional de Estadística y Censos, *Censo nacional econòmico 1963.*

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[24] Among the many studies linking industrialization to Peronism are Fayt, *La naturaleza del peronismo,* pp. 92–93; Whitaker, *Argentina,* p. 149; Carri, *Sindicatos y poder en la Argentina,* p. 36; O'Donnell, *Modernization and Bureaucratic-Authoritarianism,* p. 56; and Carlos F. Díaz Alejandro, *Essays on the Economic History of the Argentine Republic* (New Haven, Conn.: Yale University Press, 1970), pp. 112–13.

[25] It is difficult to determine the extent to which all industrial establishments are included in these data. By law all industrial firms must report their activity to the census bureau, but it is probable that many of the smaller establishments fail to do so.

The lack of substantial evidence to confirm the expected positive relationship between an area's Peronist vote and its level of industrial development encourages a re-interpretation of the original hypothesis. A closer inspection of the literature reveals, in fact, that the simple INDUSTRY–PERON linkage is offered as a harbinger of changes not in an area's economic structure but rather in the nature of its social class composition. No one suggests that industry directly causes a large vote for Peronism, but that industrial development tends to attract the types of people, generally working-class people, who then go to the polls in support of the Peronists. If an area's working-class population serves as an intervening linking variable between industrial development and the Peronist vote, a partial correlation between INDUSTRY and PERON controlling for WORKING CLASS will indicate the proportion of the +.15 zero-order relationship that may be directly attributed to industrialization. Not surprisingly, the first-order partial is –.37, demonstrating not only that all of the positive influence of INDUSTRY on PERON is due to the fact that industrialization attracts working-class voters, but also that industrialization actually has a negative impact upon the Peronist vote. Should an area manage to industrialize without attracting a disproportionately larger working-class population, it will exhibit relatively anti-Peronist voting patterns. Because this occurs so infrequently (the INDUSTRY–WORKING CLASS correlation is +.45) the question need not be pursued beyond noting once more the crucial influence of an area's working-class population upon its electoral behavior.

Outside Greater Buenos Aires are nearly 500 counties possessing incredibly varied levels of industrialization, where such major industrial centers as Rosario and Córdoba may be compared with counties inhabited by more glaciers than blue-collar workers. The zero-order correlations between the level of industrialization and the Peronist vote in these countries are presented in Table 9. Here again, INDUSTRY appears associated with neither positive nor negative Peronist electoral performance. No coefficient is able to account for as much as nine percent of the variation in the vote for Peronism.

H_5: The Proportion of an Area's Popular-Authoritarian Vote is Directly Related to its Rate of Industrial Growth

It has been asserted that the economic basis of Peronism's electoral strength lies not in an area's level of industrial development nor in its type of industrial activity, but rather in Argentina's differential levels of economic change. This process of economic change is said to initiate a complex series of interconnected socioeconomic and sociodemographic transformations which, taken together, encourage the development of popular authoritarianism. As narrowly defined here in terms of industrial growth, economic change acts as a catalyst to "detraditionalize" a region's social and political behavior.[26]

Table 9. Pearson Correlations between the Level of Industrialization and the Peronist Vote (All Argentine Counties,[a] 1946–1965)

	1946	1954	1957	1960	1962	1965
Workers per industrial establishment, 1947	.14	.20	.15	.11	.20	.28
Workers per industrial establishment, 1963	.12	.09	.23	.25	.25	.13
Wages per industrial establishment, 1947	.13	.17	.22	.13	.25	.28
Wages per industrial establishment, 1963	.10	.07	.26	.29	.24	.17
Value of production per industrial establishment, 1947	.13	.10	.22	.13	.25	.09
Value of production per industrial establishment, 1963	.06	.04	.21	.26	.27	.13
N =	290	328	348	308	304	289

[a] Excluding Greater Buenos Aires.

Source: Ministerio de Asuntos Técnicos, *IV censo general de la Nación;* Dirección Nacional de Estadística y Censos, *Censo nacional de población 1960.*

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[26] Marcos Kaplan, *Economía y política del petróleo argentino (1939–1956)* (Buenos Aires: Praxis, 1957), p. 34; Guido Di Tella and Manuel Zymelman, "Etapas

This hypothesis is supported by the Peronist electoral data. The Pearson coefficients in Table 10 indicate an extremely strong positive relationship between the rate of industrial growth and Peronist voting strength. These correlation coefficients also disclose that the areas which grew most rapidly between 1935 and 1941 were those that *least* supported Peronism in subsequent elections. This is due to dramatic transformations in several established patterns of industrial growth in the years following this prewar period. Areas which had evidenced negative industrial growth rates between 1935 and 1941 blossomed forth with an impressive expansion of industrial facilities. The Pearson correlation between increases in industrial establishments from 1935 to 1941 and increases from 1941 to 1947 is –.70. From 1935 to 1941 the annual average economic growth rate of Lanús ranked thirty-fifth among Greater Buenos Aires' 39 counties, while from 1941 to 1947 it was sixth. Similar leaps occurred in General San Martín (twenty-ninth to third), Lomas de Zamora (thirty-third to seventh), and Tres de Febrero (thirteenth to second). Not all of this growth was concentrated in areas that subsequently provided strong Peronist electoral support, of course, but the correlations in Table 10 intimate that after 1941 increases in industrial establishments and industrial workers became and remain the most reliable economic indicators of Peronist strength.

But while it is difficult to deny that areas undergoing industrial growth tend to be areas of strong Peronist electoral support, it is also important to emphasize that the simple correlation coefficients in Table 10 do not indicate the amount of direct independent influence of an area's industrial change upon its Peronist vote. As the high coefficients in the independent variable correlation matrix indicate (see Appendix B, Table B10), an area undergoing industrial change also tends to have a relatively rapidly growing population, to be relatively working-class in social composition, and to exhibit relatively little economic satisfaction. The question of the direct and indirect effects of industrial change upon popular-authoritarian electoral strength remains to be addressed below.

A similar analysis of the influence of indicators of industrial growth upon the Peronist vote in the Argentine counties outside Greater Buenos Aires did not match the strong correlations between industrial growth and the Peronist vote in the metropolitan area. Outside Greater Buenos Aires not a single zero-order correlation coefficient could account for as much as ten percent of the vote for Peronism. Even when correlations were computed between PERON and the indicators of industrial growth for the 90 counties exhibiting more than the mean rate of industrial change, the results revealed nothing significantly different from zero. Only when the nine counties with

Table 10. Pearson Correlations between Industrial Growth and the Peronist Vote (Greater Buenos Aires, 1946–1973; N = 39)

	PERON	1946	1954	1957	1960	1962	1965	1973
INDUSTRIAL GROWTH	.79	.66	.81	.74	.81	.77	.75	.74
% Average annual change in industrial establishments, 1941–1947	.59	.58	.60	.58	.60	.60	.57	.44
% Average annual change in industrial establishments, 1947–1963	.80	.69	.78	.73	.82	.82	.75	.79
% Average annual change in industrial workers, 1941–1947	.68	.58	.66	.68	.71	.69	.66	.58
% Average annual change in industrial workers, 1947–1963	.64	.45	.64	.52	.68	.66	.61	.71
Variables NOT used in construction of the composite variable INDUSTRIAL GROWTH								
% Average annual change in industrial establishments, 1935–1941	–.50	–.50	–.44	–.57	–.45	–.51	–.51	–.37
% Average annual change in industrial workers, 1935–1941	–.45	–.31	–.34	–.52	–.45	–.47	–.48	–.44

Source: See Table 9.

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del desarrollo económico argentino," in *Argentina, sociedad de masas,* pp. 184–86; Fayt, *La naturaleza del peronismo,* p. 70; Carri, *Sindicatos y poder en la Argentina,* p. 36; Germani, *Política y sociedad en una época de transición,* p. 324; Geltman, "Mitos, símbolos, y héroes en el peronismo," p. 123; James R. Scobie, *Argentina: A City and a Nation* (New York: Oxford University Press, 1964), p. 187.

more than 500 industrial establishments were grouped apart did impressive coefficients finally begin to appear. And, using Guilford's formula to deflate artificially high estimates obtained from small samples, even these lost their strength.[27]

H_6: The Proportion of an Area's Popular-Authoritarian Vote is Inversely Related to its Level of Economic Satisfaction

Nearly all theories attempting to explain the development of popular authoritarianism emphasize the influential role of economic dissatisfaction in exacerbating the tensions of traditional politics and reducing the perceived legitimacy of traditional political actors to the point that new political groups can obtain access to the arenas of power. Thus an entire literature exists which relates the growth and persistence of the Peronist phenomenon to Argentina's economic woes.[28] Little effort has been made to separate the independent influence of economic satisfaction from lower social class membership, however, and many studies define somewhat narrowly the types of persons who might be unhappy with their economic conditions. That the weighty economic burdens of Third World capitalist societies fall most heavily upon those shoulders least able to support them cannot be doubted; that the working class is the only dissatisfied social sector or that economic discontent influences Peronist electoral behavior are hypotheses in need of empirical support.

Of all the variables under discussion in this study, economic satisfaction is perhaps the least compatible with ecological analysis. Unlike social status, economic change, or population growth, economic satisfaction refers to a totally subjective phenomenon that simply cannot be measured accurately. The best that can reasonably be expected from aggregate data is a comparative analysis of similar groups which, through the grouping process itself, tends to hold constant a number of potentially significant intervening variables. Because the data for this paper are from a relatively industrialized area—Greater Buenos Aires—and because Peronism apparently finds its most substantial support among industrial workers, attention is confined to industrial employees (*obreros* and *empleados*) and their economic satisfaction is defined in the simplest of terms—wages. While the crudity of this estimating procedure is evident, in a relatively urban environment with well-developed means of communications it is not unreasonable to expect that differential wage rates would tend to create groups with different levels of economic satisfaction.

Given these constraints, then, are variations in areas' levels of economic satisfaction associated with variations in Peronist electoral strength? The data in Table 11 indicate that a fairly strong negative relationship exists between economic satisfaction and the vote for Peronism. The simple Pearson correlation between SATISFACTION and PERON of –.47 tends to confirm earlier analyses linking economically dissatisfied voters to the Peronist movement. But a closer inspection of partial correlations casts doubt upon this relationship and suggests that the SATISFACTION–PERON linkage may be spurious. At issue here is the influence of the size of an area's working-class population upon both variables. Experience with urban immigrant concentrations in the United States and elsewhere has demonstrated that recent arrivals are generally forced to accept relatively low-status employment. Because much of Greater Buenos Aires' working class may be composed of internal migrants who are generally more satisfied with

Table 11. Pearson Correlations between Economic Satisfaction and the Peronist Vote (Greater Buenos Aires, 1946–1973; $N = 39$)

	PERON	1946	1954	1957	1960	1962	1965	1973
SATISFACTION	–.47	–.60	–.50	–.49	–.42	–.44	–.38	–.33
Wages per industrial employee, 1935	–.34	–.45	–.38	–.37	–.29	–.31	–.26	–.24
Wages per industrial employee, 1941	–.60	–.66	–.62	–.62	–.59	–.58	–.52	–.47
Wages per industrial employee, 1947	–.42	–.53	–.43	–.46	–.35	–.40	–.35	–.30

Source: See Table 9.

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[27] J. P. Guilford, *Fundamental Statistics in Psychology and Education*, 4th ed. (New York: McGraw-Hill, 1965), pp. 400–01. See the Communication from David Pfotenhauer, *American Political Science Review*, 68 (March 1974), pp. 205–06.

[28] Rodolfo Mario Pandolfi, "17 de octubre, trampa y salida," *Contorno*, número 7–8 (July 1956), pp. 21–28; Kaplan, *Economía y política del petróleo argentino*, p. 34; Fayt, *La naturaleza del peronismo*, pp. 92–93; Whitaker, *Argentina*, p. 149.

their new economic positions than with their former existence,[29] it is imperative to analyze the influence of an area's working class upon its level of economic satisfaction.

As it turns out, areas that might be classified as economically satisfied tend to be areas exhibiting relatively small working-class populations—the SATISFACTION–WORKING CLASS correlation is –.54. Conversely, the areas of greatest Peronist voting strength have relatively large working-class populations—the WORKING CLASS–PERON correlation is +.77. For this reason, at any given level of working-class population, the first-order partial correlation between SATISFACTION and PERON plummets from –.47 to a nearly directionless –.11. In short, accepting the logic of the preceding paragraphs leads to the conclusion that economic satisfaction maintains only a marginal relationship with the Peronist vote.

Because its use would involve incredibly tenuous assumptions, another independent variable—percent annual *increase* in wages per industrial employee—was omitted from the composite variable of economic satisfaction. But Perón has often been described as a vote-buyer, and for that reason alone the data in Table 12 deserve mention. They indicate what many have long assumed to be true: a substantial change occurred in the relationship between Peronist voting strength and increases in wages per industrial employee as Peronism began to attract workers in areas noted for their wage increases. This does not mean that Peronism's constituency changed; rather it suggests that the economic position of the same constituency was altered dramatically in a brief period. The correlation between increases in wages from 1935 to 1941 and increases in wages from 1941 to 1947 is –.42, demonstrating that the beneficiaries of earlier wage increases lost their relatively advantageous position after 1941. Perhaps more illuminating than this summary measure is a glance at specific areas. From 1935 to 1941 industrial employees in Buenos Aires' relatively wealthy Socorro received an average annual hike in wages per capita of 3.4 percent compared to negative adjustments in wages per capita in working-class areas such as General San Martín (–1.5 percent), Lanús (–3.8 percent), and Lomas de Zamora (–7.0 percent) during the same period.[30] Between 1941 and 1947 these roles were reversed, as Socorro's substantial average annual wage increase of 8.2 percent was more than matched by the three working-class counties' average of 11.3 percent. For those employees accustomed to wage reductions, the increase might have appeared even greater.

Multivariate Analysis

It should now be possible to assess the relative importance of these independent variables from a vantage point provided by our

Table 12. Pearson Correlations between Industrial Wage Increases and the Peronist Vote (Greater Buenos Aires, 1946–1973; $N = 39$)

	PERON 1946	1954	1957	1960	1962	1965	1973
% Average annual increase in wages per industrial employee, 1935–1941	–.51	–.38	–.47	–.52	–.58	–.52	–.54	–.50
% Average annual increase in wages per industrial employee, 1941–1947	.54	.43	.54	.55	.61	.50	.47	.47
% Average annual increase in wages per industrial employee, 1947–1963	.22	.21	.26	.21	.22	.25	.21	.26

Source: See Table 9.

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[29] A massive literature now exists to confirm the *relative* satisfaction of rural-urban migrants. See Joan M. Nelson's somewhat dated but excellent *Migrants, Urban Poverty, and Instability in Developing Nations* (Cambridge, Mass.: Center for International Affairs, Harvard University, 1969), pp. 15–20; and Wayne A. Cornelius' comprehensive "The Political Sociology of Cityward Migration in Latin America: Toward Empirical Theory," in *Latin American Urban Research,* Vol. 1, ed. Francine F. Rabinovitz and Felicity M. Trueblood (Beverly Hills, Calif.: Sage Publications, 1971), pp. 95–147. Speaking in terms more general than those of economic satisfaction, perhaps our contemporary understanding of the political impact of cityward migration is best described in the final paragraph of Cornelius' "Urbanization and Political Demand Making: Political Participation among the Migrant Poor in Latin American Cities," *American Political Science Review,* 68 (September 1974), p. 1146.

[30] It is probable that the working-class districts' decreases between 1935 and 1941 stemmed more from population growth than from actual wage cuts, as migrants continued to flood into parts of Greater Buenos Aires and exert downward pressure upon wages per capita.

understanding of the manner in which they are individually associated with the vote for Peronism. The purpose here is not to construct and then to test a series of causal models in an attempt to explain Peronist electoral behavior. When the tendency of aggregate data to generate impressive correlation coefficients is combined with our not infrequent inclination to accept quantitative data on face value, it is possible that the results of an uncontrolled regression of PERON on all five composite independent variables (see Table 13) may be misinterpreted. Despite the impressive coefficient of determination, .86, this regression equation "explains" variance only in the statistical sense of the word. To employ these data as a substantive explanation of Peronism's voting behavior would be tacitly to presume not only that these aggregate data accurately reproduce individual-level attitudes and behavior but that the independent variables are all those most likely to determine the Peronist vote—presumptions many would consider untenable. Other variables which are not based entirely upon socioeconomic characteristics—party organization, factory experience, union membership, to name but a few—need to be included in any such explanatory model. While the purpose of this inquiry is somewhat less ambitious, it is an essential step toward eventual explanation: what is proposed is a clarification of only the socioeconomic basis of variation in Peronist voting patterns. It is assumed that socioeconomic variables contribute an unknown amount toward an understanding of the movement's electoral behavior. It is assumed that most if not all of the socioeconomic variables have been identified.[31] We wish to determine which among them are the most influential.

This tentative understanding of the manner in which the variables relate to one another is in large measure derived from deductive theory and the preceding discussions of hypothesized associations rather than from the data themselves. The purpose of this multivariate approach, therefore, is not to employ causal inference to reduce a large number of logical models to one or more optimum explanatory sequences. Rather it is to propose a fixed model and then to determine the relative influence of each of the model's linkages. The procedures followed here do not begin with the development of prediction equations specifying zero relationships, then, but take a given model and compute the path coefficients. With the causal order fixed, no linkages will be eliminated. This seems only logical in the present context, where the goal is to analyze variables representing specific hypotheses found throughout the literature on Peronism and to determine which among them are most important and which possess relatively little impact on the Peronist vote.

The core hypothesis which informs the basic socioeconomic model is that the rate of industrial growth is the fundamental independent variable acting directly and indirectly to influence the proportion of an area's Peronist vote. Figure 1 presents this causal model, and the appropriate path coefficients are given in Table 14. The large number of paths in Figure 1 should not be allowed to obscure some of the most important linkages, particularly the influence of INDUSTRIAL GROWTH upon PERON, WORKING CLASS, and POPULATION GROWTH as well as the impact of WORKING CLASS upon both PERON and

Table 13. Uncontrolled Multiple Regression of PERON on the Composite Independent Variables (Greater Buenos Aires)

Variable	R	R^2	b/Beta[a]	Standard Error b
INDUSTRIAL GROWTH	.79	.62	.49	.14
WORKING CLASS	.92	.84	.60	.11
SATISFACTION	.93	.86	.17	.09
POPULATION GROWTH	.93	.86	.14	.13
INDUSTRY	.93	.86	–.02	.09

[a] Since each of the variables is standardized, the path coefficients are identical to the regression coefficients.

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[31] And, because they have been aggregated, we further assume that they tend to bias upwardly the individual-level relationship with the Peronist vote. See C. Gehkle and K. Biehel, "Certain Effects of Grouping upon the Size of the Correlation Coefficient in Census Tract Material," *Journal of the American Statistical Association Supplement,* 29 (1934), 169–70; G. Udny Yule and Maurice G. Kendall, *An Introduction to the Theory of Statistics* (London: C. Griffin, 1950), p. 311.

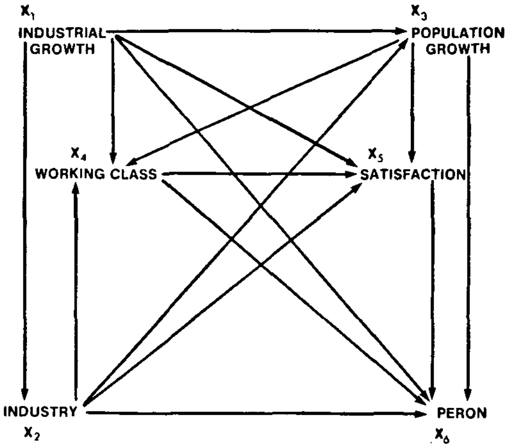

Figure 1. Six-Variable Causal Model: Paths from Industrial Growth to the Peronist Vote

SATISFACTION. Table 14 indicates that industrial growth and working-class population totally dominate this six-variable model of Peronist electoral behavior.

Given the nature of the data it is extremely difficult to separate the influence of industrial growth and population growth upon Peronist voting patterns. Although these two variables undoubtedly reinforce one another, one intuitively suspects that an area's population would grow in response to industrial growth rather than vice versa. In any event, it is upon this logic that we must rely, for the POPULATION GROWTH-INDUSTRIAL GROWTH correlation of +.84 renders the magnitudes of the regression coefficients nearly impossible to assess.

One of the most intriguing questions raised by this model is the extent to which the rate of industrial growth directly influences the Peronist vote. In Table 15 an attempt has been made to determine the relative influence among the various paths from INDUSTRIAL GROWTH to PERON by first arriving at a basic correlation between industrial growth and PERON by summing the pathways of greatest theoretical interest. These pathways are the products of the path coefficients along each given path. With the basic correlation determined, the proportion of the correlations between INDUSTRIAL GROWTH and PERON attributable to various pathways can be calculated.[32] Nearly 52 percent of the correlation is accounted for by a direct linkage. Linkages passing through the demographic variable (POPULATION GROWTH) and the other economic variables (INDUSTRY and SATISFACTION) scarcely appear to affect the relationship between industrial growth and the Peronist vote. More than 43 percent of the basic INDUSTRIAL GROWTH-PERON correlation may be attributed to a path through WORKING CLASS, the single social structure variable.

Figure 2 is a simplified version of the same model which reflects the relative importance of the various independent variables. All path coefficients of less than .200 have been omitted; all those of more than .200 but less than .500 are indicated by broken lines; and all those of .500 and over are indicated by solid lines. The three independent variables which maintain direct links to PERON in Figure 2 (INDUSTRI-

Table 14. Path Coefficients for Figure 1

Dependent Variable Equation	B_1	B_2	B_3	B_4	B_5
$X_6 = B_{61}X_1 + B_{62}X_2 + B_{63}X_3 + B_{64}X_4 + B_{65}X_5 + e_6$	.512	–.071*	.044*	.709	.201
$X_5 = B_{51}X_1 + B_{52}X_2 + B_{53}X_3 + B_{54}X_4 + e_5$	.029*	.288*	–.236*	–.607	
$X_4 = B_{41}X_1 + B_{42}X_2 + B_{43}X_3 + e_4$	.605	.564	–.066*		
$X_3 = B_{31}X_1 + B_{32}X_2 + e_3$	.830	–.031*			
$X_2 = B_{21}X_1 + e_2$	–.208				
$X_1 = e_1$					

X_1 = INDUSTRIAL GROWTH
X_2 = INDUSTRY
X_3 = POPULATION GROWTH
X_4 = WORKING CLASS
X_5 = SATISFACTION
X_6 = PERON
*Path coefficients with associated regression coefficients less than twice their standard errors.

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[32] The format for Table 15 is a modified copy of Table 1 in Norman H. Nie, G. Bingham Powell, Jr., and Kenneth Prewitt, "Social Structure and Political Participation: Developmental Relationships, II," *American Political Science Review*, 63 (September 1969), 812.

AL GROWTH, WORKING CLASS, and SATISFACTION) account for all but one percent of the total variation in the Peronist vote that can be attributed to these socioeconomic variables. The addition of INDUSTRY and POPULATION GROWTH increases the coefficient of determination from .856 to .863.

It is difficult to overestimate the combined influence of an area's rate of industrial growth and the size of its working-class population upon Peronism's electoral behavior. By themselves they explain 84 percent of the total variation in PERON, or slightly more than 97 percent of the variation in PERON explained by all five independent variables.

Conclusion

These data from Argentina Peronism provide a number of insights into the socioeconomic determinants of popular-authoritarian electoral behavior. Prime among these is the crucial role assumed by industrial growth in (1) directly contributing to an area's popular-authoritarian voting strength, and (2) inducing changes in an area's social class composition which, in turn, also dramatically affect the vote for popular-authoritarian political movements.

One of the most persistent questions to arise out of the theoretical literature on social and political mobilization in the Third World concerns the specific impact of economic variables upon political behavior. At the center of this study is an inquiry into the direct and indirect ability of one crucial variable—the rate of industrial growth—to produce a particular type of political participation, i.e., electoral support for popular-authoritarian movements. If the Argentine data may be extrapolated to other relatively modern Third World nations, it appears that industrial growth not only exerts an impressive direct influence upon the level of the popular-authoritarian vote, but substantially (beta = +.605) adds to an area's working-class population as well. Once this has occurred, these two independent variables then explain an extremely large percentage (in Argentina, 84 percent) of the vote for popular-authoritarian political movements.

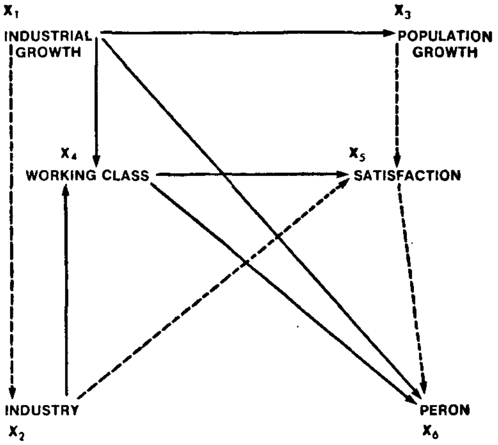

Figure 2. Six-Variable Causal Model: Selected Paths from Industrial Growth to the Peronist Vote

The Argentine case also provides insight into the negligible influence of other variables often considered of primary importance in determining the popular-authoritarian vote. An area's level of industrial development, in particular, maintains no strong direct relationship with its popular-authoritarian electoral strength, although it does tend to exhibit a significant indirect influence through its ability to attract large numbers of working-class voters. This may

Table 15. Proportional Components of the Relationship between Industrial Growth and the Peronist Vote (Greater Buenos Aires)

Proportion of Basic Correlation Attributable to:		Path Coefficients	
1. Direct link to PERON		.512	52.0%
2. Indirect link through demographic variable:			
INDUSTRIAL GROWTH → POPULATION GROWTH → PERON		.036	3.7
3. Indirect links through other economic variables:			
INDUSTRIAL GROWTH → INDUSTRY → PERON	.014		
INDUSTRIAL GROWTH → INDUSTRY → SATISFACTION → PERON	–.011		
INDUSTRIAL GROWTH → SATISFACTION → PERON	.005		
		.008	0.8
4. Indirect link through WORKING CLASS		.428	43.5
Basic correlation between INDUSTRIAL GROWTH and PERON		.984	100.0%

seem so obvious as to merit scant mention, but factory experience per se is believed to be a crucial link in the creation of a participant working class. Factories are Inkeles' "schools for citizenship."[33] Thus the absence of a relationship between industrial development and popular authoritarianism suggests either that the factory does not develop participant citizens or, more likely given the thoroughness of Inkeles' study, that the factory creates styles of participation which, relatively speaking, reject popular authoritarianism. While the data in this paper hardly bear comparison to those of Inkeles and Smith, they do provide the foundation for justifying future analyses of an entirely unexpected association.

These data also suggest an interesting insight into the relationship between a demographic variable, population growth, and popular-authoritarian electoral behavior. No doubt part of the negligible direct association between Argentine population growth and the vote for Peronism can be attributed to the inability of these data to distinguish between growth due to migration and nonmigratory increases in the resident population (see note 23). But population growth has long been thought to be closely linked to an area's vote for popular authoritarianism, for growth transmutes all too easily into instability and, until recently, demographic instability in Third World environments carried with it the specter of anomic disposable masses willing to trade their votes and multiple anxieties for strong leadership from charismatic politicians. Now that we are aware of the pitfalls inherent in a macro-micro leap from growth to anomic political behavior (see note 29), there is no reason to expect to discover a direct association between population growth and the vote for popular-authoritarian political movements, and none was found in Argentina.

Finally, because these data appear to explain such a large proportion of the popular-authoritarian vote that may be attributed to socioeconomic variables, they provide the opportunity to suggest that future research into the phenomenon of popular-authoritarian mass support might now concentrate upon the amount of the total variation in the popular-authoritarian vote that can be explained by socioeconomic variables. This would involve the testing of models in which the variables presented here share prominence with other variables, particularly party organization and the differential impact of the often-mentioned *caudillo* ethic. Unlike many socioeconomic variables, most of these variables can only be approached with reasonable confidence through the use of survey data. Fortunately, these data are slowly increasing in quality, quantity, and availability.[34] Just as we have progressively refined our knowledge of the socioeconomic determinants of the popular-authoritarian vote since Germani's 1955 analysis of Peronist electoral behavior, so we might now begin to assess the relative worth of that knowledge in a broader universe of independent variables.

Appendix A: The Source and Nature of Peronist Electoral Data

The electoral data presented here are for all 209 *circuitos*, or precincts, in the Federal Capital (Buenos Aires) between 1942 and 1973. They were obtained from the *Secretaría Electoral de la Capital Federal.* With the exception of the data for 1957 and 1973, all of the electoral data from the Federal Capital were calculated by adding the vote of every party for every *mesa* of every *circuito* in eight elections between 1942 and 1973. The number of parties varied from 2 to 26, and the number of *mesas* from 3500 to more than 7000. Electoral personnel aggregated the 1957 and 1973 data to the *circuito* level.

The 39 units of Greater Buenos Aires comprise the second data set used in this article. They are the sum of the Federal Capital's 20 *secciones*, or wards, and the 19 *partidos*, or counties, which together with the River Plate surround the Federal Capital. (Two terms are employed to indicate the Argentine equivalent of a U.S. county. The province of Buenos Aires alone retains a semblance of Hispanic purity by the use of *partido;* elsewhere in this land of Francophiles only *departamento* is officially acceptable. The Federal Capital's *secciones* are commonly referred to as *circunscripciones.*) While interesting because of the large number of variables available for analysis, the units themselves vary considerably in size and are much larger than the Federal Capital's precincts.

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[33] Alex Inkeles, "Participant Citizenship in Six Developing Countries," *American Political Science Review,* 63 (December 1969), 1137; Alex Inkeles and David H. Smith, *Becoming Modern: Individual Change in Six Developing Countries* (Cambridge, Mass.: Harvard University Press, 1974).

[34] Frederick C. Turner, "The Study of Argentine Politics Through Survey Research," *Latin American Research Review,* 10 (Summer 1975), 90–100.

Each of the eight elections was unique. The 1946 presidential election is studied in the data sets from both the Federal Capital and Greater Buenos Aires. By the time of the 1951 presidential election, however, Argentina had temporarily switched to single-member districts unrelated to county boundaries, and so only votes in the Federal Capital's unchanged precincts are available for analysis. As a surrogate, the 1954 congressional elections are utilized for Greater Buenos Aires. Following the 1955 *Revolución Liberatadora,* isolation of the Peronist vote becomes impossible at times. For this reason the 1958 presidential election (in which the Peronists were ordered to vote for the UCRI candidate, Arturo Frondizi) and the 1963 presidential election (in which many Peronists joined *frondizistas* and other small groups in casting blank ballots) are excluded. All blank ballots cast in the 1957 special election for delegates to a constitutional convention are considered Peronist, as are the blank ballots cast in the 1960 election for members of the Chamber of Deputies. While it is widely believed that many Peronists broke party discipline and voted for one candidate or another in both elections, and while a certain portion of the non-Peronist electorate also cast blank ballots (particularly the Communists in 1957), the overwhelming majority of blank ballots in 1957 and 1960 was Peronist. In elections for representatives to the Chamber of Deputies in 1962 and 1965, Peronists sought office under the banner of the *Unión Popular.* In 1962 the picture is complicated by the facts that the very small *Partido Socialista Argentino de Vanguardia* supported Peronist candidates in the Federal Capital and that the neo-Peronist *Partido Tres Banderas* dropped out of the contest in the Federal Capital two days prior to the election but after its ballots had become available. In 1965 two minor parties (the *Alianza de la Justicia Social* and the *Movimiento Las Flores-Luján*) supported candidates of the Peronist *Unión Popular.* Finally, the September 1973 election was the one in which Juan Perón once more captured the Argentine presidency.

Outside Greater Buenos Aires data are from the 1946 presidential election, the 1954 congressional election, the 1957 constitutional convention election, and the 1960, 1962, and 1965 congressional elections. While it is commonly agreed that blank ballots indicate the Peronist vote in 1957 and 1960, it is doubtful whether agreement will ever be reached on a definitive list of Peronist parties for 1962 and 1965. Nevertheless, votes for the following parties represent a fairly accurate estimate of Peronism's electoral strength in 1962: *Unión Popular* (Federal Capital and Buenos Aires province), *Partido Populista* (Catamarca, Corrientes, Chubut, Santa Cruz), *Partido Laborista* (Córdoba, Jujuy, La Pampa, Santa Fe, Tucumán), *Movimiento Cívico Bandera Popular* (Chaco), *Partido de la Provincia de Chubut* (Chubut), *Partido Tres Banderas* (Entre Ríos, Mendoza, Santa Fe, Santiago del Estero), *Partido Blanco* (Mendoza, Río Negro), *Partido Justicialista* (Misiones), *Movimiento Popular Neuquino* (Neuquén), *Partido Laborista Nacional* (Salta). Peronist votes were indistinguishable from those of other parties with whom alliances were formed in Formosa, La Rioja (both with the *Partido Demócrata Cristiano*), San Juan (*UCR Bloquista*), and San Luis (*Partido Demócrata Liberal*). Five provinces held their elections before the March 18 general election date: Catamarca, Santa Fe, and San Luis (all on December 17, 1961), Formosa (January 14, 1962), and La Rioja (February 25, 1962).

The 1965 Peronist parties are the following: *Unión Popular* (Federal Capital, Buenos Aires, Catamarca, Córdoba, Corrientes, Chaco, Chubut, Entre Ríos, Formosa, La Pampa, La Rioja, Mendoza, Misiones, Salta, San Juan, San Luis, Santa Cruz, Santa Fe, Santiago del Estero, Tucumán), *Partido Tres Banderas* (Entre Ríos, Santiago del Estero), *Movimiento Popular Mendocino* (Mendoza), *Movimiento Popular Neuquino* (Neuquén), *Partido Justicialista* (Salta, Mendoza), *Partido Laborista* (Salta), *Partido Blanco* (Río Negro, Salta, Santa Fe), *Acción Provinciana* (Tucumán), *Partido de la Justicia Social* (Córdoba, Santa Fe, Tucumán), *Alianza de la Justicia Social* (Buenos Aires), *Movimiento Las Flores-Luján* (Buenos Aires), *Unión Provincial* (Misiones), *Movimiento de la Justicia Social* (Misiones). There were no elections in Jujuy. In 1963 the newly elected members of the Chamber of Deputies drew lots to determine which half would have two-year and which half would have normal four-year terms in office. Five provinces (Catamarca, Formosa, La Rioja, Misiones, and Salta) had deputies who all drew four-year terms, and so their 1965 election results are for provincial deputies.

A composite dependent variable PERON is introduced in Table 4 and utilized thereafter to simplify data presentation. The first step in the construction of PERON was to factor analyze the seven dependent variables representing the percentage vote for Peronism in 1946, 1951 (or 1954), 1957, 1960, 1962, 1965, and 1973. Using the single factor score coefficient matrix produced by the analysis, the composite variable PERON was computed by summing the products of each variable's standardized score and its factor score coefficient (see Table A1). Table A2 contains the simple Pearson correlations between PERON and the separate elections' Peronist vote in the Federal Capital and Greater Buenos Aires.

Appendix B: The Sources and Nature of the Socioeconomic Variables

The independent socioeconomic variables have also been separated into two distinct data sets, the Federal Capital's 209 precincts and Greater Buenos Aires' 39 wards and counties. Additionally, on two separate occasions reference is made to the influence of two selected independent variables—the level of industrialization and the rate of industrial growth—upon the Peronist vote in the Argentine counties outside Greater Buenos Aires.

The precinct-level data from the Federal Capital refer only to social class as measured by occupation. I believe they are the most reliable data available for use in the ecological analysis of Argentine electoral behavior. These data were obtained from a ten percent random-start, fixed-interval sample of registration forms for all male voters as of October 1959. Then, using the same precinct tables (*mesas*) sampled in 1959, a further ten percent sample was made of the 1945 voter registration lists (that is, ten percent of the 1959 ten percent) and very few changes in the occupational structure of the Federal Capital's precincts were found to have occurred during the intervening 14 years. Working-class areas, in particular, maintained an extremely stable class structure. Martínez Estrada noted long ago that the class ecology of Buenos Aires had adopted its fundamental outlines by the turn of the century.[35]

In Argentina male voter registration is tied to registration for military service, and a very severe sanction (up to an extra year of military duty) is imposed upon those who fail to register within three months of their eighteenth birthday. Because they exclude so few males, the registration lists provide an unusually accurate estimate of a precinct's occupational structure.[36] It is impossible to obtain a stratified female sample. More than 50 percent of registered females are classified as housewives or unemployed, and a large majority of the

Table A1. Factor Score Coefficients and Component Variables of PERON

Component Variable	Factor Score	Mean	Standard Deviation
% vote for Peronism, 1946	.14	55.8	8.27
% vote for Peronism, 1954	.15	59.2	8.64
% vote for Peronism, 1957	.15	22.1	6.97
% vote for Peronism, 1960	.15	25.9	7.76
% vote for Peronism, 1962	.15	32.3	9.94
% vote for Peronism, 1965	.15	38.6	8.77
% vote for Peronism, 1973	.15	58.4	13.22

Table A2. Pearson Correlations between PERON and Its Component Variables

	PERON	
Election	Greater Buenos Aires	Federal Capital
1946	.89	.90
1951 (1954)	.97	.95
1957	.97	.95
1960	.96	.96
1962	.99	.92
1965	.98	.97
1973	.96	.93
N =	37	209

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[35] Ezequiel Martínez Estrada, *Radiografía de la pampa*, Vol. 2, 3rd ed. (Buenos Aires: Editorial Losada, 1946), p. 30.

[36] Not everyone shares this evaluation of the data's reliability. See Germani, "El surgimiento del peronismo," p. 437–38.

remainder apparently is engaged in some aspect of dressmaking. This activity is subdivided into as many as 30 processes, each claiming a rather ambiguous rank on a nearly incoherent occupational pecking order.

With the assistance of more than a dozen employees of the *Secretaría Electoral* and several Argentine and United States social scientists living in Buenos Aires, six occupational categories were rank-ordered according to contemporary Argentine standards of social stratification. Of highest status is the "professional" category—attorneys, doctors, engineers, owners or managers of large enterprises, persons supported by their investments, etc. Argentine professionals appear to occupy the same relative social position as the United States' upper middle class. No attempt was made to identify the upper class; presumably its members are included in the data as professionals. Next in social status are students. Given the orientations of Argentina's universities, students who pursue their studies until graduation become professionals. For this reason it is not surprising that the ecological correlation between percent professionals and percent students is +.65. Businessmen are third on the scale. This is an extremely vague category for all types of middle-class men who are self-employed in the broadest possible sense. Readers familiar with the varied use of *comerciante* will appreciate the imprecision of this category. Fortunately, there are relatively few self-declared businessmen in Buenos Aires; in no precinct are they more than nine percent of the registered male population and the mean is slightly less than five percent. "Employees" are white-collar workers, "workers" are blue-collar workers, and "unskilled workers" are *peones*.

Treatment of the independent variables in the second data set, that of Greater Buenos Aires, proved infinitely more difficult. The fundamental problem is multicollinearity: several of the *conceptually distinct* variables tend to vary together. This is only natural, or course, for economic change can hardly occur without demographic instability or industrialization. Nor are large numbers of urban working-class citizens able to cluster together in zones of temporary huts with the same population density of the zones of middle and upper-class high-rise apartment complexes, for flat terrain and relatively efficient mass transportation encourage extensive rather than intensive land use by the Buenos Aires working class.

As expected, therefore, an initial factor analysis of the 26 variables employed in this study yielded five rather unclear orthogonal factors (see Table B1). Because the factors appeared so difficult to interpret and, more importantly, because this was to be a test of hypotheses from a variety of previous analyses, the factors in Table B1 were bypassed and the variables were separated into six groups (WORKING CLASS, POPULATION GROWTH, URBANIZATION, SATISFACTION, INDUSTRY, and INDUSTRIAL GROWTH) according to prior usage. When prior usage proved inconsistent among authors, decisions were made by appealing to political science's ever-ambiguous court of last resort, common sense. No reader will agree with all of the groupings, although some (particularly WORKING CLASS, POPULATION GROWTH, and INDUSTRIAL GROWTH) will be far less controversial than others.

Factor analyses then explored the relationships among the composite groups. Results included verification of the differences that underlie the separation of POPULATION GROWTH and URBANIZATION (see Table B2). Internally, the six groups were considered relatively homogeneous when principal factoring without iteration produced only one factor with an eigenvalue greater than unity. Then the six composite variables were constructed in the same manner as PERON, by summing the products of each variable's standardized score and its factor score coefficient.

At this point the problem of multicollinearity among WORKING CLASS, POPULATION GROWTH, URBANIZATION, and INDUSTRIAL GROWTH became acute. The zero-order Pearson correlation between WORKING CLASS and POPULATION GROWTH was +.71; between WORKING CLASS and URBANIZATION it was −.79; and between POPULATION GROWTH and INDUSTRIAL GROWTH it was +.79. These correlations would make several partial correlations with PERON all but meaningless, and so factor analysis was employed once more, this time using only the 13 variables grouped into WORKING CLASS, POPULATION GROWTH, URBANIZATION, and INDUSTRIAL GROWTH. The results of an orthogonal analysis were disappointing, but an oblique (oblimin) rotation produced three distinct factors that could be clearly labeled WORKING CLASS, POPULATION GROWTH, and URBANIZATION. The four INDUSTRIAL GROWTH variables loaded randomly and were eliminated from further factoring. The nine WORKING CLASS, POPULATION GROWTH, and URBANIZATION variables were factored once more with an oblique rotation, and the results are presented in Table B3. Then the composite variables WORKING CLASS, POPULATION GROWTH, and URBANIZATION were re-computed using the newly generated factor score coefficients. By this method the original WORKING CLASS-POPULATION GROWTH correlation of +.71 was reduced to +.35, the WORKING CLASS-URBANIZATION correlation of −.79 to −.16, and the URBANIZATION-POPULATION GROWTH correlation of −.38 to −.30. The redefinition of POPULATION GROWTH led to an increase in the POPULATION GROWTH-INDUSTRIAL

Table B1. Orthogonally (Varimax) Rotated Factor Matrix[a] for All Independent Variables

Variables	Factor 1	Factor 2	Factor 3	Factor 4	Factor 5
1. % Industrial workers, 1947[c]	(.58)[b]	.37	(−.54)	.03	.00
2. % Industrial workers, 1960	.17	(.78)	−.13	.48	.00
3. % Illiterates, 1960	.17	.47	−.32	(.63)	.13
4. % Population growth, 1914–1947	−.08	(.87)	−.19	.11	−.23
5. % Population growth, 1947–1960	−.15	(.66)	.01	(.61)	.00
6. Population density increase, 1914–1947	−.09	(.87)	−.19	.12	−.23
7. Population density increase, 1947–1960	.06	(.72)	−.35	.33	−.11
8. % Urban population, 1947	.20	−.13	−.03	(−.90)	−.04
9. % Urban population, 1960	.19	−.01	.06	(−.86)	−.01
10. Wages per industrial employee, 1935	−.07	−.13	(.88)	−.16	.16
11. Wages per industrial employee, 1941	.06	(−.51)	(.82)	−.02	−.12
12. Wages per industrial employee, 1947	.12	−.28	(.89)	.04	.08
13. Workers per industrial establishment, 1935	(.81)	−.09	−.09	−.14	.32
14. Workers per industrial establishment, 1941	(.92)	−.04	−.23	−.10	−.01
15. Workers per industrial establishment, 1947	(.92)	.06	−.18	−.04	.22
16. Workers per industrial establishment, 1963	(.63)	−.20	.08	.04	(.63)
17. Wages per industrial establishment, 1935	(.73)	−.15	.39	−.20	.30
18. Wages per industrial establishment, 1941	(.90)	−.24	.18	−.12	−.04
19. Wages per industrial establishment, 1947	(.93)	−.06	.17	−.04	.22
20. Wages per industrial establishment, 1963	.46	−.06	.20	.03	(.77)
21. Value of production per industrial establishment, 1947	(.97)	−.01	.03	−.06	.12
22. Value of production per industrial establishment, 1963	(.50)	.02	−.03	.15	(.73)
23. Increase in industrial establishments, 1914–1947	−.10	(.89)	−.16	−.29	−.07
24. Increase in industrial establishments, 1947–1963	−.04	(.80)	−.25	.36	.16
25. Increase in industrial workers, 1914–1947	−.16	(.90)	−.15	−.11	.11
26. Increase in industrial workers, 1947–1963	−.24	(.68)	−.02	.46	.37
Eigenvalues	6.96	6.85	3.31	3.27	2.19
% Total variation	26.77	26.37	12.75	12.56	8.43
% Common variation	31.06	30.59	14.79	14.57	9.78

[a] Principal factoring without iteration using as the extraction criterion factors with eigenvalues greater than unity.

[b] Factor loadings ⩾ .50 shown in parentheses.

[c] See Tables B4–B9 for complete variable label.

Table B2. Orthogonally (Varimax) Rotated Factor Matrix[a] for Demographic Variables

Variables	Factor 1	Factor 2
1. Average annual percent population growth, 1914–1947	(.97)[b]	−.08
2. Average annual percent population growth, 1947–1960	(.59)	(−.66)
3. Average annual change in population density, 1914–1947	(.97)	−.08
4. Average annual change in population density, 1947–1960	(.84)	−.29
5. Urban population as a percent of total population, 1947	−.17	(.90)
6. Urban population as a percent of total population, 1960	−.03	(.93)
Eigenvalues	2.96	2.21
% Total variation	49.40	36.78
% Common variation	57.11	42.52

[a] Principal factoring without iteration using as the extraction criterion factors with eigenvalues greater than unity.

[b] Factor loadings ⩾ .50 shown in parentheses.

GROWTH correlation of +.79 to +.84. The independent variable URBANIZATION was subsequently eliminated from the analysis due to insufficient variation.

The five composite independent variables were constructed as indicated in Tables B4 through B8. Because each of the 26 variables is standardized, the factor score coefficients determine the relative impact of each component variable upon the composite variable.

Table B9 contains the first and second-order partial correlations between PERON and each of the five composite independent variables. Table B10 is an independent composite variable correlation matrix.

Table B3. Oblique Primary Factors of Nine Socioeconomic Variables[a]

	Pattern			Structure		
Variable	**Factor 1**	**Factor 2**	**Factor 3**	**Factor 1**	**Factor 2**	**Factor 3**
1. % Industrial workers, 1947[b]	.06	.22	(.95)	.33	.06	(.93)
2. % Industrial workers, 1960	(.52)	–.36	.43	(.77)	(–.57)	(.67)
3. % Illiterates, 1960	.10	(–.51)	(.67)	.48	(–.64)	(.78)
4. % Population growth, 1914–1947	(.99)	.08	.08	(.98)	.21	.34
5. % Population growth, 1947–1960	(.51)	(–.60)	.00	(.69)	(–.75)	.28
6. Population density increase, 1914–1947	(.99)	.07	.00	(.97)	–.22	.34
7. Population density increase, 1947–1960	(.73)	–.16	.21	(.85)	–.41	.49
8. % Urban population, 1947	–.10	(.91)	.13	–.32	(.92)	–.05
9. % Urban population, 1960	.13	(.92)	–.04	–.15	(.89)	–.14
	(Population Growth)	(Urbanization)	(Working Class)	(Population Growth)	(Urbanization)	(Working Class)

[a] Oblimin ($\delta = -.5$) solution. Initial criterion value 5.65, final criterion value 3.64. Loadings and coefficients ⩾ .50 shown in parentheses.

[b] See Table B4 for complete variable label.

Table B4. Factor Score Coefficients and Component Variables of WORKING CLASS

Component Variable	Factor Score
1. Industrial workers as a percent of economically active population, 1947	.630
2. Industrial workers as a percent of economically active population, 1960	.225
3. Illiterates as a percent of total population, 1960	.410
4. Average annual percent population growth, 1914–1947	–.087
5. Average annual percent population growth, 1947–1960	–.078
6. Average annual change in population density, 1914–1947	–.087
7. Average annual change in population density, 1947–1960	.065

Table B5. Factor Score Coefficients and Component Variables of POPULATION GROWTH

Component Variable	Factor Score
1. Industrial workers as a percent of economically active population, 1947	–.049
2. Industrial workers as a percent of economically active population, 1960	.106
3. Illiterates as a percent of total population, 1960	–.068
4. Average annual percent population growth, 1914–1947	.352
5. Average annual percent population growth, 1947–1960	.153
6. Average annual change in population density, 1914–1947	.351
7. Average annual change in population density, 1947–1960	.223

Table B6. Factor Score Coefficients and Component Variables of SATISFACTION

Component Variable	Factor Score
1. Wages per industrial employee, 1935	.358
2. Wages per industrial employee, 1941	.370
3. Wages per industrial employee, 1947	.370

Table B7. Factor Score Coefficients and Component Variables of INDUSTRIAL GROWTH

Component Variable	Factor Score
1. Average annual increase in industrial establishments, 1941–1947	.273
2. Average annual increase in industrial establishments, 1947–1960	.296
3. Average annual increase in industrial blue-collar workers, 1941–1947	.299
4. Average annual increase in industrial blue-collar workers, 1947–1960	.270

Table B8. Factor Score Coefficients and Component Variables of INDUSTRY

Component Variable	Factor Score
1. Blue-collar workers per industrial establishment, 1935	.120
2. Blue-collar workers per industrial establishment, 1941	.124
3. Blue-collar workers per industrial establishment, 1947	.130
4. Blue-collar workers per industrial establishment, 1963	.129
5. Wages per industrial establishment, 1935	.119
6. Wages per industrial establishment, 1941	.128
7. Wages per industrial establishment, 1947	.134
8. Wages per industrial establishment, 1963	.107
9. Value of production per industrial establishment, 1947	.129
10. Value of production per industrial establishment, 1963	.097

Table B9. Partial Correlations between PERON and the Composite Independent Variables

	Working Class	Population Growth	Satisfaction	Industry	Industrial Growth
Zero order:					
PERON	.77	.69	–.47	.15	.79
First order controlling for:					
WORKING CLASS		.70	–.11	–.34	.79
POPULATION GROWTH	.77		–.23	.41	.54
SATISFACTION	.69	.60		.21	.72
INDUSTRY	.79	.74	–.49		.84
INDUSTRIAL GROWTH	.77	.08	–.15	.48	
Second order controlling for:					
WORKING CLASS, POPULATION GROWTH			.21	–.06	.53
WORKING CLASS, SATISFACTION		.71		–.33	.81
WORKING CLASS, INDUSTRY		.65	.05		.76
WORKING CLASS, INDUSTRIAL GROWTH		.16	.29	.05	
POPULATION GROWTH, SATISFACTION	.77			.41	.51
POPULATION GROWTH, INDUSTRY	.72		–.25		.60
POPULATION GROWTH, INDUSTRIAL GROWTH	.77		–.14	.49	
SATISFACTION, INDUSTRY	.71	.67			.79
SATISFACTION, INDUSTRIAL GROWTH	.78	.07		.49	
INDUSTRY, INDUSTRIAL GROWTH	.68	.15	–.17		

Table B10. Independent Composite Variable Product-Moment Correlation Matrix

	Working Class	Population Growth	Satisfaction	Industry	Industrial Growth
WORKING CLASS	1.00	.35	–.54	.45	.43
POPULATION GROWTH		1.00	–.47	–.21	.84
SATISFACTION			1.00	.08	–.50
INDUSTRY				1.00	–.18
INDUSTRIAL GROWTH					1.00

Spatial Voting Models for the French Fifth Republic*

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In this study formal spatial models are applied to cross-sectional analysis of district results on the second ballot of French legislative elections. A model of probabilistic spatial voting better accounts for the data than either standard "ecological" models or a model of deterministic spatial voting. There are three substantive findings concerning voter behavior. First, the adjustment of voters to external information can be largely viewed as a shift in the spatial (Left-Right) distribution of voters. This shift, plus decisions by parties and candidates as to which districts parties will contest, determines the first ballot outcome. In arriving at second ballot choices, voters then appear to utilize decision rules that have a substantial degree of temporal stability. A second and related finding is that the second ballot can be reasonably accounted for by a single Left-Right dimension. Third, in those districts with three or more candidates on the second ballot, there may be substantial strategic voting with voters switching from candidates close to their ideal points but unlikely to win to more distant candidates who are more likely to win. The existence of strategic voting is suggested by the finding that models based solely on spatial preferences perform well for two-candidate districts, but less well for three- or four-candidate districts.

The legislative elections of the French Fifth Republic provide an advantageous setting for empirical tests of formal spatial models of voter behavior. France utilizes a two ballot general election. In districts where no candidate obtains a majority on the first ballot, a second ballot is held one week later. Any candidate receiving more than ten percent of the first ballot vote is eligible for the second ballot (rather than just the top two candidates as in runoff primaries in the United States).[1] Eligible candidates may elect not to run on the second ballot. When they do drop out, they typically endorse one of the remaining candidates. We report three substantive findings concerning voter behavior in the French electoral process.

The first finding is that the adjustment of voters to external information can be largely viewed as a shift in the spatial (Left-Right) distribution of voters.[2] This shift, plus decisions by parties and candidates as to which districts parties will contest, determines the first ballot outcome. In arriving at second ballot choices, voters then appear to utilize decision rules that have a substantial degree of temporal stability. The external information is largely incorporated in the first ballot shift and need not be considered in a model of the second ballot. To a "reasonable" degree of accuracy, the second ballot can be forecast on the basis of first ballot results and party decisions as to which eligible candidates will in fact contest the second ballot.[3] For example, voters appear to have responded to the student-worker demonstrations of May 1968 with a shift to the Right. Once voters located their ideal points on the Left-Right continuum for 1968, however, their second ballot behavior in the June election was close to identical to that of voters with the same ideal points in the more serene circumstances of 1967.

A second and related finding is that the second ballot can be reasonably accounted for by a single Left-Right dimension. We do not

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[1] The second ballot eligibility percentage was 5 percent in the 1958 and 1962 elections.

[2] Party positions are viewed as fixed over time. It should be recognized, however, that a shift of voters is, to a large extent, observationally equivalent to a shift in party positions.

[3] Candidates eliminated from the second ballot may, and usually do, counsel their first ballot supporters about which candidates to support on the second ballot. A plurality wins the second ballot.

deny that a variety of foreign and domestic issues are avidly discussed in the campaign. Thus there are many dimensions. If we are able to use just one, it is partly because each of the other issues is, in varying degree, correlated with the underlying Left-Right conflict. Further, most specific issues, such as Middle East policy or regional decentralization will be salient only to small minorities of voters. Consequently, most specific issues can be ignored when considering district-level results. One general dimension is adequate. Multidimensional models might improve our results, but such improvements should be minor.

Third, we find, in those districts with three or more candidates on the second ballot, that French voters may engage in significant strategic voting, switching from less viable candidates close to their ideal point to more viable candidates. Gaullist supporters, for example, might switch to a Centrist to insure defeat of a Socialist.[4] Specifically, strategic voting is suggested by the finding that our spatial, preference-based models perform well for two-candidate districts, but less well for three- or four-candidate districts.[5]

In presenting formal models of the second ballot process, we rely on methods developed in an earlier article for approximating candidate locations and voter preference distributions and for modeling abstention and spoiled (blank) ballot behavior. That research covered the first four elections of the Fifth Republic, those of 1958, 1962, 1967, and 1968.[6] For the same elections, we now develop and estimate models to analyze the *vote proportions* received by various political parties on the second ballot. Estimation is for actual voting data at the legislative district level.[7] As a postscript, we replicate and validate our preferred model, the "all-candidates" model, with data from the 1973 elections. This data became available subsequent to the initial writing and presentation of this paper.

We will first discuss several features of the French political system which facilitate the development of formal voting models and then present the set of criteria used to evaluate the models in this paper. Subsequently, we describe the models in detail and present empirical results for each model. There are two models motivated by spatial theory—the "a-priori" and the "all-candidates" models. In both models, voters vote probabilistically. Voters have a higher probability of voting for a close candidate than for a distant one. The models differ as much in the technical aspects of functional forms as in substantive spirit. The main substantive difference is that the "all-candidates" model permits voter decision rules to vary with spatial location. Thus, leftists need not have the same parameters as rightists. In contrast, for the a-priori models, all voters have the same decision rules; their only differences are in ideal points. Performance of these models is compared to that of two null models—a heuristic spatial model, the "closest-candidate" model, and the standard nonspatial "ecological" (constant proportions) model. Following a tactic employed in our nonvoting article, the earlier elections of 1958 and 1962 are used for model development, evaluation, and experimentation with various functional forms. On the basis of this analysis, we concluded in favor of the all-candidates model. Only this model was analyzed for the three latest elections.

Features of the French Political System That Facilitate Model Construction

The existence of the two ballot system is very helpful in model construction since the first ballot voting patterns provide important information that can be incorporated in models of voter behavior on the second ballot. We start by assuming that voter preferences are revealed on the first ballot. In other words, an individual voting for the Communist candidate (for example) on the first ballot is assumed to prefer the Communist candidate relative to the other candidates on the first ballot.[8]

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[4] That is, voters must consider not only their preference regarding a candidate but also that candidate's chances of winning the election. For discussions on viability effects, see Richard D. McKelvey and Peter C. Ordeshook, "A General Theory of the Calculus of Voting," in *Mathematical Applications in Political Science,* VI, ed. James F. Hernden and Joseph C. Bernd (Charlottesville, Va.: University of Virginia Press, 1972), pp. 32–78, and the debate between Gerald Kramer and Thomas Casstevens, in "Communications," *American Political Science Review,* 62 (September 1968), 955–56, and "Communications," *American Political Science Review,* 65 (March 1971), 187–89, concerning Casstevens' article, "A Theorem about Voting," *American Political Science Review,* 62 (March 1968), 205–07.

[5] Viability will not affect choice in two-candidate races if voter utility is associated solely with "win-lose" outcomes. In other contexts, where the margin of victory is important to the voter, viability effects may also be present in two-candidate races. See William H. Riker and Peter C. Ordeshook, "A Theory of the Calculus of Voting," *American Political Science Review,* 62 (March 1968), 25–42. Much more direct findings concerning strategic voting are reported in Richard P. Castanias II, Timothy W. McGuire, and Howard Rosenthal, "The French Voter as Rational Man" (Paper prepared for the Third World Congress of the Econometric Society, Toronto, Canada, August 20–25, 1975).

[6] Howard Rosenthal and Subrata Sen, "Electoral Participation in the French Fifth Republic," *American Political Science Review,* 67 (March 1973), 29–54.

[7] Our analysis applies to all metropolitan districts with second ballots, except for Corsica where vote fraud has been common.

As only one week separates the two ballots, it is also reasonably safe to assume that in this week preferences are not substantially modified by the host of exogenous variables that affect preferences and, hence, voting behavior. Consequently, we can examine the short-term essence of an electoral process: how preferences are translated into a decision in the choice situation occasioned by a specific context.[9]

The French multiparty system with its emphasis on a unidimensional Left-Right conflict is crucial to the analysis. Relatively simple survey items can be used to locate the parties on the Left-Right dimension.[10] This article uses the Barnes and Pierce "all" measurements shown in Table 1.[11] And, since mass media and other aspects of modernization have led the legislative elections to turn primarily on national rather than local issues, we can, without too much error, regard candidates as undifferentiated representatives of their national parties, having a spatial location equal to that of the national party.

Evidence that candidates are not strongly differentiated intraparty is available from the Guttman scales developed by David Wood for the last legislature of the Fourth Republic and the first legislature of the Fifth. On Wood's major scales, most deputies seeking reelection had the median scale position of their parties

Table 1. Spatial Positions of French Party Groups and Percentage of Incumbents at Party Median Position on Wood's Major Scales

Party	Barnes and Pierce Spatial Position[a]	Incumbents at Party Median			
		1958		1962	
		%	N[b]	%	N
Communists	0.000	100	127	100	10
PSU	1.230	36	11	–	–
Socialists, 1958 and 1962	1.557	100	66	92	38
Federation of the Left, 1967 and 1968	1.148	–	–	–	–
Radicals, 1958 and 1962	2.049	65	26	3	35
MRP, Centre Démocrate,[c] PDM, Mouvement pour la Réforme, Technique et Démocratie	3.607	98	53	50	48
UNR,[c] UDR, Gaullists	4.670	27	11	76	190
Independents, Independent Republicans,[c] Right	4.180	13	76	11	81
Extreme Right, Alliance Républicaine[c]	5.000	73	22	60	5
Total		75	392	56	407

[a] All measures are normalized so that Communists = 0.0 and Extreme Right = 5.0.

[b] N = number of incumbents.

[c] Labels used by Barnes and Pierce.

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[8] Since voting is a function of both preference and candidate viability, some strategic voting undoubtedly occurs on the first ballot. However, casual empiricism suggests that strategic voting occurs much less often on the first ballot than on the second, decisive ballot. In any event, our models assume that voters always vote for their most preferred candidate on the first ballot.

Our claim here that voters reveal information about their true preferences on the first ballot is to some extent inconsistent with our view, expressed later, that spatially oriented voters vote probabilistically on the second ballot. A more accurate, if perhaps less intuitively appealing, statement of our overall approach is that we assume voters vote probabilistically on the second ballot, the probabilities being functions of the spatial position of the expressed first ballot choice of the voter and the spatial positions of the second ballot candidates.

[9] That is, we deliberately ignore political socialization, campaigns, etc., as determinants of voting behavior.

[10] See Rosenthal and Sen, pp. 35–36.

[11] Samuel H. Barnes and Roy Pierce, "Public Opinion and Political Preferences in France and Italy," *Midwest Journal of Political Science*, 15 (November 1971), 643–60.

(see Table 1).[12] (These scales contained 52 ordinal classes for 1958 incumbents and 37 for 1962; hence, a single scale position is a highly specific location.) Even though the evidence pertains only to the roll-call behavior of incumbents, it does suggest that little is lost by assigning candidates the spatial position attributed to their party on the basis of survey data.

Given the use of national sample surveys to estimate the perceived spatial positions of the candidates, it is still necessary to locate *voters* along the Left-Right dimension. Using the preferences revealed by the first ballot, we can approximate the distribution of voters in each district. As indicated in greater detail in our earlier article, the approximation procedure assigns a probability mass to each of the Left-Right locations occupied by parties represented on the first ballot. This mass is equal to the proportion of valid votes received by the party on the first ballot. We assume the approximation's validity not only for voters but also for those who abstained or cast blank ballots on the first ballot.[13]

The preceding discussion has focused on the substantive features of the political setting that facilitate model building: relative isolation from exogenous variables that affect voter preferences and relatively facile estimation of the spatial positions of candidates and voters. There is also an important statistical feature: the roughly 400 second ballot districts per election present the large number of observations needed for reliable estimation of complex multivariate models.

Design Criteria in Model Construction

Constructing models for voting as well as nonvoting leads to a complex ten-equation model in which different but overlapping sets of observations apply to each equation. The large number of equations stems from our use of eight party groups in addition to abstentions and blank ballots. The overlapping is a consequence of the fact that each party is present in only a subset of the second ballot districts. Observations for all second ballot districts are present only for the abstention and blank ballot equations; the other equations have fewer observations. This complexity suggests the need for a brief statement of our design criteria for model formulation and evaluation.

Theory. Models whose equations are a consequence of a well-specified theory are to be preferred to other types of models. This criterion establishes a preference for this essay's models compared, for example, to a regression of second ballot votes against the first five factors from a factor analysis of a large number of social and economic indicators.

Parsimony. Although there are many ways to consider the parsimony of a scientific model, we here employ three operational standards: (1) the number of parameters that are estimated from the data; (2) the scope of the equations as indicated by the number of districts postdicted by the model—for example, a model which postdicts all parties in all districts has greater scope than a model which excludes some of the data; (3) the number of equations—for example, a model in which a single equation is run for the Communist party has greater parsimony than a model for which there are several Communist equations, the observations having been segregated by number and party affiliation of other candidates.

The "Adding Up" Condition. The probability that registered voters will abstain, the probability that they will cast a blank ballot, and the probabilities with which they vote for the various parties must all sum to 1.0. Theoretical models of individual voter behavior, when viewed across equations, must also have this property. When the theoretical models "add up" at the individual level, the aggregated proportions at the district level will also add to 1.0.

Explanatory Power. Again, although there are a variety of ways in which to evaluate the explanatory power of a model, we employ the conventional criterion of squared error.

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[12] David M. Wood, "Majority vs. Opposition in the French National Assembly, 1956–1965: A Guttman Scale Analysis," *American Political Science Review*, 62 (March 1968), 88–109. In Table 1, the incumbent deputies have been classified by their party affiliation used in the election (as against their parliamentary group in the legislature). Of those seeking reelection in 1958, 75 percent had the median position; in 1962, 56 percent. The only parties with over half the deputies not at the median were, in 1958: the PSU, the UNR, and the Right; in 1962: the Radicals and the Right. Similar results are obtained for the other Wood scales for each legislature.

[13] Rosenthal and Sen, pp. 35–37. The approximation is tolerable because there are typically far fewer candidates on the second ballot than on the first. This implies that the differences of the distances from a voter to the various candidates will be large relative to the measurement error.

Agreement Between Estimated Parameters, Theory, and Substance. Explanatory power is not enough. The signs and magnitudes of the estimated coefficients should agree with those called for by the theory in question. In addition, estimated voting probabilities and proportions should all lie in the interval [0, 1]. Furthermore, estimated proportions should be checked against substantive considerations external to the theoretical model. For example, we would discredit a model which showed 98 percent of the Communist first ballot supporters voting for the Gaullists on the second ballot.[14]

Tradeoffs. Judged against the multidimensional criteria we have outlined, none of the models to be presented can be considered "best." Rather, tradeoffs abound, particularly between parsimony and explanatory power. These will be apparent in the empirical results, which follow presentation of the models.

A-Priori Models

Rather than modeling voters as voting with certainty—if they cast a ballot—for the "closest" candidate, we developed models in which the *probability* of voting for a candidate was a function of candidate distances.[15] Naturally, the probability of voting for the closest candidate should, *ceteris paribus*, be greater than the probability of voting for any other candidate.

In addition to creating models of probabilistic choice among competing candidates, we also needed to model, for each constituency, the probability that a voter at a given spatial location would cast a valid ballot. The appropriate estimates, available from our abstention and blank ballot models,[16] allowed us to estimate the spatial distribution of valid ballot casters (as opposed to the distribution of registered voters). We then had only to build a model of voter behavior *conditional* on the fact that a valid ballot was cast. Our dependent variables are thus proportions of the vote cast *(exprimés)* rather than proportions of registered voters *(inscrits)*.[17]

Our first set of "conditional" models of voter behavior are termed the "a-priori" models because a specific functional form determines voting probabilities as a function of distances and no parameters are estimated beyond those in the abstention and blank ballot equations. An a-priori model is obviously strong on parsimony, and the functional forms chosen guarantee a reasonable theoretical and substantive interpretation and that "adding up" is satisfied.

In developing the a-priori models, we first need to set forth some notation that will be useful throughout the article. Let:

$j = 1, \ldots, J$		index the districts.
$i = 1, \ldots, I$		index the spatial locations of the voters.
$k_j = 1, \ldots, N_{2j}$		index the candidates on the second ballot in district *j*. Where the meaning is clear, the *j* subscript is dropped.
D_{ij}^k	=	the distance from spatial location *i* to candidate *k* in district *j*.
DIS_{ij}^t	=	the distance from spatial location *i* to the *t*-th closest candidate in district *j*. (No tied distances exist in the data.)
PV_{ij}	=	the probability that voters will cast a valid ballot given that they are at location *i* in district *j*.

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[14] We also prefer models for which an analytical solution leads to global minimization of the loss function, in this case squared error. This leads us to regressions with a proportion as the dependent variable. As Theil points out, such regressions can have undesirable econometric properties. However, the fact that the vote proportion is modeled as a sum of proportions aggregated across spatial locations implies that in this case we cannot apply the attractive alternative models Theil discusses (Henri Theil, "A Multinomial Extension of the Linear Logit Model," *International Economic Review*, 10 [October 1969], 251–59). We have to fall back on an assumption of approximate linearity over the range of the proportion under investigation and a reliance on the robustness of the estimates given our large number of observations.

[15] Probabilistic models provide a means of coping with error introduced both in our approximation of the Left-Right distribution and in our use of a single dimension. Discussion of why aggregation problems force the direct expression of probabilities rather than deriving them from an individual utility maximization perspective is contained in Castanias, McGuire, and Rosenthal.

[16] Rosenthal and Sen, pp. 40–52.

[17] We eliminated, using newspaper sources, candidates who had withdrawn from the race but whose names were still on the ballot. The actual dependent variable then becomes the proportion of the vote cast for the "true" candidates. In our earlier paper, we used the heuristic decision rule of eliminating all candidates with less than 1,000 votes on the second ballot. This decision rule eliminated practically all the appropriate candidates for 1958, 1967, and 1968. For 1962, the approximation was somewhat worse. With the 1973 replication, we were forced to rely on the 1,000 vote rule.

P_{ij} = the proportion of registered voters in district j at spatial location i. This is approximated by the first ballot share of the valid ballots obtained by the party with spatial location i. If a party does not have a candidate on the first ballot, $P_{ij} = 0$. If there is more than one candidate from the same party or tendency, their votes are summed. We are guaranteed $\sum_i P_{ij} = 1.0$.

Q_{ij} = the proportion of voters casting valid second ballots in district j that are located at i. Again, $\sum_i Q_{ij} = 1.0$.

$Pr(k)_{ij}$ = the probability that voters at spatial location i vote for candidate (party) k on the second ballot, *given that they cast a valid ballot.*

$V(k)_j$ = the proportion of the valid vote in district j for candidate k.

The a-priori models were designed to satisfy the following two axioms:

1. Voters vote for a candidate at their ideal point with a probability of 1.0. Formally, $Pr(k)_{ij} = 1.0$ if $D^k_{ij} = 0$.

2. When a voter's own party is not represented on the ballot, voting probabilities are ordered with distance to the left or the right of the voter. However, it is possible that voters will vote with higher probability for a candidate on their left (right) who is more distant than a candidate on their right (left). Actually, this possibility occurs only for model II (see below). Otherwise, the greater the distance, the lower the probabilities. To state the axiom formally, let $x(i)$, $x(a)$, $x(b)$ represent the spatial positions on the Left-Right continuum of voter i and second ballot candidates a and b. Then if $x(i) \leqslant x(a) < x(b)$ or $x(b) < x(a) \leqslant x(i)$, then $Pr(a)_{ij} > Pr(b)_{ij}$ or $Pr(a)_{ij} = Pr(b)_{ij} = 0$.

We now describe three a-priori models which satisfy these axioms.

Model I. The first model expresses $Pr(k)_{ij}$ for a specific candidate located at a, as follows:

$$Pr(a)_{ij} = \frac{1/D^a_{ij}}{\sum_{k=1}^{N_{2j}} 1/D^k_{ij}}$$

Note that if a is located at i, (1) $D^a_{ij} = 0.0$ and $Pr(a)_{ij} = 1$ in the limit; (2) for all $b \neq a$, $Pr(b)_{ij} = 0$ in the limit. Otherwise, if the party located at i is not on the second ballot, $0 < Pr(k)_{ij} < 1$ for *all* candidates k on the second ballot. This model is a simple version of the "gravity" models used in marketing studies.[18]

Model II. The second model expresses $Pr(k)_{ij}$ as follows for the case when i is *not* on the second ballot:

1. If a is the closest candidate to i and *all* candidates are to the right *or* all are to the left of i,

$Pr(a)_{ij} = 1.0$.

2. If i is located *between* candidates a and b,

$Pr(a)_{ij} = D^b_{ij}/(D^a_{ij} + D^b_{ij})$ and

$Pr(b)_{ij} = D^a_{ij}/(D^a_{ij} + D^b_{ij})$. Hence,

$Pr(a)_{ij} + Pr(b)_{ij} = 1.0$.

Of course, if the party located at i is on the second ballot, it is voted for with probability 1.0. In this model $Pr(k)_{ij} > 0$ for *at most* two candidates on the second ballot: the closest candidates to the right and left of location i. For the remaining candidates, $Pr(k)_{ij} = 0$.

Model III. This version states that when the party located at i is *not* on the second ballot, the probability of voting is split between the two closest candidates, a and b, as follows:

$Pr(a)_{ij} = D^b_{ij}/(D^a_{ij} + D^b_{ij})$ and

$Pr(b)_{ij} = D^a_{ij}/(D^a_{ij} + D^b_{ij}) = 1 - Pr(a)_{ij}$.

In contrast to Model II, the probability of voting is split between the two closest candidates even if they are *both* on the right or left of i. As in Model II, $Pr(k)_{ij} > 0$ for *at most* two second ballot candidates.

The three models described above are intuitively sensible models of voter behavior that satisfy the "adding up" condition. Though one can propose other models satisfying the basic axioms, we will restrict our analysis to these three models.

Aggregation of Individual Voters. We wish to use $Pr(k)_{ij}$ to determine $V(k)_j$, the proportion of valid votes *(exprimés)* cast on the second ballot for candidate k in district j. $V(k)_j$ can be expressed as follows:

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[18] David L. Huff, *Determination of Intraurban Retail Trade Areas* (Los Angeles: University of California, Real Estate Research Program, 1962). The relationship to gravity models was suggested by Richard Staelin.

$$V(k)_j = \sum_{i=1}^{I} Pr(k)_{ij}\, Q_{ij} + \epsilon_{kj} \qquad (1)$$

while the *predicted* proportion can be calculated as:

$$\hat{V}(k)_j = \sum_{i=1}^{I} Pr(k)_{ij}\, Q_{ij} \qquad (2)$$

where ϵ_{kj} is a random disturbance and Q_{ij} *is the proportion of voters casting valid second ballots in district j that are located at i.* Q_{ij} is estimated from the vote data on the first ballot as follows: Let PV_{ij} represent the probability that a voter in location i and district j will vote on the second ballot. Then,

$$\hat{PV}_{ij} = 1.0 - A\hat{B}S_j - \hat{\alpha}_j(BB) - \hat{\gamma}_i DIS^1_{ij}$$

where $A\hat{B}S_j$ = the estimated proportion of second ballot abstentions in district j;

$\hat{\alpha}_j(BB)$ = the nonspatial component of the blank ballot predictive equation for district j;

$\hat{\gamma}_i DIS^1_{ij}$ = the spatial component of the blank ballot predictive equation for location i and district j.

$A\hat{B}S_i$ can be computed from the heuristic abstention model. Similarly, $\hat{\alpha}_j(BB)$ and $\hat{\gamma}_i DIS^1_{ij}$ can be computed from the heuristic-alienation blank ballot model.[19] Then, weighting by the proportion of all voters located at i and normalizing gives:

$$Q_{ij} = \frac{(P_{ij})(\hat{PV}_{ij})}{\sum_{i'=1}^{I} (P_{i'j})(\hat{PV}_{i'j})}$$

The Q_{ij} can now be used to compute the predicted proportions, $\hat{V}(k)_j$, using equation (2). These predictions are consistent in that $\sum_k \hat{V}(k)_j = 1.0$. They require no parameter estimation. The predictive accuracy of each a-priori model can be tested by computing the root mean square error (RMSE) for the candidates of each party as follows:

$$\text{RMSE}_k = \sqrt{\sum_j [\hat{V}(k)_j - V(k)_j]^2 / m_k}$$

where m_k = the number of districts where k is on the second ballot.

The reported root mean square errors multiplied by 100 represent percentage prediction errors. We have used RMSE rather than R^2 in part to emphasize that the RMSEs are greater than those found by regressing $V(k)_j$ on $\hat{V}(k)_j$. In other models, where R^2 is applicable, there is, of course, a one-to-one inverse relationship between RMSE and R^2—that is, low RMSE means high R^2.

For the a-priori models, RMSE calculations were performed separately for each party. For a given party, the model is limited to the districts in which a candidate of the party ran on the second ballot. Therefore, for the model testing results to be meaningful, we have to restrict ourselves to those parties which have a reasonably large number of second ballot candidates.[20] Of the eight principal French "parties," only the Communists, Socialists, and Gaullists satisfied this criterion.[21] Consequently, our analysis is limited to these three parties only. For each party, Tables 2 and 3 report model tests performed for all districts where the party had a second ballot candidate. Tests, reported in Tables 5 to 10, were also performed for various subsets of these districts. For 1962, the subsets were: (1) districts with two second ballot candidates, including the party's candidate, (2) Communist vs. Gaullist second ballot duels, and (3) Socialist vs. Gaullist second ballot duels. For 1958, the subsets were: (1) three-candidate second ballot districts and (2) Communist vs. Socialist vs. Gaullist second ballot truels. These particular subsets were used because they were the only relevant subsets containing relatively large numbers of second ballot districts. The subsets are used later in the article to provide a point of comparison with the standard ecological model which, as explained below, is typically effective only for district subsets.

It should be noted that a-priori models I and III are equivalent for districts with two candidates on the second ballot. Hence RMSEs for models I and III are identical for the two sets of duels and for the two-candidate district subsets. Also, the Communist RMSE and the Gaullist RMSE are identical for districts involving Communist-Gaullist duels as are the Socialist RMSE and the Gaullist RMSE for Socialist-Gaullist duels (see Tables 8 to 10). Since the analysis is performed for the same pair of parties over the same set of districts and the predicted vote proportions for the two parties must sum to 1.0, the RMSE will be the same whether we predict the vote proportion of one party or the other.

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[19] Rosenthal and Sen, pp. 44–46, 49–52. The nonlogarithmic models are used here.

[20] This restriction does not apply to the estimation techniques used for the all-candidates model (described below) in which all second-ballot districts are pooled to estimate simultaneously the parameters of the model which applies to all parties.

[21] The "party" groupings are indicated in Table 1.

Results for the A-Priori Models. Turning to the overall results (shown in Tables 2 and 3), no single a-priori model clearly dominates the others in terms of explanatory power. As all three models are intuitively reasonable models of voter behavior, a possible reason why no one model emerges as the best is that each model describes the behavior of different subsets of voters. Moreover, in many cases, two or more of the models make identical predictions. Consider, for example, the predictions for the 1962 Communists. Identical predictions were obtained for *all three* models for 98 (or 46.7 percent) of the 210 districts. Apart from these 98 districts, models I and III yield identical predictions for 19.1 percent of the districts while models II and III show identical predictions for another 24.8 percent of the districts.

The RMSE values are reasonably small for the a-priori models, especially for 1962 where the percentage errors vary between 3.4 and 5.7 percent. In 1958, there are larger errors, reaching 11 percent in the case of the Gaullists. In our earlier analysis of nonvoting, 1958 was also the year in which spatial models performed least well, perhaps in part because parties and voters were both feeling their way in a new constitutional system. Indeed, the results are poorer for the new party, the Gaullist UNR, than for the older Socialists and Communists. The 1958 Gaullists were an amalgamation running from leftist intellectuals to diehard colonialists. Only 11 of their candidates were incumbents from the Fourth Republic, and our assumption of a unique spatial location may be least appropriate in their case. It would indeed take over a decade and the death of General de Gaulle before the Gaullists would be more precisely defined as a modern conservative party.

With the foregoing proviso for 1958, the a-priori models are reasonably successful, especially considering their parsimony. Nonetheless, the RMSEs are sufficiently large to suggest a gain from a more sophisticated model. We thus turn to the all-candidates model.

The All-Candidates Model

While the a-priori models were successful in a number of respects—the models include all districts, have more explanatory power than the ecological models, and are frequently competitive with the other spatial models—it was felt that a parameterized model, termed the "all-candidates" model, should be investigated. In studying blank ballots, we had found that voters' sensitivity to their distance from the closest candidate, the "alienation" coefficient, varied with spatial location. Similarly, voters' spatial locations could affect the response of their voting probabilities to distance. For example, Communist voters might well be expected to be more disciplined that their counterparts on the Right in voting for close candidates. Therefore, we now describe a model which considers the voter's spatial location and all

Table 2. Root Mean Square Error for the 1958 A-Priori Models: All Districts

	Communist	Socialist	Gaullist
Model I	0.0466	0.0619	0.1008
Model II	0.0479	0.0704	0.1075
Model III	0.0484	0.0674	0.1046
Number of districts	385	208	268

Table 3. Root Mean Square Error for the 1962 A-Priori Models: All Districts

	Communist	Socialist	Gaullist
Model I	0.0344	0.0538	0.0506
Model II	0.0405	0.0462	0.0572
Model III	0.0353	0.0528	0.0548
Number of districts	210	117	327

second ballot candidates in specifying an individual's probability of voting.

The Model of Individual Voter Behavior. In the all-candidates model, the voting probabilities for an individual located at i are shown in Table 4 for up to four second ballot candidates. The table can be easily extended if the number of second ballot candidates exceeds four. β_i is a model parameter which has to be estimated for each voter location i. The estimated value of β_i ought to have the following properties:

1. $\beta_i \geqslant 0$. If $\beta_i = 0$, individuals vote for their closest candidate with a probability of 1.0. As β_i increases in magnitude, the individual's voting probability is dispersed more among the other candidates. Therefore, β_i can be interpreted as a propensity for voting for the closest candidate. It should be noted that it is possible for $(1 - \beta_i DIS^1_{ij})$ to become negative for sufficiently large values of $\beta_i DIS^1_{ij}$. This linear functional form is chosen only because it simplifies estimation. However, we will want to verify that the estimated values of β_i are such that $(1 - \beta_i DIS^1_{ij})$ is positive for all actual values of DIS^1_{ij} in our data.
2. The probability of voting for the closest candidate must be greater than the probability of voting for the second closest candidate. (The model formulation guarantees that the probability of voting for the second closest is greater than that for the third, etc.) Consequently, for all districts in which $P_{ij} > 0$, $(1 - \beta_i DIS^1_{ij})$ must be greater than the appropriate expression for the probability of voting for the second closest candidate.

Finally, it should be noted that the functional forms used in the all-candidates model yield $\sum_k Pr(k)_{ij} = 1.0$ for any number of candidates and satisfy the axioms listed in the section describing the a-priori models.

The all-candidates model turns out to be the best compromise between parsimony and explanatory power. Furthermore, the theory is demanding not only as to the signs but also as to the magnitudes of the estimated coefficients. Consequently, we have several checks on the desirability of this parameterized model over the a-priori models.

Aggregation to Regression Models. Since the available data consist of aggregate data for election districts, individual voter behavior must be aggregated over all voters to permit analysis using multiple regression. The aggregation is performed by using the Q_{ij} as indicated in equation (1). The all-candidates model leads to a regression without an estimated intercept but with a fixed, nonunit column. Consider, for example, a district with a Communist-Gaullist second ballot duel with only the Communists (denoted by the letter C), Socialists (denoted by S) and the Gaullists (denoted by G) present on the first ballot. The Barnes and Pierce scale indicates that the Socialists are closer to the Communists than to the Gaullists. Hence, using equation (1) and the vote probability relationships in Table 4, the aggregated equations for the Gaullists and the Communists are as follows:

Table 4. Specification of the Individual's Voting Probabilities for the All-Candidates Model

Probability of Voting for the:	Number of Candidates on the Second Ballot		
	2	3	4
Closest candidate	$1 - \beta_i DIS^1_{ij}$	$1 - \beta_i DIS^1_{ij}$	$1 - \beta_i DIS^1_{ij}$
Second-closest candidate	$\beta_i DIS^1_{ij}$	$\beta_i DIS^1_{ij}\left[0.5 + \dfrac{DIS^1_{ij}}{DIS^1_{ij} + DIS^2_{ij}}\right]$	$\beta_i DIS^1_{ij}\left[0.5 + \dfrac{DIS^1_{ij}}{DIS^1_{ij} + DIS^2_{ij}}\right]$
Third-closest candidate		$\beta_i DIS^1_{ij}\left[0.5 - \dfrac{DIS^1_{ij}}{DIS^1_{ij} + DIS^2_{ij}}\right]$	$\beta_i DIS^1_{ij}\left[0.5 - \dfrac{DIS^1_{ij}}{DIS^1_{ij} + DIS^2_{ij}}\right]\left[0.5 + \dfrac{DIS^2_{ij}}{DIS^2_{ij} + DIS^3_{ij}}\right]$
Fourth-closest candidate			$\beta_i DIS^1_{ij}\left[0.5 - \dfrac{DIS^1_{ij}}{DIS^1_{ij} + DIS^2_{ij}}\right]\left[0.5 - \dfrac{DIS^2_{ij}}{DIS^2_{ij} + DIS^3_{ij}}\right]$

$$\begin{aligned} V(G)_j &= (0)Q_{Cj} + (\beta_S DIS^1_{Sj})Q_{Sj} + (1)Q_{Gj} \\ &+ \epsilon_{Gj} = Q_{Gj} + \beta_S(DIS^1_{Sj}Q_{Sj}) + \epsilon_{Gj} \\ V(C)_j &= (1)Q_{Cj} + (1 - \beta_S DIS^1_{Sj})Q_{Sj} + (0)Q_{Gj} \\ &+ \epsilon_{Cj} = (Q_{Cj} + Q_{Sj}) + \beta_S(-DIS^1_{Sj}Q_{Sj}) \\ &+ \epsilon_{Cj} \end{aligned}$$

For this district, Q_{Gj} is the fixed component for the Gaullist equation while $(Q_{Cj} + Q_{Sj})$ is the fixed component for the Communist equation. Clearly, such components vary from district to district depending upon the candidates present on the two ballots. Consequently, before using regression analysis to estimate β_i, this nonunit component is first subtracted from the dependent variable, $V(k)_j$, giving us a regression without an intercept term.

Estimation. The parameter estimates were not obtained from a set of regressions whose dependent variables were the vote proportions of the various parties. Instead we used ordinary least squares to estimate a *pooled* regression in which the votes for *all* parties were predicted by means of a *single* regression equation. This procedure is appropriate because β_i, although variant with the spatial position of the voter, is invariant with the party vote being predicted. However, in each district we have $\sum_j V(k)_j = 1$. Because of this constraint, we always deleted the leftmost candidate in each district.[22] The deleted candidate's predicted vote was given as one minus the sum of the predicted votes for the other candidates. For each district, then, we had $(N_{2j} - 1)$ observations; total observations were $\sum_j N_{2j} - J$.

Results for the All-Candidate Model. When we make RMSE comparisons between the all-candidates model and the previously discussed a-priori models, we find that the all-candidates model has a moderately greater level of success. Tables 5 to 10 display, for the three major parties, the relevant RMSEs for 1958 and 1962. Since there are three a-priori models, there are 3(models) x 2(years) x 3(parties) = 18 possible comparisons. For the pooled regression covering all districts, the all-candidates model has a lower RMSE in 15 of the 18 cases. While at worst it leads to a 13.4 percent increase in RMSE, at best it reduces RMSE by 43.5 percent. The average RMSE reduction is 17.1 percent. The pattern of RMSEs by year and party is similar to the results for the a-priori models. In particular, the worst predictions again result for the 1958 Gaullists.

In 14 of 15 comparisons the all-candidates model also does better than the a-priori model when we control for the number of candidates. In 1958, the modal number of candidates was three. For three-candidate races, the nine possible comparisons all favor the all-candidates model. For the modal two-candidate races of 1962, five of the six comparisons favor the all-candidates model.[23] The only RMSE increase in 1962 is 2.0 percent, substantially less than the maximum reduction of 24.4 percent. (Discussion of comparisons for the duels and truels of Tables 5 to 10 is deferred until the section on ecological models.)

The RMSE reduction afforded by the all-candidates model is far from uniform, and, indeed, in some comparisons it is inferior to one or more of the a-priori models. These facts may in part reflect our lack of knowledge concerning voter decision rules. What we can, with some degree of safety, conjecture about spatially oriented voters is contained in our pair of axioms; these are extremely weak in their implications. The three a-priori models and the all-candidates model are only four members of the class of decision rules consistent with the axioms. Of the four, the all-candidates model is, in one aspect, least satisfying since the linearity imposed to facilitate estimation implies that the model can only be consistent with the axioms over a restricted range of distances. It is not surprising then that the greater flexibility incorporated in the β_i coefficients of the all-candidates model is sometimes offset by the relative inappropriateness of the form of the decision rule.

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[22] The constraint also implies that it is incorrect to assume uncorrelated errors for observations within a given district. The ordinary least squares (OLS) estimates are thus inefficient. An estimation problem similar to ours has been addressed in T. W. McGuire, J. U. Farley, R. E. Lucas, Jr., and L. W. Ring, "Estimation and Inference for Linear Models in Which Subsets of the Dependent Variable Are Constrained," *Journal of the American Statistical Association*, 63 (December 1968), 1201–13. They obtain efficient estimates by deleting one observation per district and by applying a generalized least squares procedure. Their procedure cannot be applied directly because it would require that the same set of parties contest every district on the second ballot. A modified procedure we developed yielded estimated coefficients quite similar to those found by OLS. On the other hand, the results suggest that the standard error estimates produced by OLS are overly conservative.

[23] There are six, not nine, comparisons because of the equivalence of models I and III for two-candidate races.

Since the additional reduction in RMSE afforded by the all-candidates model is modest, there ought to be a strong concordance between theoretical requirements and the signs and magnitudes of the estimated coefficients if we are to sacrifice the parsimony of the a-priori models. In fact, agreement is exceptionally strong. First, as required, all coefficients, for all four years, are positive (see Table 11). An indication of how the calculated probabilities match up with the requirement that the probability of voting for the closest candidate be greater than that for the second closest is provided in Table 12. To understand Table 12, consider the 1958 entry, "MRP" for the Communist voter location. This implies that if the closest candidate for a Communist voter in a two-candidate race is an MRP candidate, the Communist voter's probability of voting for the MRP candidate will be less than 0.5, thus violating a requirement on the magnitude of the estimated β_i parameters. If the closest candidate for the Communist voter is from a party to the Left of the MRP party (e.g., Socialist) this requirement is not violated. Although Table 12 shows that for the majority of the coefficients, scenarios could arise in which the requirement would be violated, such scenarios are nonexis-

Table 5. Root Mean Square Error Comparisons for 1958 Communist Predictions

Model	All Districts	Three-Candidate Districts	Truels[a]
A-Priori			
Model I	0.0466	0.0379	0.0283
Model II	0.0479	0.0416	0.0208
Model III	0.0484	0.0397	0.0217
All-candidates	0.0274	0.0287	—[b]
Closest-candidate			
CC Model[c]	0.0379	0.0439	0.0404
HCC Model[d]	0.0142	0.0155	0.0151
Ecological			
Exprimés Model	0.0261	0.0223	0.0111
Number of districts	385	229	68

[a] Districts with Communist-Socialist-Gaullist truels on the second ballot.
[b] Not computed.
[c] Closest-candidate model with a constant nonspatial component.
[d] Heuristic + closest-candidate model.

Table 6. Root Mean Square Error Comparisons for 1958 Socialist Predictions

Model	All Districts	Three-Candidate Districts	Truels[a]
A-Priori			
Model I	0.0619	0.0636	0.0746
Model II	0.0704	0.0733	0.0851
Model III	0.0674	0.0702	0.0839
All-candidates	0.0613	0.0616	—[b]
Closest-candidate			
CC Model[c]	0.0628	0.0672	0.0704
HCC Model[d]	0.0486	0.0469	0.0446
Ecological			
Exprimés Model	0.0721	0.0518	0.0448
Number of districts	208	126	68

[a] Districts with Communist-Socialist-Gaullist truels on the second ballot.
[b] Not computed.
[c] Closest-candidate model with a constant nonspatial component.
[d] Heuristic + closest-candidate model.

tent or rare in practice. For example, there is an entry "Extreme Right" for the PSU coefficient in 1967. As we are guaranteed, however, that there will be a candidate to the Left of the "Extreme Right," no violation occurs.

Let us consider the 1968 situation in detail. No violation is possible for the Communist, PSU, and Gaullist locations. For the Center, no violation occurs empirically for it is never the case that the only candidate is a Communist. For the Right, violations will occur only in the absence of a nonleftist candidate; for the Federation, only in the absence of a leftist. Just a handful of districts correspond to these conditions. Only the very small Extreme Right group shows a serious violation. Similarly, there are few actual violations for the three other years.

The stability of the estimated coefficients for the *last three* elections is another factor supporting the model. As can be seen from Table 11, the range of variation of the coefficient for a given spatial location for 1962–1968 is small compared to the total range of variation. The only exceptions are the PSU and the Extreme Right which represent the smallest numbers of voters. For example, the Gaullist coefficient ranges from 0.094 to

Table 7. Root Mean Square Error Comparisons for 1958 Gaullist Predictions

Model	All Districts	Three-Candidate Districts	Truels[a]
A-Priori			
Model I	0.1008	0.0994	0.0894
Model II	0.1075	0.1068	0.0783
Model III	0.1046	0.1057	0.0873
All-candidates	0.0917	0.0969	—[b]
Closest-candidate			
CC Model[c]	0.0808	0.0807	0.0539
HCC Model[d]	0.0719	0.0732	0.0434
Ecological			
Exprimés Model	0.1232	0.1043	0.0483
Number of districts	268	144	68

[a] Districts with Communist-Socialist-Gaullist truels on the second ballot.
[b] Not computed.
[c] Closest-candidate model with a constant nonspatial component.
[d] Heuristic + closest-candidate model.

Table 8. Root Mean Square Error Comparisons for 1962 Communist Predictions

Model	All Districts	Two-Candidate Districts	Comm-Gaul Duels[a]
A-Priori			
Model I	0.0344	0.0342	0.0339
Model II	0.0405	0.0426	0.0342
Model III	0.0353	0.0342	0.0339
All-candidates	0.0390	0.0349	—[b]
Closest-candidate			
CC Model[c]	0.0553	0.0612	0.0637
HCC Model[d]	0.0287	0.0245	0.0257
Ecological			
Exprimés Model	0.0489	0.0283	0.0214
Number of districts	210	114	99

[a] Districts with Communist-Gaullist duels on the second ballot.
[b] Not computed.
[c] Closest-candidate model with a constant nonspatial component.
[d] Heuristic + closest-candidate model.

0.189 while the coefficients for the five major locations range from .041 to .662.

This stability is impressive in view of the varying substantive context of the three elections. The 1962 election was conducted with Algeria still an issue, and the controversy over De Gaulle's referendum on direct election of the president was its immediate issue. The 1967 election was much more representative of classic Left-Right economic and social welfare issues while the 1968 election was characterized by "the great fear" that followed the student-worker demonstrations and strike of May-June Apparently, from the viewpoint of the model, these substantive changes can be represented as shifts in the spatial distribution shown by the first ballot. On the other hand, voter second ballot decision rules were stable in the sixties and the all-candidates model thus had predictive capacity.

The differences between 1958 and the other years might, as we suggested in our earlier article, be due either to lack of experience or to a substantive difference. In effect, the Left was

Table 9. Root Mean Square Error Comparisons for 1962 Socialist Predictions

Model	All Districts	Two-Candidate Districts	Soc-Gaul Duels[a]
A-Priori			
Model I	0.0538	0.0516	0.0526
Model II	0.0462	0.0442	0.0408
Model III	0.0528	0.0516	0.0526
All-candidates	0.0425	0.0390	—[b]
Closest-candidate			
CC Model[c]	0.0422	0.0365	0.0306
HCC Model[d]	0.0358	0.0339	0.0287
Ecological			
Exprimés Model	0.0904	0.0480	0.0263
Number of districts	117	76	58

[a] Districts with Socialist-Gaullist duels on the second ballot.
[b] Not computed.
[c] Closest-candidate model with a constant nonspatial component.
[d] Heuristic + closest-candidate model.

Table 10. Root Mean Square Error Comparisons for 1962 Gaullist Predictions

Model	All Districts	Two-Candidate Districts	Gaul-Comm Duels[a]	Gaul-Soc Duels[b]
A-Priori				
Model I	0.0506	0.0487	0.0339	0.0526
Model II	0.0572	0.0457	0.0342	0.0408
Model III	0.0548	0.0487	0.0339	0.0526
All-candidates	0.0508	0.0383	—[c]	—
Closest-candidate				
CC Model[d]	0.0358	0.0318	0.0285	0.0293
HCC Model[e]	0.0320	0.0312	0.0285	0.0285
Ecological				
Exprimés Model	0.0788	0.0548	0.0209	0.0283
Number of districts	327	195	99	58

[a] Districts with Gaullist-Communist duels on the second ballot.
[b] Districts with Gaullist-Socialist duels on the second ballot.
[c] Not computed.
[d] Closest-candidate model with a constant nonspatial component.
[e] Heuristic + closest-candidate model.

Table 11. Spatial β Coefficients for the All-Candidates Model (All Districts)

Voter Location		Estimated β Coefficients[a] 1958	1962	1967	1968
Communist		.141 (.079)	.041 (.015)	.069 (.016)	.084 (.019)
PSU		.820 (.251)	.326 (.066)	.137 (.056)	.057 (.051)
Socialist	Federation in 1967 and 1968	.412 (.045)	.222 (.027)	.265 (.021)	.295 (.026)
Radical		.371 (.039)	.225 (.028)		
MRP–Center		.104 (.045)	.366 (.046)	.429 (.030)	.194 (.041)
Right-Rep. Ind.		.097 (.046)	.412 (.075)	.662 (.103)	.522 (.070)
Gaullist		.251 (.067)	.094 (.036)	.189 (.035)	.105 (.040)
Extreme Right		.127 (.134)	.447 (.232)	1.016 (.369)	.328 (.512)
RMSE		.0695	.0507	.0463	.0388

[a] Standard errors of the coefficients are in parentheses.

Table 12. Analysis of Voting Probabilities for the All-Candidates Model (All Districts): Closest Candidate for Which a Theoretical Violation Occurs

Voter Location		1958	1962	1967	1968
Communist		MRP[a]	None	None	None
PSU		Communist	MRP	Extreme Right	None
Socialist	Federation in 1967 and 1968	Communist	Right	Center	Center
Radical		Communist	Gaullist		
MRP–Center		None	Extreme Right	Extreme Right	Communist
Right		None	Radicals	Extreme Right	Federation
Gaullist		Radicals	None	PSU	None
Extreme Right		Communist	MRP	Gaullist	Gaullist

[a] Party closest to location for which probability of vote is less than 0.5 if the party is actually the closest candidate in a two-candidate race.

Table 13. How Viability Affects the Postdictions of the All-Candidates Model: RMSEs by Year and Number of Candidates

Year	Number of Candidates 2	3	4	All Districts
1958	.048 (83)[a]	.071 (240)	.073 (91)	.070 (422)
1962	.040 (229)	.060 (125)	––[b] (11)	.051 (365)
1967	.033 (329)	.066 (65)	–– (2)	.046 (396)
1968	.032 (270)	.063 (46)	–– (1)	.039 (316)

[a] Number of districts are in parentheses.
[b] Too few districts for computation.

divided in 1958 while it tacitly or overtly made second ballot coalitions in the later years.[24] On the other hand, the failure of purely spatial models to accommodate viability factors suggests such models are inappropriate for multicandidate races. In turn, these were by far most numerous in 1958 (reflecting the division of the Left). If we can link large prediction errors to viability considerations, the more distinct coefficients found for 1958 can be viewed as the result of specification error rather than a modification of voter behavior.

We began our investigation of viability effects with a detailed examination of 1962 Gaullist residuals. No geographic, urban-rural, or other socioeconomic pattern was suggested. On the other hand, of the largest residuals—those over .10 in magnitude—22 of 28 came from districts with three or more candidates, precisely those where viability effects are present. Viability thus appears as the critical element missing from our all-candidates model specification. To isolate its effects, we reestimated the model controlling for the number of candidates, with the results shown in Table 13. Two-candidate races, where viability is not a consideration, have substantially lower RMSEs. Although coefficient differences between 1958 and later years persist, interyear RMSE differences are attenuated if not entirely eliminated. The importance of viability is further demonstrated by results from the closest-candidate model described below.

The Closest-Candidate Model

The Model of Individual Voting Behavior. In the closest-candidate model, the individual's voting behavior is represented as before, by $Pr(k)_{ij}$. In this model, $Pr(k)_{ij}$ contains a nonspatial component in addition to a spatial component. The spatial component of $Pr(k)_{ij}$ is expressed as a function of the difference between: (1) the distance of candidate k and (2) the distance of the candidate *closest* to location i. The larger the value of this difference, the smaller should be the value of $Pr(k)_{ij}$. On the other hand, when k is the candidate closest to i, $Pr(k)_{ij}$ should attain its maximum value (all other factors remaining constant). Thus, $Pr(k)_{ij}$ may be expressed as:

$$Pr(k)_{ij} = \alpha_{jk} + \delta_{ik}(DIS^1_{ij} - D^k_{ij})$$

where α_{jk} is the nonspatial component of the model and δ_{ik} is a coefficient to be estimated. δ_{ik} should have a positive sign. Although aggregation procedures follow those of the previous models, a separate regression is run for each party for this model.

Specification of the Model's Nonspatial Component. The nonspatial component of the model, α_{jk}, can be represented by a constant, α, or by an expression containing a constant and one or more variables. α_{jk} is represented here as a heuristic model consisting of a constant and the following explanatory variables:

1. $V1$: the proportion of first ballot votes cast for candidate k. As an indicator of the candidate's viability, the coefficient of $V1$ should clearly be positive.
2. $1/N_2$: N_2 is the number of second ballot candidates. The larger the value of N_2, the smaller should be the vote proportion for candidate k. In the regression, we use the reciprocal of N_2, $1/N_2$.[25] The coefficient of $1/N_2$ should have a positive sign.
3. *COMP:* the ratio of the votes obtained on the first ballot by candidate k to the first ballot votes obtained by the leading candidate. In general, $0 < COMP \leqslant 1$. $COMP$ equals 1 if candidate k is the leading candidate on the first ballot. Clearly, the coefficient of $COMP$ should have a positive sign.

Potential explanatory variables such as the proportion of blank ballots and the proportion of abstentions on the first ballot were included in the model initially but provided no explanatory power; neither did party coalition dummy variables or regional dummy variables.[26] The failure of the dummy variables to provide explanatory power provides support for our view that the nonspatial component is of greater importance to the participation decision than to the voting choice decision.

There were two major problems with the closest-candidate model. First, the signs of the estimated spatial parameters demonstrated poor agreement with the microtheory of individual spatial voting.[27] Next, the model violated the adding-up condition at the microlevel although at the macro, constituency, level the sum of the estimated vote shares for the parties was "close" to 1.0 in all districts. Despite these deficiencies, the closest-candidate model was highest in "explanatory power," if only marginally better than other models more satisfying from a theoretical viewpoint. We will use the closest-candidate model only as a benchmark to suggest a level of root mean square error that might be achieved by a better theoretical model using the same input data.

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[24] Rosenthal and Sen, p. 45.

[25] Use of the reciprocal is motivated in Rosenthal and Sen, p. 42.

[26] These variables are defined in Rosenthal and Sen, p. 43.

[27] Subrata K. Sen, "The Calculus of Voting: Empirical Studies for France," (Ph.D. dissertation, Carnegie-Mellon University, 1971), Ch. 3, pp. 41–42.

Because the microlevel results for this model are poor, we omit any discussion of the spatial coefficients. Moreover, without the nonspatial component, the RMSEs of this model (i.e., the CC model in Tables 5 to 10) for "All Districts" regressions fail to dominate those of the all-candidates model except for the 1958 and 1962 Gaullists. On the other hand, including the nonspatial component makes this model (i.e., the HCC model in Tables 5 to 10) best from the viewpoint of low RMSE. It is also the case that *V*1 and *COMP*, the two variables most clearly related to viability, always have the correct sign, except for the *COMP* coefficient in the 1958 Socialist truels. However, the t-statistic (–.88) indicates that this coefficient does not differ significantly from zero. $1/N_2$ has an incorrect sign only for the 1962 Gaullists.

These results suggest that a viability component could be added to the all-candidates model. One way to accomplish this would be to allow the vote accorded the closest candidate to be calculated as:

$$1 - B_{1i}DIS^1_{ij} - B_{2i}V^1_i - B_{3i}(1/N_2) - B_{4i}COMP^1_i$$

where V^1_i is the first ballot vote of the candidate closest to *i* and $COMP^1_i$ is the ratio of the vote of the candidate closest to *i* to the leading candidate on the first ballot. Table 4 could then be readily revised to yield the vote for the other candidates. Potential problems are that V^1_i may be highly collinear with spatial terms and that the variables involved are relatively poor proxies for viability effects, which involve a consideration of the chances of all the candidates and not just the preferred candidate.

Ecological Models

The results for the closest-candidate model complete our presentation of spatial models. For comparison purposes, we now turn to the standard ecological models other researchers have used to analyze French legislative elections.[28] Consider the subsets of individuals exercising a given choice on the first ballot: either voting for a party, abstaining, or casting a blank ballot. Ecological models are based on the assumption that in each subset the expected value of the proportion voting for a given party on the second ballot is constant for all districts.

With $i = 1, \ldots, I$ again indexing the parties, let $i = I + 1$ index abstention and $I + 2$ index blank ballot. Then let R_{ij} equal the proportion of registered voters in district *j* who made choice *i* on the first ballot, and $R(k)_{ij}$ equal the (conditional) proportion of those choosing *i* on the first ballot who also choose $k (k = 1, \ldots, I + 2)$ on the second ballot. The standard ecological model assumes:

$$E[R(k)_{ij}] = \mu_{ik},$$

i.e., a constant for all districts. Aggregating for all first ballot choices over all voters in the district, we obtain

$$R(k)_j = \sum_{i=1}^{I+2} \mu_{ik} R_{ij} + \epsilon_{jk}$$

leading to the regression equation:

$$R(k)_j = \rho_{0k} + \sum_{i=1}^{I+1} \rho_{ik} R_{ij} + \epsilon_{jk}$$

where the ρ's are coefficients to be estimated and $R(k)_j$ is the total proportion of registered voters making choice *k* on the second ballot in district *j*. Blank ballots $(R_{I+2,j})$ are purposely omitted from the set of explanatory variables in order to avoid a linearly dependent set of variables (since all vote proportions must sum to 1.0). We can now express the μ_{ik} in terms of the regression coefficients as follows:

$$\mu_{ik} = \rho_{0k} + \rho_{ik}, i = 1, 2, \ldots, I + 1$$

$$\mu_{I+2,k} = \rho_{0k}.$$

Since the μ_{ik} are voting probabilities, one should obtain estimates such that $0 \leqslant \rho_{0k} \leqslant 1$, and $0 \leqslant (\rho_{0k} + \rho_{ik}) \leqslant 1$, for $i = 1, 2, \ldots, I+1$.

These models of registered vote proportions are not directly suitable for comparisons with the spatial models of *exprimés* (valid vote) proportions. Clearly, we can convert the $R(k)_j$ predictions to $V(k)_j$ predictions through the formula:

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[28] For French applications see: B. Mendès-France and L. Laumonier, "Une Application des Methodes de l'Analyse Statistique à l'Estimation des Déplacements de Voix entre les Deux Tours des Elections Présidentielles de 1965," *Revue Française de Science Politique*, 17 (February 1967), 110–14, and Alain Lancelot and Pierre Weill, "Les Transferts de Voix du Premier au Second Tour des Elections de Mars 1967; Une Analyse de Regression," in *Les Elections Legislatives de Mars 1967*, Centre d'Etude de la Vie Politique Francaise (Paris: A. Colin, 1970), pp. 373–88.

$$V(k)_j = \frac{R(k)_j}{\sum_{k'=1}^{I} R(k')_j}$$

We will refer to this as the ecological *(inscrits-exprimés)* prediction.

Similarly, we can convert the spatial predictions of $V(k)_j$ to predictions of $R(k)_j$ through the formula:

$$\hat{R}(k)_j = \hat{V}(k)_j [\sum_{i=1}^{I} P_{ij} P\hat{V}_{ij}].$$

We will refer to this, for the all-candidates model, as the all-candidates *(exprimés-inscrits)* prediction.

Another form of comparison, however, is to construct an ecological model based on *exprimés* proportions. The regression model is:

$$V(k)_j = \rho_{0k} + \sum_{i=1}^{I-1} \rho_{ik} P_{ij} + \epsilon_{jk}$$

and the μ_{ik} are now given as:

$$\mu_{ik} = \rho_{0k} + \rho_{ik},\ i = 1, 2, \ldots, I-1,$$

$$\mu_{Ik} = \rho_{0k}.$$

As stated earlier, the standard ecological models assume that the expected proportion of voters voting for a specific party on the second ballot is constant over all districts, given party choice or abstention or blank ballot on the first ballot. Naturally, such an assumption has little face validity, as the probability of a Socialist voting for a Communist, say, ought to depend heavily on whether a Socialist is on the ballot. Consequently, unlike our models, which are designed to have full scope and apply to all districts, ecological models are traditionally run on subsets of the districts for which the "constant" assumption is reasonable, an example being districts with Communist-Gaullist second ballot duels. In fact, one of the aims of this study is to show that the spatial model in large part eliminates the need for subsets and, in addition, has the advantage of providing analysis for district types that cannot be included in one of the subsets. The 11 four-candidate races in 1962 illustrate districts that would normally be excluded by ecological models.

Results for the Ecological Model. In discussing the ecological model we emphasize comparisons with the all-candidates model, our preferred spatial model. Only results for the *exprimés* model are shown; results for the *inscrits* model are similar. The ecological model clearly cannot be used with full scope. When applied to all districts, both *inscrits* and *exprimés* comparisons between the ecological model and the all-candidates model show the all-candidates model dominating. Even the a-priori model dominates the ecological model, with the exception of the Communists in 1958 (see Tables 5 to 10).

For two-candidate races in 1962, the ecological model is also dominated by the all-candidates model (except for the Communists) and is roughly equalled in explanatory power by the a-priori model. Only in duels between specific parties does the ecological model tend to postdict better. There are exceptions, however, and the ecological model's advantage is slight, postdictions RMSEs being lowered by less than .003 on the average. Moreover, in data sets restricted to specific parties, an unpooled all-candidates model is equivalent to an ecological model except that (1) the estimated Q_{ij}'s are used in place of the first ballot results and (2) the estimated probabilities are constrained to satisfy our first axiom. The lack of this constraint is primarily responsible for the slight advantage of the ecological model. This advantage is often bought, however, at the cost of estimated probabilities being greater than 1.0.

Similarly, for the 1958 data, the ecological model does well only in highly specific subsets, namely Communist-Socialist-Gaullist truels. For all three-candidate districts, the ecological model is better than the all-candidates model for Communists and Socialists but worse for Gaullists.

The microlevel results for the ecological models are poor. When estimated for all districts, the μ coefficient of the party that is the dependent variable in the equation is always greater than 1.0 for the 12 possible cases (2 years x 2 models x 3 parties). When this coefficient is tested against 1.0 (as against the usual test of 0.0), the t-statistics range from 0.70 to 13.89, with only two of 12 less than 2.0. While the microlevel results for duels are satisfactory, they are not so for the 1958 truels. Consider, for example, the 1958 Communist *inscrits* model. (The *inscrits-exprimés* predictions outperform the direct *exprimés* predictions here.) For the truels, the Communist μ coefficient is substantially greater than 1.0 while the coefficients for three of the other parties are less than zero, two of them substantially so. Multicollinearity, of course, can have a heavy hand in these results, giving another reason for preferring the more constrained all-candidates model.

For the voting equations, therefore, the ecological models appear to be less desirable than the spatial models, especially when the greater scope and parsimony of the spatial models is considered. We also recall that ecological models are even less successful for nonvoting than for voting.[29] This suggests that spatial models should replace ecological models as the mode for analyzing aggregate voter shifts between the two ballots.

Summary of Results for the Four Models

We have described and estimated four types of models designed to predict the vote proportions of candidates on the second ballot. The first two types, the a-priori models and the all-candidates model, are theoretically based. The remaining models, the closest-candidate model and the ecological model, are more heuristic in nature and are viewed as null models for the two theoretical types.

The empirical results indicate that of the two types of theoretical models, the all-candidates model is superior. Its coefficients appeared stable throughout the 1960s. When viability considerations were eliminated by restricting the analysis to two-candidate races, candidate vote proportions were postdicted with very satisfactory root mean square errors (3.2 to 4.8 percent). The closest-candidate null model gives rise to good macro predictions but does not provide microlevel interpretation. The ecological model is good only when the domain of analysis is restricted to districts involving duels or truels. Compared to the null models, the two theoretical models also rely on a smaller number of estimated parameters. (Table 14 displays the total number of parameters needed when the models are applied to predicting abstentions, blank ballots, and the vote for eight parties.) Moreover, as compared to the ecological model, the theoretical models, particularly the all-candidates model, have reasonably low RMSEs when applied to the entire body of data. These results, coupled with those of our earlier article, demonstrate the importance of spatial models in empirical analysis.

Postscript: Validation and Replication

Subsequent to the original writing and presentation of this paper, we were able to acquire and code the 1973 election results. These are now used for purposes of replication and cross-validation of the all-candidates model.

The replication was performed by reestimating the model using the new data. In terms of RMSE (as can be seen comparing Tables 15 and 13), the model performed better for 1973 and 1968 than for any of the earlier years. Similarly, in our nonvoting paper, we found that the alienation model performed better for later elections. The 1973 results therefore lend weight to the conjecture advanced in the earlier article that, through learning initiated with the new system of 1958, the French electorate is tending toward more patterned and systematic behavior.

The replication also reinforces our findings with regard to viability: once again results were substantially better when estimation was restricted to districts with only two candidates. The only disappointing aspect of the replication concerned the negative coefficient estimated

Table 14. Number of Parameters Estimated for Each Model

	8 Parties	8 Parties Plus Abstentions and Blank Ballots
A-Priori Models[a]	28	28
All-Candidates Model[a]	36	36
Closest-Candidate Model[a]		
Without heuristic variables	64	92
With heuristic variables	88	116
Ecological Model		
Exprimés version	64	--[b]
Inscrits version	80	92

[a] Parameters estimated for the abstention and blank ballot models in Rosenthal and Sen, "Electoral Participation in the French Fifth Republic," *American Political Science Review*, 67 (March 1973), 44–51, are included in the total.

[b] Does not apply.

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[29] Rosenthal and Sen, pp. 38–40.

for the PSU. However, the PSU point encompasses only a small fraction of the electorate, and the coefficient is not large relative to its estimated standard error.

We also cross-validated the model by using estimated 1967 and 1968 nonvoting and voting coefficients to make simulated *ex ante* forecasts of the 1973 results. Clearly, one would expect the 1967 and 1968 coefficients to lead to poorer results than those estimated from the data. (Because our model uses the Q_{ij} terms estimated from the nonvoting equations, there is no absolute guarantee that 1973 estimated coefficients will outperform those from prior elections.) Consequently, we need a benchmark that indicates "how much worse is worse." As a benchmark, we calculated (as shown in Table 15) RMSEs for the "deterministic" model which says that if voters vote, they vote for the closest candidate with probability 1.0. This is equivalent to the all-candidates model with all β_i constrained to zero. The benchmark has a RMSE of 5.0 percent as against 3.9 percent for the all-candidates model (4.7 vs. 3.0 percent in the case of duels).

The cross-validation results using the 1968 coefficients are impressive. As shown in Table 16, for all districts, the RMSE is 4.1 percent as against 3.9 percent for the estimated 1973 model. For two-candidate races, the RMSE increases from 3.0 to 3.7 percent but this is still substantially less than the benchmark of 4.7 percent. We stress that the Barnes and Pierce spatial position measures were computed from a 1968 survey. These combined with the 1968 coefficients would thus have led to a reasonably successful, if very short-run, forecast of the second ballot.

Table 16 also discloses that 1968 could have been forecast with some success from 1967 coefficients. On the other hand, use of 1967 coefficients on 1973 data leaves one little better off than the benchmark.

In conclusion, the replication and validation results show that a one-dimensional spatial model of the second ballot has substantial explanatory power. While there are some changes in time, the coefficients of the model are sufficiently stable to tentatively suggest that coefficients from the previous election, if not

Table 15. The All-Candidates Model Vs. Deterministic Voting for the Closest Candidate: Spatial (β) Coefficients and RMSEs for 1973

	Spatial (β) Coefficients[a]			
	All-Candidates Model		Deterministic Model	
Voter Location	All Districts	Two Candidates	All Districts	Two Candidates
Communist	.039 (.011)	.060 (.009)	0.0	0.0
PSU	−.136 (.081)	−.066 (.060)	0.0	0.0
Socialist	.198 (.016)	.222 (.012)	0.0	0.0
Radical	.230 (.040)	.217 (.029)	0.0	0.0
Center	.266 (.026)	.301 (.019)	0.0	0.0
Right-Rep. Ind.	.339 (.078)	.592 (.069)	0.0	0.0
Gaullist	.102 (.041)	.160 (.032)	0.0	0.0
Extreme Right	.516 (.105)	.215 (.129)	0.0	0.0
RMSE	.0385	.0300	.0497	.0472
Number of districts	483	352	483	352

[a] Standard errors of all estimated coefficients are in parentheses.

still earlier elections, can be used for forecasting purposes. The success of the model is particularly apparent for two-candidate races where, paralleling theoretical arguments, empirical evidence now indicates that viability considerations are of small import. The major area for improvement lies with a model of strategic voting that would ameliorate the all-candidates model's performance for multicandidate races.

Table 16. Replication and Validation:
RMSEs by "Election Predicted" and "Election Used to Estimate Coefficients"

	Election Used to Estimate Coefficients		
Election Predicted	**1967**	**1968**	**1973**
1967:			
All districts	.046		
Two-candidate districts	.033		
1968:			
All districts	.044	.039	
Two-candidate districts	.040	.032	
1973:			
All districts	.047	.041	.039
Two-candidate districts	.046	.037	.030

Political Parties and Macroeconomic Policy*

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This study examines postwar patterns in macroeconomic policies and outcomes associated with left- and right-wing governments in capitalist democracies. It argues that the objective economic interests as well as the subjective preferences of lower income and occupational status groups are best served by a relatively low unemployment-high inflation macroeconomic configuration, whereas a comparatively high unemployment-low inflation configuration is compatible with the interests and preferences of upper income and occupational status groups. Highly aggregated data on unemployment and inflation outcomes in relation to the political orientation of governments in 12 West European and North American nations are analyzed revealing a low unemployment-high inflation configuration in nations regularly governed by the Left and a high unemployment-low inflation pattern in political systems dominated by center and rightist parties. Finally, time-series analyses of quarterly postwar unemployment data for the United States and Great Britain suggests that the unemployment rate has been driven downward by Democratic and Labour administrations and upward by Republican and Conservative governments. The general conclusion is that governments pursue macroeconomic policies broadly in accordance with the objective economic interests and subjective preferences of their class-defined core political constituencies.

In so far as stable prices are regarded as desirable for their own sake, as contributing to social justice, it must be recognized that justice to the rentier can be achieved only by means of the injustice to the rest of the community of maintaining a lower level of effective demand than might otherwise be achieved. We are here presented with a conflict of interests . . . and actual policies are largely governed by the rival influences of the interests involved. (Joan Robinson, *Essays in the Theory of Employment* (New York: Macmillan, 1937), p. 35.)

From one important point of view, indeed, the avoidance of inflation and the maintenance of full employment can be most usefully regarded as conflicting class interests of the bourgeoisie and the proletariat, respectively, the conflict being resolvable only by the test of relative political power in the society. (Harry G. Johnson, "Problems of Efficiency in Monetary Management," *Journal of Political Economy*, 76 (September/October 1968), p. 986.)

We tend to get our recessions during Republican administrations. . . . The difference between the Democrats and the Republicans is the difference in their constituencies. It's a class difference . . . the Democrats constitute the people, by and large, who are around median incomes or below. These are the ones whom the Republicans want to pay the price and burden of fighting inflation. The Democrats [are] willing to run with some inflation [to increase employment]; the Republicans are not. (Paul A. Samuelson, "Some Dilemmas of Economic Policy," *Challenge*, 20 (March/April 1977), pp. 30–31.)

The most important problem of macroeconomic policy facing public authorities in industrial societies during the postwar period has been the unfavorable trade-off that exists between unemployment and inflation—the so-called "Phillips curve." Although the unemployment/inflation trade-off has not exhibited great stability in recent years—for example, the U.S. economy is undoubtedly more vulnerable to inflation at low levels of unemployment now than it was a few years ago—there is widespread agreement among economists that in capitalist economies wage and price stability requires relatively high levels of unemployment, and, conversely, that low rates of unemployment

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yield relatively high rates of inflation.[1] Put another way, price stability and full employment are incompatible goals in the sense that conventional macroeconomic policy has not been able to achieve both simultaneously. Since political authorities can (and do) influence the rate of unemployment and inflation by manipulation of monetary and fiscal policy instruments, macroeconomic policy has been the focus of intense controversy and conflict between key political actors and interest groups.

This article examines postwar patterns in macroeconomic policies and outcomes associated with left- and right-wing governments in capitalist democracies. The main body of the article has three parts. The first section briefly reviews evidence, which is documented in great detail elsewhere,[2] indicating that different unemployment/inflation outcomes have important, class-linked effects on the distribution of national income. It is argued that the economic interests at stake in various macroeconomic configurations are (implicitly) reflected in public opinion data on the relative aversion of different income and occupational groups to unemployment and inflation. The second part of the article presents a general scheme rank-ordering the preferences of political parties, arrayed along the traditional left to right spectrum, toward various economic goals, and analyzes highly aggregated data on unemployment and inflation outcomes in relation to the political orientation of regimes in 12 West European and North American nations. These international comparisons suggest that the "revealed preference" of leftist governments has been for relatively low unemployment at the expense of high rates of inflation, whereas, comparatively low inflation and high unemployment characterize political systems dominated by center and right-wing parties. The third and longest section of the article presents time-series analyses of quarterly postwar data on unemployment in the United States and Great Britain. The estimation results from the time-series models support the conclusion that unemployment has been driven downward during the tenure of Democratic and Labour administrations and has moved upward during periods of Republican and Conservative rule in the United States and Great Britain, respectively.

The general conclusion of the study is that the macroeconomic policies pursued by left- and right-wing governments are broadly in accordance with the objective economic interests and subjective preferences of their class-defined core political constituencies.

Unemployment and Inflation: Objective Economic Interests and Subjective Preferences

A common rationalization for deflationary macroeconomic policies is that inflation adversely affects the economic position of wage and salary earners and, in particular, erodes the economic well-being of the poor. Empirical studies, however, give little support to this argument. The work of Blinder and Esaki, Hollister and Palmer, Metcalf, Thurow, Schultz, and others strongly indicates that a relatively low unemployment-high inflation macroeconomic configuration is associated with substantial relative and absolute improvements in the economic well-bring of the poor and, more generally, exerts powerful equalizing effects on the distribution of personal income.[3]

Although these studies suggest that inflationary periods with tight labor markets are associated with a general equalization of the income distribution—the poor and certain middle income groups gaining at the expense of the rich—it nevertheless has been argued that the economic position of a substantial fraction of the labor force suffers a net decline during periods of vigorous economic expansion. The usual observation is that price rises tend to outstrip money wage increases during cyclical upswings and real wage rates therefore fall. Moreover, business expansions bring a general inflation of profits which yields increases in the share of the national income going to capital.[4] If the profit-inflation and wage-lag hypotheses are accurate, it is possible in principle that the relative and absolute gains enjoyed by lower income groups during economic booms come at the expense of other wage earning groups and conceal substantial declines in the national income share of labor as a whole.

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[1] A detailed review of the theoretical and empirical literature on Phillips-curve inflation models is given in my *Economic Interest and the Politics of Macroeconomic Policy*, No. C/75–14, Center for International Studies, M.I.T., Cambridge, Mass., January 1976. Copies of this monograph are available at cost from the C.I.S. Publications officer.

[2] Ibid.

[3] See, for example, A. Blinder and H. Esaki, "Macroeconomic Activity and Income Distribution in the Postwar U.S." (mimeo., November 1976); Robinson G. Hollister and John L. Palmer, "The Impact of Inflation on the Poor," in *Redistribution to the Rich and the Poor,* ed. K. E. Boulding and M. Pfaff (Belmont, Calif.: Wadsworth, 1972), pp. 240–70; Charles E. Metcalf, *An Econometric Model of the Income Distribution* (Chicago: Markham, 1972); Lester C. Thurow, "Analyzing the American Income Distribution," *American Economic Review: Papers and Proceedings,* 60 (May 1970), 261–69; and T. Schultz, "Secular Trends and Cyclical Behavior of Income Distribution in the United States: 1944–1964," in *Six Papers on the Size Distribution of Wealth and Income,* ed. L. Soltow (New York: National Bureau of Economic Research, 1969), pp. 75–100.

However, contemporary empirical work provides little or no evidence in favor of either the profit-inflation or wage-lag hypothesis. Long's examination of historical relationships in the United States (1860 to 1958) found that real wage movements were not countercyclical, as Keynes and others argued, but on the whole corresponded quite closely to business fluctuations.[5] Bodkin's analysis of postwar quarterly and longer-run annual data on trend-corrected real wage changes in Canada and the United States detected no systematic association one way or the other between real wage movements and unemployment in Canada, whereas inverse associations prevailed in the United States.[6] Finally, studies by Bach and Stephenson, Boddy and Crotty, Burger, Hibbs, Kuh, Hultgren, and the Organization for Economic Cooperation and Development on the cyclical behavior of factor shares, i.e., shares of the national income going to capital and labor over the business cycle, show that in general the ratio of profits to wages increases steadily after a trough in business activity, reaches its highest point about midway through an expansion, and thereafter drops off markedly.[7] Thus the latter halves of business upswings, during which unemployment typically falls and the rate of inflation rises, are associated with a pronounced squeeze on profits and are more accurately described as periods of *wage-lead* and *profit-deflation.* Although it is difficult to say whether these patterns in the cyclical behavior of wages and profits would persist in prolonged expansions, the evidence does demonstrate that the economic position of wage and salary earners as a group improves substantially, both in relative and absolute terms, during periods of relatively low unemployment and high rates of inflation.

If sustained economic expansions confer such obvious benefits on wage and salary earners generally and on low and middle income groups in particular, why have macroeconomic policy makers exhibited such keen sensitivity to the inflationary consequences of full employment? One explanation of why political authorities have been willing to accept less than full employment is that the mass of wage and salary earners have an "irrational" aversion to inflation, perhaps because people tend to view rising prices as an arbitrary "tax."[8] Deflationary macroeconomic policies may therefore represent the political response to widespread anti-inflation sentiment in the mass public.[9] Sample survey evidence for the United States and Great Britain squarely contradicts this argument. For more than 20 years George Katona and has associates at the Survey Research Center of the University of Michigan have polled national samples of American households about their expectations and attitudes toward inflation, unemployment, and other socioeconomic issues. Katona writes that until 1973 more people felt that unemployment was a greater evil than inflation. Moreover, a majority of the respondents in the SRC surveys repeatedly indicated that they were hurt "little" or "not at all" by inflation and that they would not be willing to accept substantial increases in unemployment in order to halt increasing prices.[10]

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[4] These hypotheses have a distinguished pedigree. They have appeared, among other places, in Earl J. Hamilton, "Prices and Progress," *Journal of Economic History,* 12 (Fall 1952), 325–49; Alvin Hansen, "Factors Affecting the Trend of Real Wages," *American Economic Review,* 15 (March 1925), 27–42; John Maynard Keynes, *The General Theory of Employment, Interest and Money* (New York: Harcourt, 1936); Jacques Rueff, "Nouvelle discussion sur le chomage, les salaires et les pris," *Revue d'Economie Politique* (1951), 761–91; and Sidney Weintraub, *An Approach to the Theory of Income Distribution* (Westport, Conn.: Greenwood Press Inc., 1958). Weintraub, for example, has flatly asserted that ". . . only entrepreneurs and the actual unemployed have an unequivocal stake in maximum employment, while rentiers and the employed find their interests better served at lower levels of activity," p. 60.

[5] Clarence D. Long, "The Illusion of Wage Rigidity: Long and Short Cycles in Wages and Labor," *Review of Economics and Statistics,* 42 (May 1960), 140–51.

[6] Ronald G. Bodkin, "Real Wages and Cyclical Variations in Employment: A Re-Examination of the Evidence," *Canadian Journal of Economics,* 2 (February to November 1969), 353–74.

[7] G. L. Bach and James B. Stephenson, "Inflation and the Redistribution of Wealth," *Review of Economics and Statistics,* 61 (February 1974), 1–13; Raford Boddy and James Crotty, "Class Conflict and Macro-Policy: The Political Business Cycle," *Review of Radical Political Economics,* 7 (Spring 1975), 1–19; Albert Burger, "Relative Movements in Wages and Profits," *Federal Reserve Bank of St. Louis Review,* 55 (February 1973), 8–16; Hibbs, "Economic Interest"; Edwin Kuh, "Income Distribution and Employment over the Business Cycle," in *Brookings Quarterly Econometric Model of the United States,* ed. J. Dusenberry et al. (Chicago: Rand McNally, 1965), pp. 227–78; Thor Hultgren, *Costs, Prices, and Profits: Their Cyclical Relations* (New York: National Bureau of Economic Research, 1965); and OECD, *Inflation, The Present Problem* (Paris: OECD Publications, 1970).

[8] This has been proposed, for example, in William D. Nordhaus, "The Political Business Cycle," *Review of Economic Studies,* 42 (April 1975), 169–90.

[9] As one White House economist reportedly put it in April of 1975 "One hundred percent of the people have been hit by inflation. Only 10 percent really worry about unemployment." Quoted by S. Golden, "High Joblessness Expected to Persist as a Condition of U.S. through Decade," *New York Times* (April 21, 1975), p. 46.

My own analyses of survey data from Great Britain and the United States on public aversion to unemployment and inflation supports the inferences of Katona and his associates. Space permits me to report only the general conclusions of these analyses here.[11] First, the British and American public opinion data clearly show that in the period through 1972 (which is the relevant period for the purposes of this study) solid majorities of the mass public(s) typically expressed greater aversion to unemployment than inflation. Second, popular concern about unemployment and inflation is class-related. Low and middle income and occupational status groups are more averse to unemployment than inflation, whereas, upper income and occupational status groups are more concerned about inflation than unemployment. Although the available survey evidence is by no means definitive, it does appear that the subjective preferences of class or status groups are at least roughly in accordance with their objective economic interests, insofar as these are reflected by the behavior of wages, profits, and the distribution of personal income under various unemployment/inflation macroeconomic configurations.[12]

Having outlined the group or class cleavages surrounding the unemployment/inflation trade-off, we now turn to the main task of this article and consider to what extent these cleavages are reflected in the economic policies pursued by governments of different political orientations.

Macroeconomic Policies and Outcomes: International Comparisons

The evidence reviewed in the previous section suggests that the objective economic interests and subjective preferences of lower income, blue-collar groups differ markedly vis-à-vis the unemployment/inflation trade-off from those of higher income, white-collar groups. Although the importance of socioeconomic status as a basis of electoral cleavage varies substantially across party systems, the mass constituencies of political parties in most advanced industrial societies are distinguished to a significant extent by class, income, and related socioeconomic characteristics. Even a casual examination of the historical record makes it clear that differences in the economic interests and preferences of income and occupational groups are reflected in the contrasting positions toward various economic goals associated with left- and right-wing political parties. (This is not to suggest, incidentally, that the influence linkages between mass constituencies and party elites are unidirectional.) Hence, labor-oriented, working-class-based Socialist and Labor parties typically attach far greater importance to full employment than to inflation, whereas business-oriented, upper middle-class-based Conservative parties generally assign higher priority to price stability than to unemployment. The implied preferences or issue positions of political parties (or *tendances*), arrayed along the traditional left-right spectrum, are outlined more systematically in Table 1. The table is adapted from a study by Kirschen et al. and is based on questionnaires administered to experts in eight industrial societies.[13] The most important thing to notice in the table is the reversal in the relative preferences of the parties regarding various economic goals as one moves from left to right across the political spectrum. In particular, notice that the party preferences concerning unemployment and inflation are consistent with the class-related cleavages surrounding these issues that were identified previously.

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[10] George Katona, "Disputing Galbraith," *New York Times* (December 22, 1974), and George Katona et al., *Aspirations and Affluence* (New York: McGraw-Hill, 1971). Many of the results from these surveys appear in annual volumes of the *Survey of Consumer Finances* (Ann Arbor: Survey Research Center, 1960–72). Results of surveys taken before 1960 are available as mimeo reports from the SRC.

[11] The analyses are presented fully in the section "Public Opinion Toward Inflation and Unemployment" in Hibbs, "Economic Interest," pp. 24–40.

[12] The class interests at stake in unemployment/inflation outcomes and policies show up in the policy positions taken by organized labor and capital as well as in the distribution of mass opinion. Throughout the postwar period, trade union spokesmen have invariably placed primary emphasis on the objective of full employment, while business elites have attached far more importance to price stability. A clear statement of labor's position is given by Nat Goldfinger, "Full Employment: The Neglected Policy?" *The American Federationist,* 79 (November 1972). Data on corporate thinking on the inflation and unemployment issues is presented in L. Silk and D. Vogel, *Profits and Principles: The Social and Political Thinking of American Businessmen* (New York: Simon and Schuster, 1977).

[13] E. S. Kirschen et al., *Economic Policy In Our Time,* Vol. I (Amsterdam: North-Holland, 1964). With the exception of the balance of payments issue (the importance of which depends critically on the inter-

Since political authorities in the post-Keynesian age have considerable influence on macroeconomic outcomes, we would expect to observe (*ceteris paribus*, of course) a relatively low unemployment-high inflation macroeconomic configuration under leftist regimes and conversely under rightist regimes. Highly aggregated, cross-national evidence supporting this proposition appears in Figure 1, which shows a Phillips curve-like scatterplot of the average rates of unemployment and inflation over the 1960 to 1969 period in 12 industrial societies. The vertical and horizontal axes in this Figure identify the median average rates of unemployment and inflation, respectively. Five of the six nations enjoying an average level of unemployment below the West European-North American median (i.e., the nations to the left of the vertical axis) are countries with large Socialist or Social Democratic parties (closely linked to organized labor) that have governed for much or most of the time since World War II. Looking at the postwar period as a whole, Socialist parties have been in power (or have shared power as members of coalition governments) for the entire period in Sweden, for the bulk of the period in Denmark, Finland, and Norway, and for about two-thirds of the period in the Netherlands. As one would anticipate from the Phillips curve (inverse association of unemployment and inflation), the majority of the nations lying below the unemployment median have on the average experienced above-median rates of inflation. The principal exception to these generalizations is West Germany, which has been governed for most of the postwar period by the conservative CDU party and has experienced both low unemployment and low rates of inflation.

With the exception of Belgium and to a lesser extent the United Kingdom, the governments of all nations in Figure 1 falling above (i.e., to the right of) the average unemployment median have been dominated by center or right-wing political parties. In the United States and Canada, where problems of deficient aggregate demand are chronic, unemployment rates have consistently been the highest in the Western industrial world. Neither of these countries has politically important Socialist or

Table 1. Preferences of Political Parties in Advanced Industrial Societies Regarding Various Economic Goals[a]

	Socialist-Labor	Center	Conservatives
Decreasing Importance of Goals ↓	Full Employment		Price Stability
	Equalization of Income Distribution		
		Price Stability	
	Economic Expansion		
		Economic Expansion	Balance of Payments Equilibrium
		Full Employment	
		Equalization of Income Distribution	
	Price Stability		Economic Expansion
		Balance of Payments Equilibrium	Full Employment
	Balance of Payments Equilibrium		
			Equalization of Income Distribution

[a]Based on Kirschen et al., 1964.

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national economic position of a given nation), the positions attributed to the various *tendances* were homogeneous across countries. For a similar scheme, see Bruno Frey and Lawrence J. Lau, "Towards a Mathematical Model of Government Behaviour," *Zeitschrift für Nationalökonomie*, 28 (1968), 355–80.

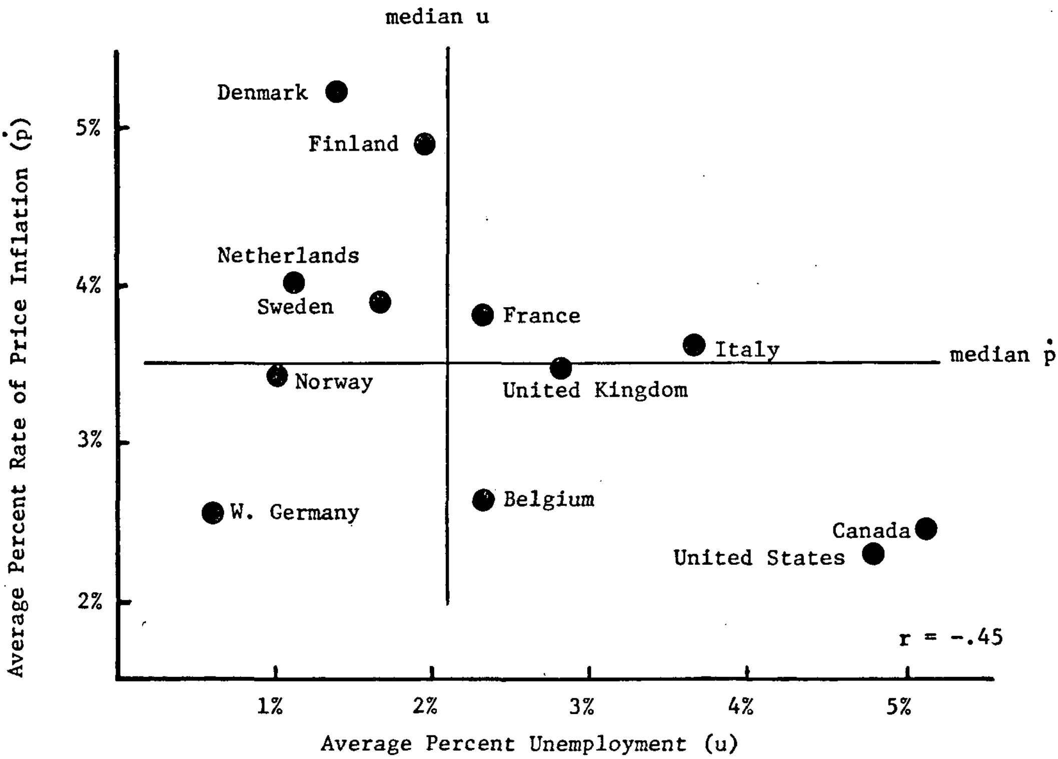

Figure 1. Mean Inflation and Unemployment, 1960–1969, in 12 West European and North American Nations

Source: Unemployment data for Canada, France, Italy, Sweden, U.K., and W. Germany are adjusted to the U.S. definition and are from Constance Sorrentino, "Unemployment in the United States and Seven Foreign Countries," *Monthly Labor Review*, 93 (September 1970), 12–23. All other data are from I.L.O., *Yearbook of Labor Statistics;* various volumes.

Labor parties,[14] and centrist or rightist governments have ruled throughout the postwar era.

The Communist and Socialist political blocs in France and Italy have commanded a sizable share of the vote in all postwar elections, but aside from the governments of national unity in the immediate postwar period and the marginal representation of the French and Italian socialists in various Center coalition governments, they have been largely frozen out of positions of executive power.[15] Belgium deviates from the general pattern in that the Socialists have ruled (in coalition with other parties) for just over half of the postwar years, and the average rate of unemployment stands just above the West European-North American median. However, unemployment has on the average been lower (and the rate of inflation on the average higher) during the tenure of Socialist coalition governments than during periods of Center-Right rule. Great Britain also constitutes something of an exception. The Labour and Conservative parties have alternated in power (although the Conservatives ruled continuously from 1951 to 1964) and the average unemployment rate is above the median. The mean British unemployment rate, however, is substantially less than the average rates prevailing in the United States, Canada, and Italy.

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[14] Canada's New Democratic Party, a genuinely socialist party with close connections to organized labor, has exhibited increasing political vitality in recent years (capturing several provincial governments) but remains at this writing a "minor" party with little influence on national policy.

[15] Actually there was one brief period of Socialist-led rule in France after 1951: Guy Mollet's government of February 1956 to May 1957. Analysis of annual data shows that unemployment was lower and inflation higher during Mollet's government (as well as during the subsequent Center-Left government of Bourgès-Maunoury) than during the right-wing Gaullist governments of the late 1950s and 1960s. The Center-Left governments of the middle 1950s clearly assigned higher priority to full employment and expansion than the Gaullist regime, which pursued policies geared to disinflation and economic "stabilization." As a result, France's location on the "international Phillips curve" has changed dramatically. (Contrast the data shown in Figure 1 to a similar display of average rates of inflation and unemployment reported by D. Smyth, "Unemployment and Inflation: A Cross-Country Analysis of the Phillips Curve," *American Economic Review*, 61 (June 1971), 426–29, for the period 1950–1960.) Of course France's entry into the EEC in 1958 increased the importance of the external balance-of-payments constraint during the Fifth Republic. However, the deflationary policies of the Gaullist governments must be attributed to some extent to the priorities of the regime. See M Maclennan et al., *Economic Planning and Policies in Britain, France and Germany* (New York: Praeger, 1968).

Taken as a whole, the evidence in Figure 1 indicates that the "revealed preference" of governments of the nations in the northwest quadrant of the figure has been for relatively low unemployment at the expense of high inflation, whereas the opposite appears to be true for governments of the countries in the southeast quadrant of the figure. This is reinforced by Figures 2 and 3, which show simple scatterplots of the average rates of inflation and unemployment in relation to average government participation (percentage of postwar years in the executive branch) of Socialist and Labor parties. These plots merely provide a slightly different illustration of the earlier argument. Nations in which Social Democratic and Labor parties have governed for most or much of the postwar period have generally experienced high rates of inflation. Conversely, low rates of inflation have prevailed in countries where center and right-wing parties have dominated the policy-making process (Figure 2). The reverse is true of the association between average unemployment and average Socialist-Labor executive participation. Comparatively low rates of unemployment characterize systems in which left-wing parties have regularly controlled the executive, and high unemployment rates have been typical in systems governed primarily by center and right-wing parties.[16]

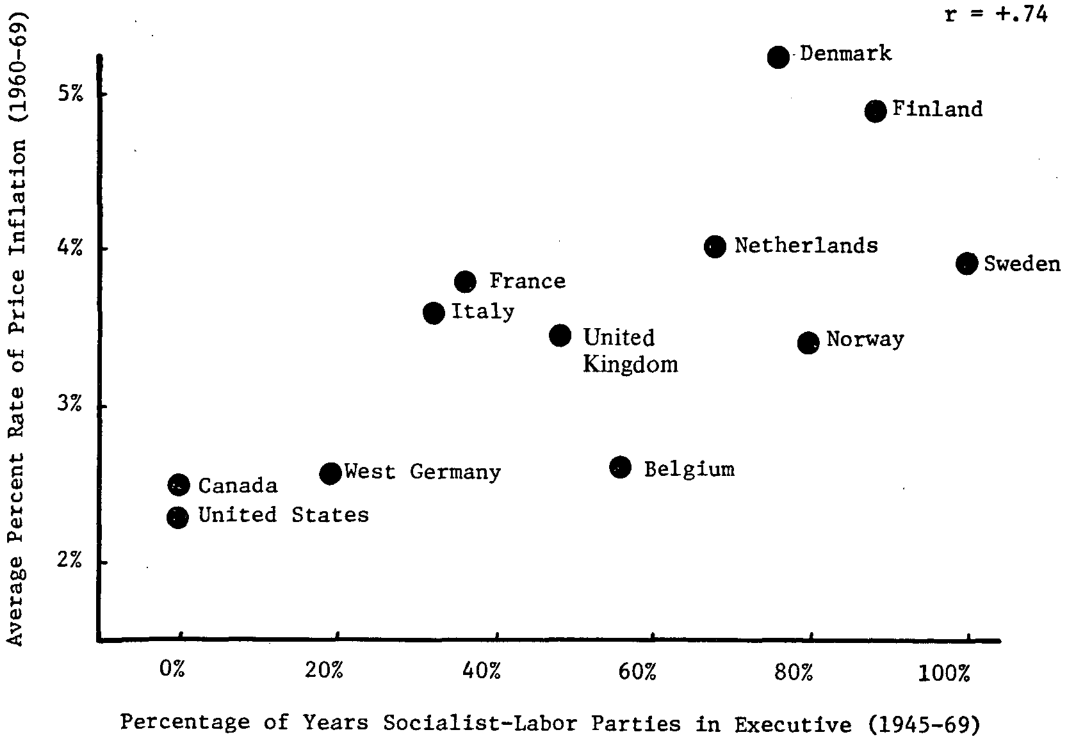

Figure 2. Mean Inflation and Socialist-Labor Executive Participation in 12 West European and North American Nations

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[16] Since the macroeconomic policies (and outcomes) of the 1960s were to a significant extent

If a common unemployment/inflation trade-off (or "menu of policy choices") confronted each of the nations appearing in Figures 1 through 3, the cross-national variation in unemployment/inflation configurations might be attributed primarily to systematic differences in the short-run monetary and fiscal policies pursued by political authorities.[17] The modest but inverse relationship between the average rates of inflation and unemployment (the correlation is –.45) suggests that there is some merit in this interpretation. Rates of unemployment even approaching those typical of Canada and the United States are simply not politically feasible or acceptable in countries with large Socialist-Labor parties that are frequently governed by the Left. Prior economic performance and continued emphasis on low unemployment in political discourse has gen-

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influenced by the performance record of the late 1940s and 1950s (especially in countries in which Social Democratic-led governments managed to maintain full employment after the war), the Socialist-Labor participation rate has been calculated over the entire postwar period (1945–69) rather than for the years 1960 to 1969 alone.

[17] This has been suggested, for example, in reference to the difference in unemployment rates between North America and Western Europe, by Albert Rees, "The Phillips Curve as a Menu for Policy Choice," *Economica*, 37 (August 1970), 227–38. Monetary policy instruments include interest rates and the supply of credit and money. Fiscal policy instruments include taxation and public spending.

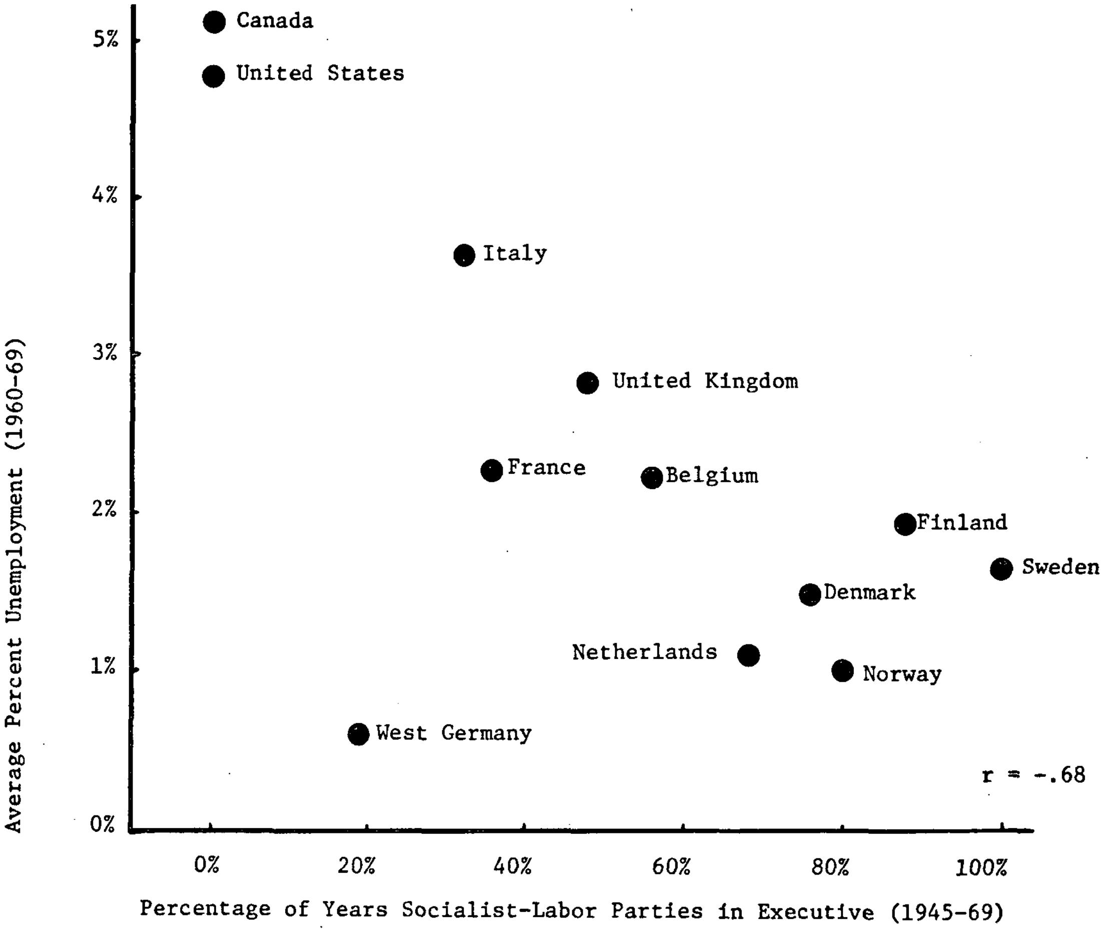

Figure 3. Mean Unemployment and Socialist-Labor Executive Participation in 12 West European and North American Nations

erated widespread public expectations of sustained full employment, which insures that short-run macroeconomic policy is geared to preserving the low unemployment, continuous inflation pattern observed in the northwest quadrant of Figure 1.

However, empirical time-series studies have established that unemployment/inflation trade-offs exhibit considerable cross-national diversity. A comparative investigation by Flanagan, for example, has shown that over the 1951 to 1968 period the Phillips curve trade-off available to political authorities in the United States was less favorable than the trade-off curves for Great Britain and Sweden.[18] Evidence of this sort indicates that international differences in institutional and structural arrangements underlie, at least to some extent, the cross-national variation in aggregate, equilibrium outcomes, depicted in Figures 1 through 3. In particular, the enormous emphasis placed on full employment in nations with large Socialist-Labor parties has led to the introduction of centralized economic planning and coordination, extensive public sector investment, and, perhaps most important, a wide range of labor market and manpower policies that are designed to minimize the incidence and duration of unemployment.[19] Hence, the critical historical role of the Left in shaping longer-run policies and institutional arrangements must also be considered in order to account adequately for cross-national variation in unemployment/inflation configurations.

Macroeconomic Policies and Outcomes: Time-Series Analyses

Thus far only static, aggregated evidence has been presented in support of the hypothesis that macroeconomic outcomes systematically covary with the political orientation of governments. A dynamic country-by-country analysis of postwar time-series data might provide a more convincing test of this general proposition except for the major constraint that many advanced industrial societies have simply not experienced very much partisan variation (defined in the traditional left-right sense) in their governments.

Time-series analyses of unemployment rates have been undertaken for Great Britain and the United States. Great Britain is an ideal candidate for dynamic analysis in that national political power has oscillated between the working class-based Labour party and the middle class-based Conservative party. In comparison to the British Labour and Conservative parties, the two dominant American political parties are less distant ideologically and have more heterogeneous social bases.[20] Nonetheless, the Democratic party has relatively close connections to organized labor and lower income and occupational status groups, while the Republican party is generally viewed as being more responsive to the interests of capital or business and upper income and occupational status groups.[21] Other things being equal, we would therefore expect to observe a downward movement in the unemployment rate during the tenure of Democratic and Labour governments and an upward movement in the unemployment rate during periods of Republican and Conservative rule in the United States and Great Britain, respectively.

In order to evaluate this proposition rigorously, we need a model that permits estimation of the hypothesized effects of government macroeconomic policies on the unemployment rate, net of trends, cycles and stochastic fluctuation in the unemployment time-series observations. In contrast to more conventional approaches, I have used the "intervention analysis" scheme of Box, Jenkins and Tiao.[22]

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[18] Robert J. Flanagan, "The U.S. Phillips Curve and International Unemployment Rate Differentials," *American Economic Review,* 63 (1973), 114–31. For additional evidence on cross-national variation in Phillips curves, see Ronald G. Bodkin et al., *Price Stability and High Employment: The Options for Canadian Economic Policy* (Ottawa: Economic Council of Canada, 1967).

[19] Of course leftist governments have not been equally effective in this regard. For example, British Labour governments have been much less imaginative in developing macroeconomic policy (and have pursued a more centrist political strategy) than Swedish Social Democratic administrations. See the perceptive comparative analysis in Andrew Martin, *The Politics of Economic Policy in the U.S.: A Tentative View from a Comparative Perspective* (Beverly Hills: Sage Professional Paper in Comparative Politics, 1973). The best treatment in English of the archetypal Swedish model is probably A. Lindbeck, *Swedish Economic Policy* (Berkeley: University of California Press, 1974).

[20] See, for example, Robert Alford, *Party and Society* (Chicago: Rand-McNally, 1963).

[21] For an argument that organized labor and the Democratic party in the United States are interpenetrated in a way that is at least partially equivalent to Socialist party-labor union alliances in much of Western Europe, see J. D. Greenstone, *Labor in American Politics* (New York: Alfred A. Knopf, 1969).

[22] See G. E. P. Box and G. M. Jenkins, *Time Series Analysis; Forecasting and Control* (San Francisco: Holden-Day, 1970), part III; and G. E. P. Box and G. C. Tiao, "Intervention Analysis with Applications to Economic and Environmental Problems," *Journal of the American Statistical Association,* 70 (March 1975), 70–79. The scheme of Box, Jenkins, and Tiao

Box-Jenkins or Box-Tiao models represent time-series observations on the endogenous variable (in this case unemployment) as the realization of a linear stochastic process of autoregressive, moving average, or mixed, autoregressive-moving average form. The autoregressive-moving average (ARMA) model provides a stochastic benchmark against which intervention-induced changes in the slope and/or level of the endogenous time-series are assessed. Intervention occurrences (in this case partisan changes in government) are represented by binary variables (0, 1) or by related coding schemes (e.g., +1, −1), and the effects of interventions are specified by simple "transfer functions."

Regarding the problem at hand, the most plausible hypothesis is that shifts in the political orientation of governments during the postwar period in Great Britain and the United States will be associated with *gradual* changes in the net *levels* of the British and American unemployment rates. The intervention models therefore take the general form

$$U_t = \frac{\beta}{1-\delta L} G_{t-1} + \frac{\theta_0 + \theta_q(L)}{\phi_p(L)(1-L)^d} a_t \qquad (1)$$

where: U_t = the percentage of the civilian labor force unemployed (quarterly data);

G_t = +1 during Labour or Democratic administrations; −1 during Conservative or Republican administrations;

β, δ = parameters describing the effects of shifts in G_t on U_t;

L = lag operator such that $LU_t = U_{t-1}$, $L^i U_t = U_{t-i}$, etc.;

$(1-L)^d$ = a lag difference operator such that $(1-L)U_t = U_t - U_{t-1}$, $(1-L)^2 = (1-2L+L^2)U_t = U_t - 2U_{t-1} + U_{t-2}$, etc.;

$\theta_q(L) = 1 - \theta_1 L - \theta_2 L^2 - \ldots - \theta_q L^q$

$\phi_p(L) = 1 - \phi_2 L - \phi_2 L^2 - \ldots - \phi_p L^p$ are moving average and autoregressive polynomials in L of order p and q, respectively;

θ_0 = a constant indexing a deterministic time trend of degree d in U_t; and

a_t = a sequence of independently distributed random variables with mean zero and variance σ_a^2.

Equation (1) simply expresses the proposition that—net of trends, cycles, and stochastic fluctuation in the unemployment time-series, which are captured by the autoregressive-moving average terms in the model[23]—we anticipate a gradual rise in unemployment levels under Conservative and Republican governments and, conversely, a gradual decline in unemployment levels during Labour and Democratic administrations. If a partisan change in government, occurring, for example, at time n, was sustained indefinitely (e.g., $G_t = +1$ for all $t \geqslant n$), the unemployment rate would eventually fluctuate about the steady state or equilibrium value $\beta/1-\delta$. The rate of adjustment to the new equilibrium depends on the magnitude of the dynamic parameter δ. Since we assume that the macroeconomic policies of a new government are not introduced or implemented instantaneously, the intervention term G_t is specified with a one period (quarter) delay or lag.[24]

The British Unemployment Model. The first step in the model building process is to develop a preliminary specification of the stochastic or ARMA component of equation (1) by analyzing the sample autocorrelation and partial autocorrelation functions of the endogenous variable (i.e., unemployment).[25] The sample autocorrelation function[26] r_k for seasonally unadjusted quarterly observations on the Brit-

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is contrasted with the conventional structural equation approach in Douglas A. Hibbs, Jr., "On Analyzing the Effects of Policy Interventions: Box-Jenkins and Box-Tiao vs. Structural Equation Models," in *Sociological Methodology 1977,* ed. D. Heise (San Francisco: Jossey Bass, 1977), pp. 137–79.

[23] The cyclical or seasonal component of the model is not represented explicitly by the ARMA terms of eq. (1).

[24] The one quarter lag on G_t may be too short, especially for the United States. However, since the intervention function allows U to respond gradually to shifts in G, this is not an important problem.

[25] The ARMA model building process is systematically reviewed in Hibbs, "On Analyzing Policy Interventions," and developed in great detail by Box and Jenkins, *Time Series Analysis.*

[26] Sample autocorrelations are simply the correlations between observations separated k periods in time and are given by:

$$r_k = \frac{\Sigma(U_t - \bar{U}_t)(U_{t-k} - \bar{U}_t)}{\Sigma(U_t - \bar{U}_t)^2} \qquad r = 1, 2, \ldots$$

Thus r_1 denotes the correlation between U_t and U_{t-1}; r_2 denotes the correlation between U_t and U_{t-2}; and so on.

ish unemployment rate over the 1948(1) to 1972(4) period is graphed in Figure 4.[27] The sample autocorrelations decay steadily as the lag k increases, which indicates that a low-order autoregressive process is compatible with the British unemployment observations. Since the partial autocorrelations (which are not reported here) are insignificant for $k > 1$, we tentatively entertain a first order autoregressive specification:

$$U_t = \phi_1 U_{t-1} + e_t, \text{ or} \qquad (2)$$

$$(1-\phi_1 L)U_t = e_t.$$

Figure 5 presents the sample autocorrelations of the residuals $\hat{e}_t$, that is the autocorrelations of the transformed data $U_t - \hat{\phi}_1 U_{t-1}$. The autocorrelations exhibit distinct peaks every fourth quarter—at k = 4, 8, 12, 16 . . .—which suggests a strong seasonal dependence between unemployment rates of the same quarter in different years. This dependence comes as no surprise, since it is well known that unemployment is influenced by seasonal factors and the British data were not available in seasonally adjusted form. The seasonal dependence identified in Figure 5 shows no tendency to die out as the lag k increases, and therefore, four-quarter, seasonal differencing is called for. Hence we propose the model:

$$(1-L^4)e_t = \theta_0 + a_t, \text{ or} \qquad (3)$$

$$e_t = \frac{\theta_0 + a_t}{(1-L^4)}.$$

Substituting (3) into (2) yields the following expression for the stochastic component of the general intervention scheme given in (1):

$$(1-\phi_1 L)U_t = \frac{\theta_0 + a_t}{(1-L^4)}, \text{ or} \qquad (4)$$

$$U_t = \frac{\theta_0 + a_t}{(1-L^4)(1-\phi_1 L)}.$$

Adjoining (4) to the intervention function proposed in (1) to represent the hypothesized net impact of partisan changes in government on the unemployment level, we arrive at the equation:

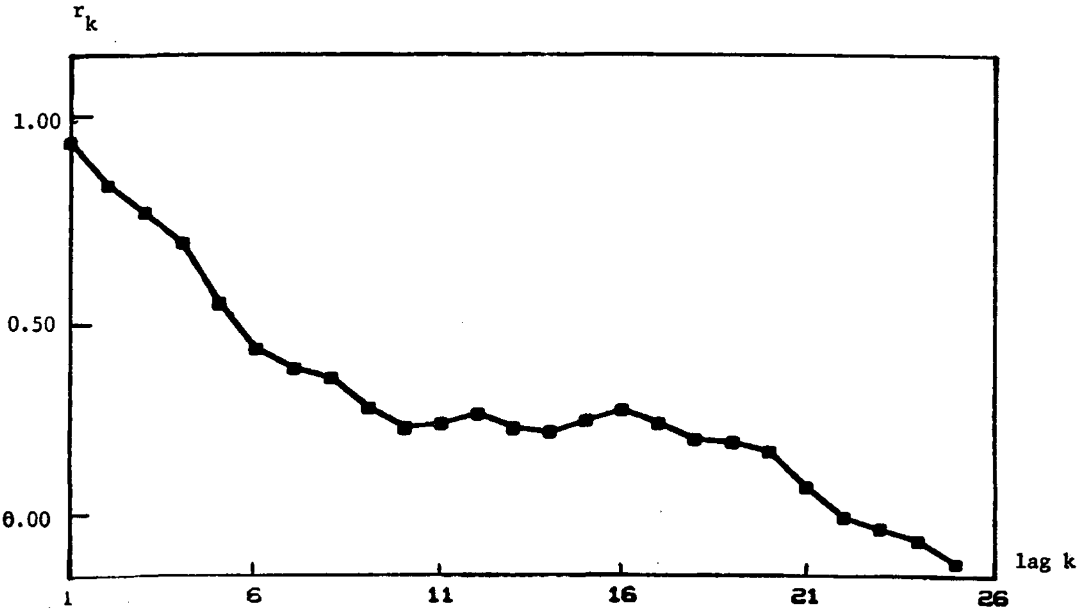

Figure 4. Sample Autocorrelation Function of the British Unemployment Rate Data, 1948(1)–1972(4)

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[27] The British unemployment data (wholly unemployed as a percentage of the civilian labor force) were obtained from the *Ministry of Labour Gazette*, various issues. In view of the unprecedented exogenously imposed economic crisis facing advanced industrial societies since 1973, the time series analyses are intentionally not taken beyond the fourth quarter of 1972.

$$U_t = \frac{\beta}{1-\delta L} G_{t-1} + \frac{\theta_0 + a_t}{(1-L^4)(1-\phi_1 L)} \quad (5)$$

where: U_t = the percentage of the civilian labor force wholly unemployed in Great Britain quarterly 1948(1)–1972(4);

G_t = +1 during Labour governments; −1 during Conservative governments;

and all other terms are as previously defined.

A second intervention term should be added to the British unemployment model in order to take account of an important change in the British unemployment compensation scheme which was initiated in October 1966. Until 1966, the unemployed in Great Britain received a relatively flat-rate benefit that was not tied to previous earnings. The change in the unemployment system initiated by the Labour government in 1966 provided for an "earnings-related supplement" equal to about one-third of the unemployed person's previous average weekly earnings between £9 and £30. This represented a substantial increase in benefits for most wage earning groups.[28] As a result, unemployed workers were under less financial pressure to accept unattractive jobs and presumably spent more time in searching for new employment. It is therefore widely believed that the new compensation scheme increased the rate and duration of unemployment.[29] Thus we define a

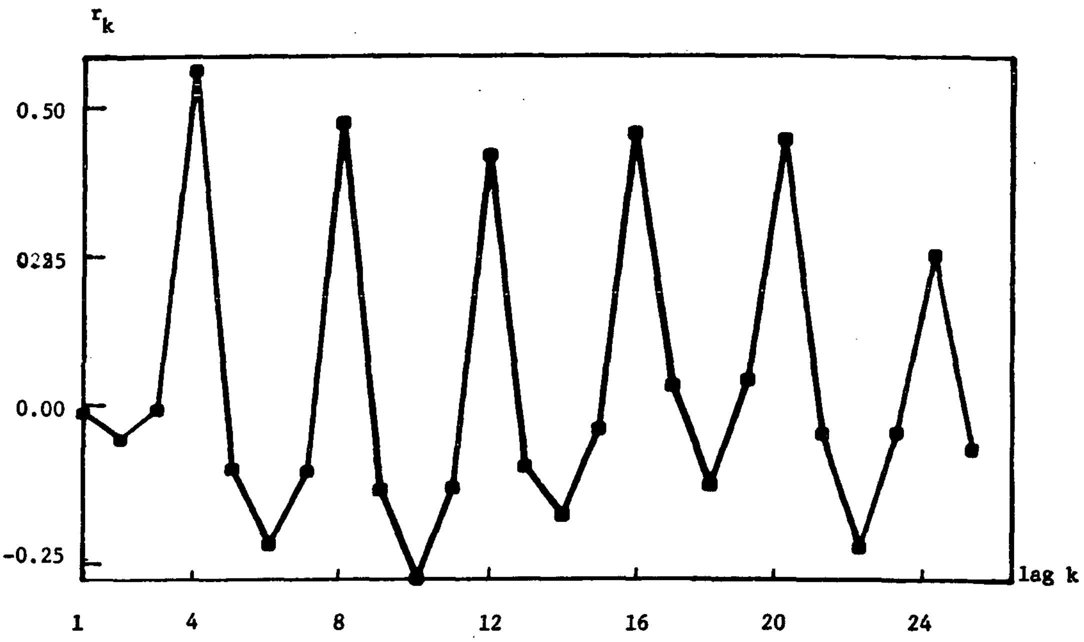

Figure 5. Sample Autocorrelation Function of the Transformed British Unemployment Rate Data $(1-\phi_1 L)\,U_t$, 1948(1)–1972(4)

new variable C_t taking a value of 0 prior to 1966(4) and a value of +1 otherwise, and specify the revised model:

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[28] For example, it is estimated that the earnings-related benefits increased the unemployment income of a typical married male worker with two children from about 40 percent to 60 percent of average employment income. See OECD, *Manpower Policy in the United Kingdom* (Paris: OECD Publications, 1970).

[29] Unfortunately the picture is complicated by the fact that a number of other macroeconomic policy changes were implemented during the 1965–67 period. These policy changes are reviewed by Bowers et al., in "The Change in the Relationship Between Unemployment and Earnings Increases: A Review of Some Possible Explanations," *National Institute Economic Review* (November 1970), 44–63. However, the survey-based analysis of D. MacKay and G. Reid in "Redundancy, Unemployment and Manpower Policy," *Economic Journal* (December 1972), 1256–72, leaves little doubt that the new compensation law had a significant effect on the duration (and thus the rate) of unemployment. Also see the discussion by M. Feldstein, "The Economics of the New Unemployment," *Public Interest*, 33 (Fall 1973), 3–42.

$$U_t = \frac{\beta_1}{1-\delta_1 L} G_{t-1} + \frac{\beta_2}{1-\delta_2 L} C_t + \frac{\theta_0 + a_t}{(1-L^4)(1-\phi_1 L)} \quad (6)$$

The revised model in (6) allows the introduction of the new unemployment compensation system as well as unrelated interparty differences in macroeconomic policy to alter gradually the level of British unemployment.

Table 2 reports the estimation results for equation (6).[30] All coefficients (except the constant or trend term θ_0) are substantially larger than their estimated standard errors and therefore are significant by conventional statistical criteria. Before considering the implications of these estimates, let us first evaluate the adequacy of the fitted model. Figure 6 shows the actual and predicted levels of the unemployment time series.[31] The predicted unemployment observations track the actual data quite well, which of course is expected in view of the highly significant parameter estimates and small residual variance reported in Table 2. Diagnostic checks applied to the residuals provide more convincing evidence of the model's adequacy. Figure 7 presents the residual autocorrelations $r_k(\hat{a}_t)$ for lags 1 through 25. The autocorrelations exhibit no systematic patterns and, except for $k = 4$, fall within the approximate ±2 standard deviation limits.[32] The mean of the residuals is $\bar{a} = .0000003$ and the estimated standard error $\sigma_{\hat{a}}^2 = .023$. The sample evidence strongly suggests therefore that the a_t are independently distributed random variates with zero means.

Returning to the parameter estimates in Table 2, interest centers on the intervention coefficients $\hat{\beta}$ and $\hat{\delta}$. The coefficients associated with the unemployment compensation dummy variable C_t ($\hat{\beta}_2$, $\hat{\delta}_2$) indicate that the additional unemployment benefits available since October 1966 produced a net increase of about 0.86 percent in the equilibrium level of unemployment, that is,

$$+\frac{\hat{\beta}_2}{1-\hat{\delta}_2} = \frac{+.511}{1-.407} = 0.86.$$

In view of the fact that the dynamic response parameter $\hat{\delta}_2 = .407$, the steady state effect of

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[30] The models in this section were estimated with Kent D. Wall's ERSF program, which provides Full Information Maximum Likelihood estimates of Rational Distributed Lag Structural Form equations. Details are given in Wall, "FIML Estimation of Rational Distributed Lag Structural Form Models," Working Paper No. 77 (Cambridge: National Bureau of Economic Research, Inc., March, 1975).

[31] The predicted level data are obtained by summing the predicted four-quarter difference series, i.e.,

$$\hat{U}_t = U_0 + \sum_t \widehat{(1-L^4) U_t}.$$

The summation operator Σ is the inverse of the difference operator $(1-L)$ in the same way that integration is the inverse of differentiation in continuous time problems.

[32] The lag 4 autocorrelation is of course significant and therefore the model might be improved by specifying $a_t = (1-\theta_4 L^4) v_t$ where the v_t are $N(0,\sigma_v^2)$. Since the k=4 autocorrelation was essentially induced by the seasonal differencing (which overcompensates for the four-quarter seasonal dependency), and we are primarily interested in predicting the level unemployment series, modification of the model in this way is not advantageous.

Table 2. Estimation Results for the British Unemployment Rate Model (Eq. 6)

	Parameter Estimates	Standard Errors
G_{t-1}	$\hat{\beta}_1 = -.094$ $\hat{\delta}_1 = +.692$	.035 .118
C_t	$\hat{\beta}_2 = +.511$ $\hat{\delta}_2 = +.407$	.115 .228
Trend (4 quarter)	$\hat{\theta}_0 = +.002$	.023
Autoregressive	$\hat{\phi}_1 = +.773$	.071
	Residual Variance, $\hat{\sigma}_a^2 = .045$	$R^2 = .95$[a]

[a] The R^2 reported here pertains to the level data rather than to the four-quarter difference data. The four-quarter difference R^2 is .85.

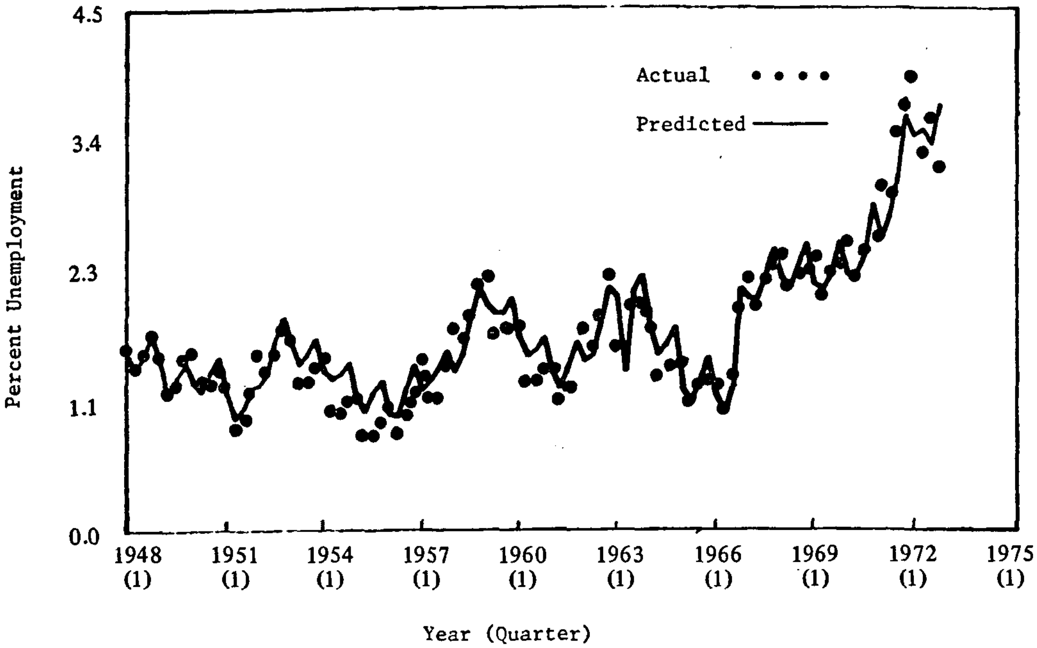

Figure 6. Actual and Predicted Values from the British Unemployment Rate Model (Eq. 6)

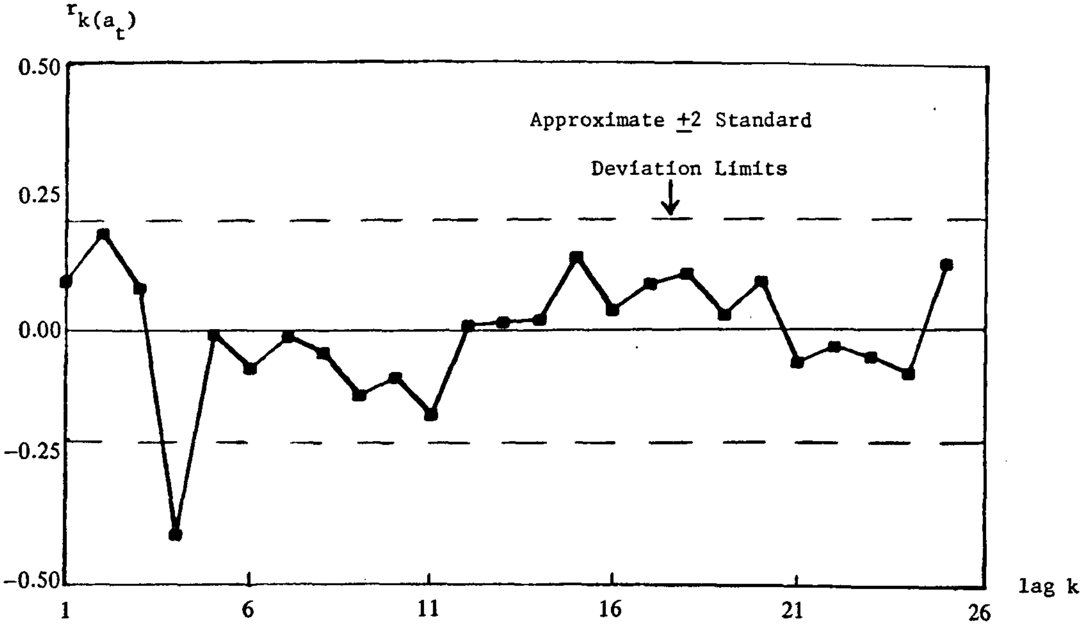

Figure 7. Residual Autocorrelations from the British Unemployment Rate Model

0.86 percent was fully realized rather quickly—after only four or five quarters. However, effects associated with the C_t term, although sizable, are only of incidental interest in this study.

More important for our purposes are the maximum likelihood estimates of β_1 and δ_1 which clearly support our initial proposition concerning the impact of partisan change on the British unemployment rate. Net of the effects attributed to the new unemployment compensation law, and independent of trends, seasonal dependencies, and stochastic fluctuation in the time series, the unemployment rate appears to be driven downward during the tenure of Labour governments and to move upward during periods of Conservative rule. The estimated steady state effects are ±0.31 percent, that is:

$$\pm \frac{\hat{\beta}_1}{1-\hat{\delta}_1} = \pm \frac{.094}{1-.692} = \pm 0.31,$$

which implies a difference of about 0.62 percent between the equilibrium unemployment levels associated with Labour and Conservative governments. Holding fixed the C_t variable and the stochastic ARMA terms in the model, we see that the expression $U_t = \frac{\hat{\beta}_1}{1-\hat{\delta}_1 L} G_{t-1}$ implies $U_t = \hat{\delta}_1 U_{t-1} + \hat{\beta}_1 G_{t-1}$, which upon repeated substitution gives:

$$U_t = \hat{\delta}_1^t U_0 + \hat{\beta}_1 \sum_{i=0}^{t-1} \hat{\delta}_1^i G_{t-1-i} . \qquad (7)$$

Imposing the arbitrary initial condition $U_0 = 0$ and applying the coefficient estimates $\hat{\beta}_1 = -.094$, $\hat{\delta}_1 = .692$, we obtain the dynamic time paths of the unemployment rate that can be attributed to Labour and Conservative macroeconomic policies by simulating (7) for G_t held at +1 and −1, respectively. Figure 8 depicts the unemployment time paths for regimes of 20 quarters (5 years) duration. Notice that the steady state values of ±0.31 percent are fully realized after about 16 quarters or 4 years.

An interparty difference of just over one-half of one percent in government-induced unemployment levels may seem small by American standards, but, if evaluated against Great Britain's average postwar unemployment rate of 1.67 percent, it is by no means trivial. Applied to the British civilian labor force, which has averaged 24.1 million workers during the postwar period, the effects graphed in Figure 8 translate into about 149,000 jobs. Since British unemployment data are compiled by the regis-

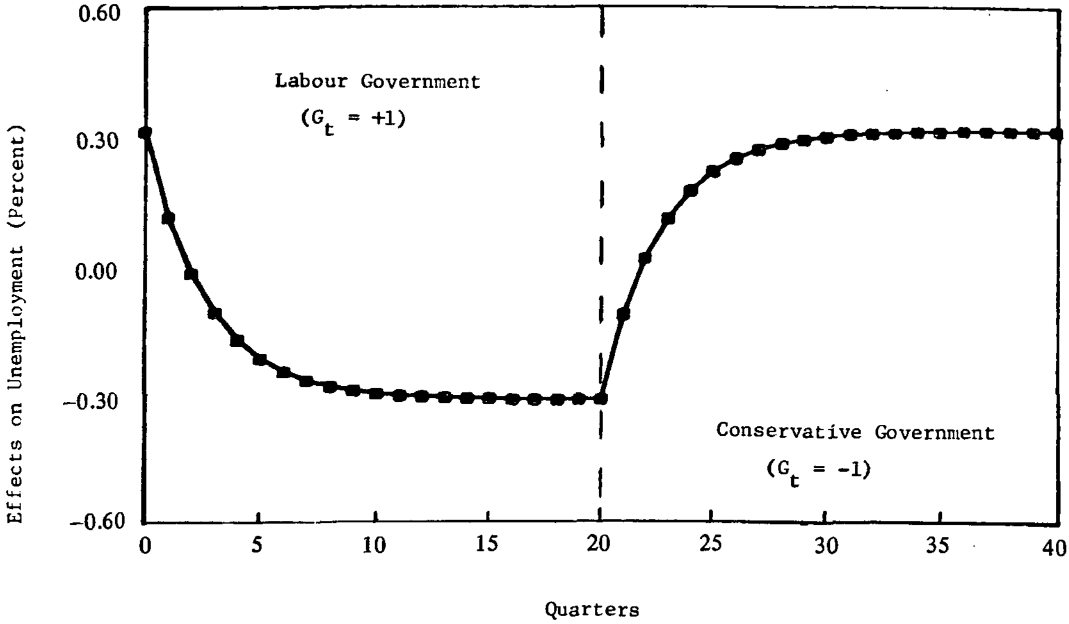

Figure 8. Simulated Net Effects of Labour and Conservative Governments on the Unemployment Rate

tration method, the measured unemployment rate tends to be biased downward relative to that of the United States, which is based on labor force survey data. Adjusting the British data to the American definition therefore permits more accurate comparisons to be made with the U.S. experience. Myers estimates the adjustment factor to be 1.51.[33] Applying this to the British data yields an interparty steady state difference of 0.94 percent, or about 226,000 jobs.

Nonetheless, the estimated effect of Labour versus Conservative macroeconomic policies on the equilibrium level of unemployment is perhaps smaller than one might have anticipated from the earlier discussion of left-to-right cleavages regarding various economic goals. Indeed the ideological distance between the Labour and the Conservative parties on the full employment issue is undoubtedly not as great as that implied by the general scheme introduced previously in Table 1. Throughout the postwar period the Conservatives have made great efforts to disassociate themselves from the mass unemployment of the 1930s by repeatedly emphasizing their commitment to the full employment goal, although in practice it was sometimes viewed as necessary to induce increases in unemployment in order to fight inflation. However, it should be recognized that, unlike the United States, Great Britain is very much an open economy and the macroeconomic policies of both Labour and Conservative governments have been severely constrained by the necessity of maintaining a satisfactory external trade balance. Political authorities of both parties had to insure that the country did not inflate at a rate exceeding that of its principal trading partners in order to maintain the competitiveness of British exports in world markets. In view of the international economic constraints facing all British governments, the estimated interparty difference of 0.62 percent (0.94 percent adjusted to U.S. concepts) does not appear quite as modest in magnitude.

The U.S. Unemployment Model. The impact of Democratic versus Republican administrations on the U.S. unemployment rate is also estimated by developing an ARMA-intervention model. The model building procedure is the same as that outlined in the course of the British analysis. Figure 9 shows the sample autocorrelation function for seasonally adjusted

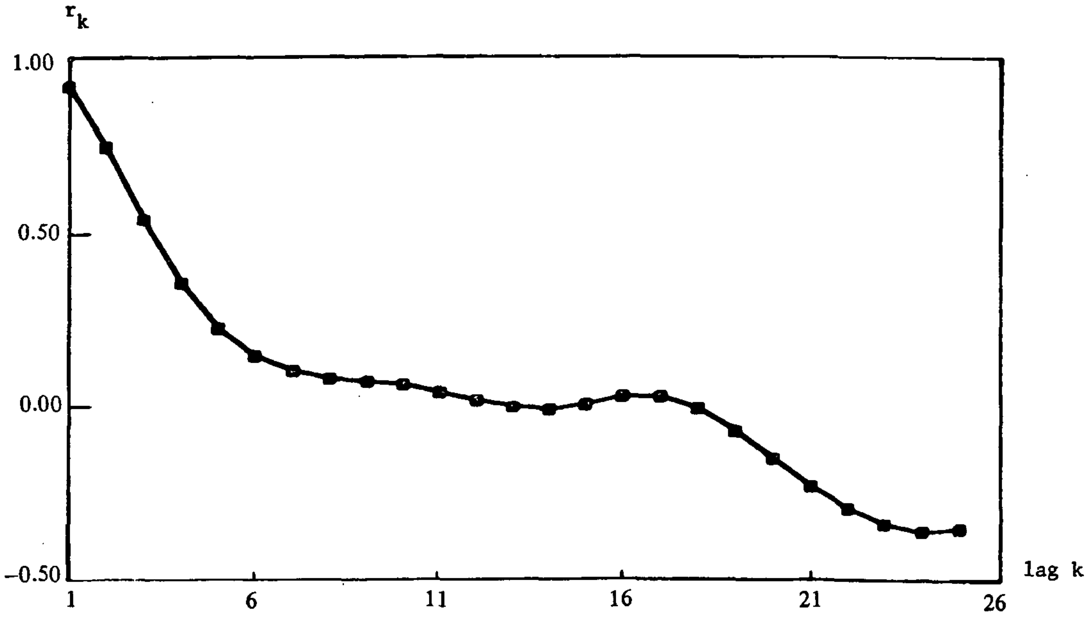

Figure 9. Sample Autocorrelation Function of the U.S. Unemployment Rate Data, 1948(1)–1972(4)

quarterly observations on the U.S. employment rate over the period 1948(1)–1972(4). The autocorrelations exhibit mild oscillations and decay as the lag k increases–properties which are characteristic of a low-order autoregressive process. Partial autocorrelations (which are not reported here) are significant for $k \leqslant 2$, and therefore we propose a second-order process for the stochastic component of the intervention model:[34]

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[33] R. J. Meyers, "The Unemployment Problem: What We Can Learn from European Experience," in *Measuring Employment and Unemployment* by the Joint Economic Committee of the U.S. Congress (Washington, D.C.: Government Printing Office, 1963).

$$U_t = \theta_0 + \phi_1 U_{t-1} + \phi_2 U_{t-2} + a_t, \text{ or} \qquad (8)$$

$$U_t = \frac{\theta_0 + a_t}{1 - \phi_1 L - \phi_2 L^2}.$$

Adjoining (8) to the intervention function introduced previously in (1) yields the estimating equation:

$$U_t = \frac{\beta}{1 - \delta L} G_{t-1} + \frac{\theta_0 + a_t}{1 - \phi_1 L - \phi_2 L^2} \qquad (9)$$

where: U_t = the percentage of the civilian labor force unemployed in the U.S. quarterly 1948(1)–1972(4);

G_t = +1 during Democratic administrations, −1 during Republican administrations;

and other terms are as previously defined.

In its present form, the model in equation (9) is unlikely to provide a very good estimate of the net effect of Democratic versus Republican macroeconomic policies on the U.S. unemployment rate. An important omitted variable, which is not likely to be captured by the autoregressive terms in the model, is American intervention in the Korean and Vietnamese civil wars. The enormous fiscal stimulus to the domestic economy (not to mention the sizable number of young men withdrawn from the civilian labor force) generated by American participation in these conflicts shows up clearly in the steadily declining unemployment rates of the war years. (The same can of course be said about the contribution of World War II to the recovery from the Great Depression.) Indeed the United States experienced its lowest postwar unemployment rates during the peaks of these wars. Since American involvement in the Korean and Vietnamese conflicts occurred during (covaried with) Democratic administrations, it is necessary to include an additional "war" term in the model in order to disentangle the party effects of interest from the war effect. Therefore we introduce an additional variable W_t taking a value of +1 during the Korean and Vietnamese wars and a value of 0 otherwise, and specify the revised model

$$U_t = \frac{\beta_1}{1 - \delta_1 L} G_{t-1} + \frac{\beta_2}{1 - \delta_2 L} W_t + \frac{\theta_0 + a_t}{1 - \phi_1 L - \phi_2 L^2}. \qquad (10)$$

The specification of the W_t term in (10) is identical to that of the G_t term, except that the war variable appears without a delay or lag. The revised model therefore allows the economic stimuli accompanying American intervention in Korea and Vietnam as well as non-war-related interparty differences in macroeconomic policy to alter gradually the level of unemployment.

Estimation results for the U.S. unemployment model of equation (10) are presented in

Table 3. Estimation Results for the U.S. Unemployment Rate Model (Eq. 10)

	Parameter Estimates	Standard Errors
G_{t-1}	$\hat{\beta}_1 = -.071$	.020
	$\hat{\delta}_1 = +.974$	.017
W_t	$\hat{\beta}_2 = -.179$	.145
	$\hat{\delta}_2 = +.513$	.320
Autoregressive	$\hat{\phi}_1 = +1.49$	.072
	$\hat{\phi}_2 = -.718$	.071
	Residual Variance, $\hat{\sigma}_a^2 = .085$	$R^2 = .94$

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[34] Equation (8) is nearly identical to the model developed by C. R. Nelson for quarterly U.S. unemployment data over the period 1948(1)–1966(4). Nelson's model, incidentally, outperformed the MIT-FRB-Penn econometric model in short-term forecasting experiments. "The Predictive Performance of the FRB-MIT-PENN Model of the U.S. Economy," *American Economic Review* (1972), 902–17.

Table 3.[35] The coefficient estimates associated with the administration term G_{t-1} ($\hat{\beta}_1, \hat{\delta}_1$) and the estimates of the autoregressive parameters ($\hat{\phi}_1, \hat{\phi}_2$) are substantially larger than their respective standard errors and thus easily satisfy the usual criteria of statistical significance. However, the coefficients associated with the war term W_t ($\hat{\beta}_2 \hat{\delta}_2$), although larger than their respective standard errors, are not significant by conventional standards, and therefore we cannot place very much confidence in these parameter estimates.[36] In view of the collinearity between W_t and G_t noted earlier, it is not surprising that $\hat{\beta}_2$ and $\hat{\delta}_2$ exhibit relatively large variances. However, we are primarily interested in securing an unbiased estimate of the net response of the unemployment rate to inter-administration differences in macroeconomic policy, and hence the war term should be retained in the model in order to insure that the administration effect is not confounded with the war effect.

The actual and predicted values of the unemployment time series are graphed in Figure 10. The fitted values track the actual data very closely and errors do not appear to exhibit any systematic pattern. The residual autocorrelations reported in Figure 11 confirm this observation. Except for $k = 8$, all of the $r_k(\hat{a}_t)$ fall within ±2 standard deviations from zero, suggesting that the a_t are independently distributed random variates.[37] Finally, the average of the residuals is $\bar{a} = -.034$ and the estimated standard error is $\sigma_{\bar{a}}^2 = .030$, which indicates that the residual mean is not significantly different from zero.

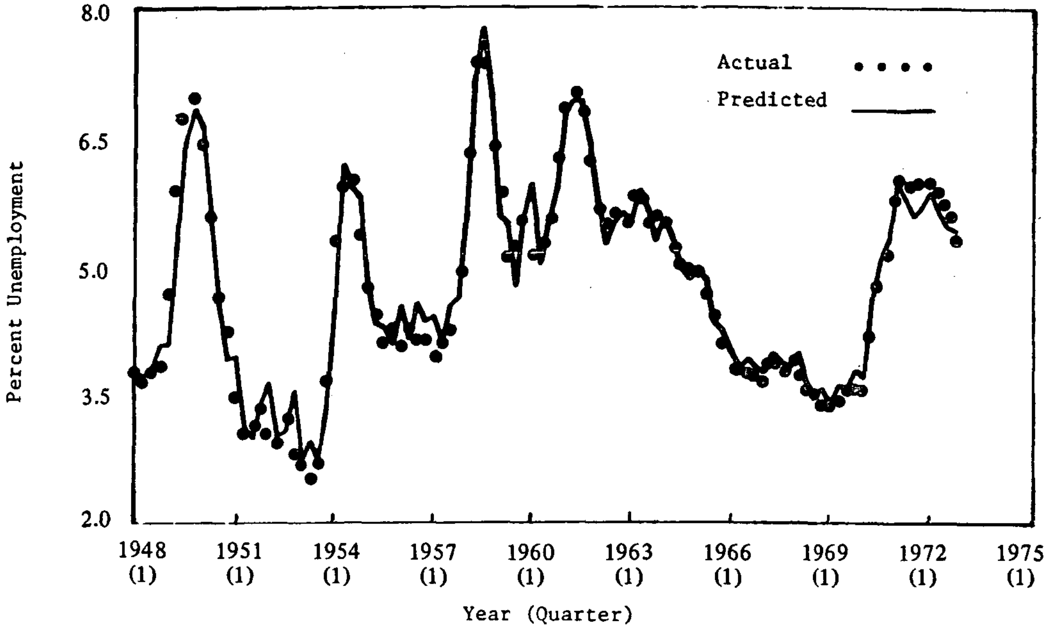

Figure 10. Actual and Predicted Values from the U.S. Unemployment Rate Model (Eq. 10)

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[35] Since the unemployment data did not exhibit a trend over the observation period, all variables were deviated from their means and the model was estimated without a constant term. θ_0 therefore does not appear in Table 3.

[36] The t ratio of $\hat{\beta}_2 = 1.23$ and of $\hat{\delta}_2 = 1.60$; both are insignificant at the .05 level. Computation of the implied dynamic response of the unemployment rate to American involvement in the Korean and Vietnamese civil wars is therefore problematic. Robert Solow has suggested to me that since the effects of both the war term and the administration term work through the actual tax, expenditure, and monetary actions of the government, the model might be better specified by constraining $\delta_1 = \delta_2$. However, estimates obtained by imposing this constraint did not alter the results reported in Table 3 and graphed below in Figure 12 appreciably: the war coefficient remained insignificant, $\hat{\delta}_1 = \hat{\delta}_2 = .969$, and $\hat{\beta}_1 = -.091$.

[37] The negative residual autocorrelation at $k = 8$ ($r_8(\hat{a}_t) = -.253$) indicates that there is a modest, negative two-year (8 quarter) dependency between U.S. unemployment rates. This is compatible with the

Having established the overall adequacy of the model, we focus on the substantive implications of the administration parameters $\hat{\beta}_1$ and $\hat{\delta}_1$. The estimates reported in Table 3 give strong support to the basic hypothesis: Democratic administrations appear to engineer downward movements in the U.S. unemployment level, whereas the reverse is true of Republican administrations. The estimation results indicate that the steady state effects are on the order of ±2.73 percent, that is:

$$\pm\frac{\hat{\beta}_1}{1-\hat{\delta}_1} = \pm\frac{.071}{1-.974} = \pm 2.73,$$

which implies an interadministration difference of about 5.46 percent in the long-run, equilibrium level of unemployment. In view of the fact that the (seasonally adjusted) U.S. unemployment rate has varied between 2.6 percent and 7.4 percent during the 1948 to 1972 period, an interadministration difference of this magnitude is simply not plausible. Note, however, that this is a steady state figure, that is, it gives the implied, net difference in unemployment levels if one and then the other party were to govern nationally for an indefinitely long period of time. Since the dynamic adjustment parameter $\hat{\delta}_1$ is estimated to be .974, convergence to equilibrium is very slow and would not be fully realized until a given party had held office for more than 100 quarters or 25 years.[38] However, neither political party in the

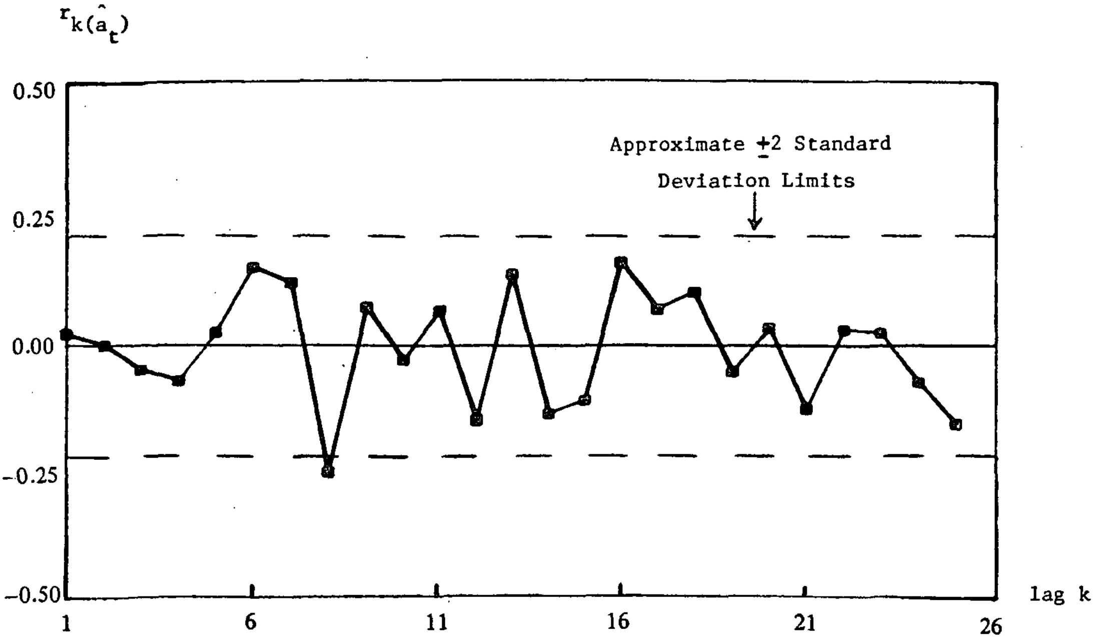

Figure 11. Residual Autocorrelations from the U.S. Unemployment Rate Model

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political-electoral business cycle argument of Nordhaus, Tufte and others, in which unemployment tends to fall before Presidential elections and to rise thereafter in response to administration efforts to engineer favorable economic conditions just prior to elections and to postpone austerity measures until after elections are safely over. [Nordhaus, "The Political Business Cycle;" and Edward Tufte, *Elections and Economics: Macroeconomics Under Conditions of Political Competition* (Princeton, N.J.: Princeton University Press, forthcoming).] If this pattern was strong and more or less uniform across four-year presidential administrations, we should observe a sizable negative autocorrelation at $k = 8$ (two-year intervals) and a positive autocorrelation at $k = 16$ (four-year intervals). Although the focus of this study is on long-run patterns in macroeconomic policies and outcomes that distinguish left- and right-wing regimes, attempts were made to build an electoral unemployment cycle of this sort into the model. However, elaborations of the model along these lines did not yield significant results.

[38] This is readily confirmed by evaluating the expression $\hat{\beta}_1 \sum_{i=0}^{\infty} \hat{\delta}_1^i G_{t-1-i}$ over the index i for fixed G_t. Unlike the British results, which implied convergence to steady state after only 16 quarters, the U.S. steady state is not reached until the index i is taken to well over 100 quarters.

United States has held the presidency for more than two terms in succession during the postwar period, and therefore it is sensible to restrict the interpretation of the estimation results to 32 quarters or 8 years.

Figure 12 shows the dynamic time paths of the unemployment rate implied by the G_t component of the model for Democratic and Republican administrations, respectively.[39] Notice that after 32 quarters (i.e., two presidential administrations) the estimated administration effects are on the order of ±1.18 percent and hence the interadministration difference in government induced unemployment levels is about 2.36 percent. This estimate is of course much more compatible with the postwar U.S. experience than the long-run, steady state difference of 5.46 percent reported earlier. A comparison of the U.S. results in Table 3 and Figure 12 to the corresponding results for Great Britain in Table 2 and Figure 8 also indicates that the ultimate impact of an administration on the rate of unemployment accumulates much more slowly in the United States than in Great Britain. In other words, the results suggest that the effects of government macroeconomic policies on the unemployment rate are processed much more quickly through the British system than through the American system. These inferences are entirely reasonable in view of the fact that the political and economic environment facing macroeconomic policy makers in the United States is considerably more decentralized and heterogeneous than that facing macroeconomic policy makers in the parliamentary system of Great Britain.

Discussion

The estimated interparty difference of 2.36 percent in the unemployment performance of Democratic versus Republican administrations is perhaps best illustrated historically by con-

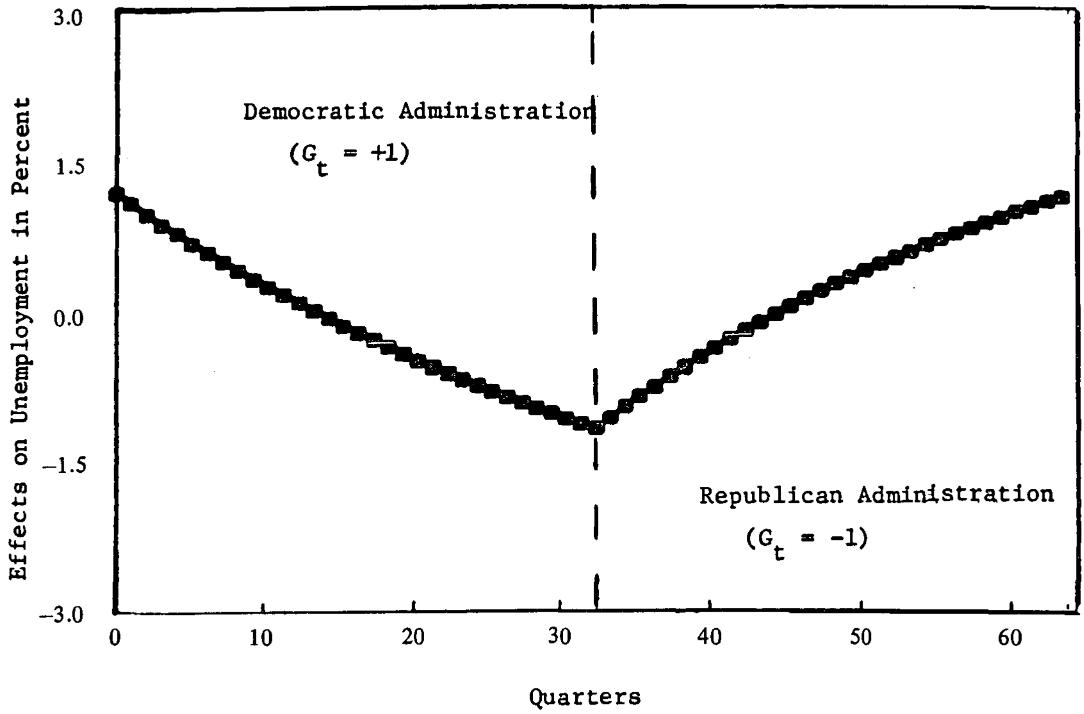

Figure 12. Simulated Net Effects of Democratic and Republican Administrations on the Unemployment Rate

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[39] The results graphed in Figure 12 were obtained in the same way as described earlier for the British case, i.e., by simulating

$$U_t = \hat{\delta}_1^t U_0 + \hat{\beta}_1 \sum_{i=0}^{t=1} \hat{\delta}_1^i G_{t-1-i}$$

for G_t held at +1 and then −1 over regimes of 32 quarters (8 years).

trasting briefly the macroeconomic policies of the Eisenhower, Kennedy-Johnson and Nixon administrations. The principal economic goals of the Eisenhower administrations were a balanced federal budget and a reduction in the rate of inflation.[40] Despite repeated contractions in aggregate economic activity (the "Eisenhower recessions"), full employment and economic expansion never became primary goals. Indeed the emphasis on price stability and a balanced budget was so great that federal expenditures were actually decreased during the 1953–54 recession and budget outlays only barely exceeded receipts during the recession years of 1957–58 and 1960.[41]

The excessive caution exercised by the Eisenhower administrations in dealing with recession and the great weight placed upon price stability were of course roundly attacked by liberal Keynesian economists, organized labor, and others. In his memoirs of this period Eisenhower responded to such criticism by noting that "critics overlooked the inflationary psychology which prevailed during the mid-fifties and which I thought it necessary to defeat. . . . The anti-inflation battle is never-ending, though I fear that in 1959 the public was apathetic, at least uninformed, regarding this issue."[42] The consequence of this never-ending battle against inflation was an economy that was chronically in stagnation and an unemployment rate that regularly exceeded six percent.

The Kennedy-Johnson administrations' posture toward recession and unemployment stands in sharp contrast to Eisenhower's. The most significant manifestation of the greater importance attached to full employment and economic expansion by these Democratic administrations was the 1964 tax cut. First proposed publicly by Kennedy in June 1962, introduced in Congress in January 1963, and signed into law by Johnson in February 1964, the Revenue Act of 1964 injected a ten billion dollar fiscal stimulus into a sagging economy. This represented a clear break with the budget balancing ideology of previous Republican administrations (although the rhetoric of the balanced budget lingered on) and, in view of the economic outlook at the time and the historical periodicity of U.S. recessions, undoubtedly helped prevent a serious economic contraction in 1964–65 and thereby contributed to the prolongation of the longest expansion in postwar U.S. history. Johnson defended government initiatives on the employment front by arguing that "the number 1 in priority today is more jobs. This is our dominant domestic problem and we have to face it head-on."[43]

The basic economic priorities associated with the Eisenhower era were reestablished during the Nixon and Ford administrations. Although Nixon-Ford macroeconomic policies were more interventionist than those of earlier Republican administrations, high employment once again was sacrificed for the sake of restraining inflation. It is generally agreed that the 1970–71 recession was deliberately induced by the Nixon administration to check inflation, though this policy was later jettisoned in an attempt to stimulate a pre-election boom. In most respects the short-lived Ford administration was a replay of the Eisenhower years. The macroeconomic game-plan called for running the economy at considerable "slack" to reverse "inflationary expectations," and repeated attempts by the Democratic Congress to pass measures promoting a more rapid economic expansion were vigorously opposed.

Macroeconomic outcomes, then, are not altogether endogenous to the economy, but obviously are influenced to a significant extent by long- and short-term political choices. The real winners of elections are perhaps best determined by examining the policy consequences of partisan change rather than by simply tallying the votes.

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[40] See, for example, O. Eckstein, "Economic Policy in the United States," in *Economic Policy in Our Time,* Vol. II, ed. O. Eckstein et al. (Amsterdam, North Holland, 1964), pp. 1–88; and especially H. Stein, *The Fiscal Revolution in America* (Chicago: University of Chicago Press, 1969), Ch. 11–14.

[41] Many analysts argue that Eisenhower's fiscal policies not only did little to combat the economic contradictions of the period but were also important causes of the 1957–58 and 1960–61 recessions. See W. Lewis, *Federal Fiscal Policy in the Postwar Recessions* (Washington, D.C.: The Brookings Institution, 1962); and Stein, *The Fiscal Revolution in America.*

[42] D. Eisenhower, *Waging Peace, 1956–61* (Garden City, N.Y.: Doubleday, 1965), pp. 461, 462.

[43] Cited in F. R. Dulles, *Labor in America* (New York: Crowell 1966), p. 394.

A Critique of "Democratic Administration" and Its Supporting Ideation*

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This paper analyzes Vincent Ostrom's major work, The Intellectual Crisis in American Public Administration, *which he offers as providing paradigmatic direction for public administration and political science. The analysis urges caution as to that theory's status, especially from five analytic perspectives. Basically, attention is directed at the methodology or mode of inquiry associated with Ostrom's grounding of his argument in public choice theory, with special attention to the role of values. The adequacy of major assumptions of Ostrom's argument as descriptions of reality also is evaluated. Moreover, the critical lack of content in several key concepts is established. In addition, the analysis shows how opposite and simultaneous courses of action are implied by the argument. Finally, attention is directed at how Ostrom's argument can lead to unexpected consequences, even some that are opposite those effects Ostrom intends.*

In the ongoing ferment about leading ideas to guide public administration, attention has focused on Vincent Ostrom's effort to make "democratic administration" *the* paradigm of the 1970s.[1] This essay acknowledges Ostrom's centrality, and also provides some of the critical treatment that his ideas require. In dealing with Ostrom's "democratic administration," this critique necessarily refers to the broad ideational infrastructure of public choice theory on which Ostrom relies.

Given the critical tone of much of what follows, five points of agreement with Ostrom deserve highlighting:

1. It is inadequate to separate sharply politics and administration, as serious students have acknowledged for several decades.
2. Monocratic bureaucracy deserves criticism, both empirically and normatively, as a huge literature testifies.
3. Individuals should have a broader role in influencing public policy and its administration, as both members and clients of public agencies.
4. Greater administrative responsiveness to clients' perceptions of needs, as well as greater productivity in providing public services, are to be desired and worked for.
5. Economic modes of thought and analysis can be powerful aids to enlightened choice making.

The basic concern here is more with Ostrom's argumentation than with his conclusions, as the three parts of this analysis reveal. Two early sections briefly introduce Ostrom's view of the major contenders for the dominant paradigm to guide the study and practice of public administration, with special attention to how these contenders deal with such central questions as determining the scale of public delivery systems. A long argument then provides a detailed analysis of the ideation supporting democratic administration, that ideation being seen as basically inadequate from five central perspectives.

Two Contenders for the Reigning Paradigm

Ostrom has two goals; thus he seeks to explain the "intellectual crisis" of public administration by pitting against one another two broad and contrasting paradigms—"paradigm" meaning roughly the set of guiding ideas which detail "significant" problems, how these problems are to be analyzed, and in what priority they are to be dealt with. One paradigm is the cause of the crisis, and the second constitutes the cure. Simply, Ostrom argues, supplant one of those paradigms, root and branch, by the other.

The first or "traditional" paradigm—clearly recognizable as close kin to the monocratic

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*This paper is one reflection of a substantial history of exchanges of ideas and papers that the author has had with a colleague, Dr. Keith Baker. Other reflections appear in Baker's works: "Public Choice Theory: Some Important Assumptions and Public Policy Implications," in *Public Administration*, ed. Robert T. Golembiewski, Frank Gibson, and Geoffrey Y. Cornog, 3rd ed. (Chicago: Rand McNally, 1976), pp. 42–60; and "The Search for a New Paradigm in Public Administration," paper delivered at the Annual Meeting, Southern Political Science Association, New Orleans, La., November 1974. The author warmly acknowledges that interchange.

[1] Vincent Ostrom's *The Intellectual Crisis in American Public Administration* (University, Ala.: University of Alabama Press, 1973) is the central source.

bureaucracy of Max Weber—has a "top, down" bias and a long-lived influence. Its overall image, in Ostrom's telling, is an overflowing font of sovereignty, authority being concentrated at high levels but allowed to trickle variously down the hierarchy of state enterprises. More specifically, the traditional paradigm postulates that there will be a single dominant center of power in any system of government. Consequently, all modern governments face the same challenge: developing a unitary hierarchy under a single executive, which leads a corps of trained and politically neutral professionals.[2] These notions, patently consistent with a naive separation of politics and administration, provided essential guidance for a half-century or more to both students and practitioners of public administration, Ostrom advises.

Ostrom offers a contrasting, alternative, and (to him) more useful paradigm, "democratic administration." That alternative has a "bottom, up" and heterogeneous character, in contrast with the ordered symmetry of a hierarchy. To illustrate, democratic administration rests on such propositions as the following:[3]

1. Perfection in the hierarchical ordering of a professionally trained public service accountable to a *single* center of power will reduce the capability of a large administrative system to respond to diverse preferences among citizens for many different public goods and services and also will reduce its ability to cope with diverse environmental conditions.
2. Perfection in a hierarchical organization accountable to a *single* center of power will *not* maximize efficiency as measured by least cost expended in time, effort, and resources.
3. There are two necessary conditions for maintaining a stable political order which can advance human welfare under rapidly changing conditions: fragmentation of authority among diverse decision centers with multiple veto capabilities in any one jurisdiction; and the development of multiple, overlapping jurisdictions of widely different scales.

Criteria Central to Democratic Administration: Determining the Scale of Public Delivery Systems

Even the sparse illustrations above clearly imply that democratic administration must deal with major problems that are superficially finessed by the traditional paradigm. Note only two questions which Ostrom's alternative must face: What should be the scale of public service delivery systems? How should they be differentiated and coordinated?

Such questions are not central ones for the traditional paradigm, which simply focuses on approaching the ideal of one increasingly integral authority structure. Both the implied theoretical and practical urgencies, then, center around reducing any extant autonomy of component government units so as to increase the reality of *a* "chain of command." Hence this answer to the key issue of how big a public service delivery system should be: as big as necessary to create a unitary organization under a single head. This is a prescription for monolithic giantism, clearly.

The literature on organizations is in fact far from being as monolithic as Ostrom usually insists, which oversight at once makes his analytic task simpler in a trivial sense and more difficult in more central senses. Beyond the core agreement about initial centralization, opinions of those supporting the traditional paradigm tend to fall into two camps. One camp maintains that effective centralization must precede effective decentralization. Its proponents presume that acting on the urgencies above is only a necessary prelude to dealing with the issues of fine-tuning the system, of determining the scale of component units as well as of defining a rationalized system of differentiated/integrated subsystems. In more extreme versions of the traditional paradigm, however, the achievement of the totally rationalized and centralized system is seen as *the* goal rather than as a prelude to some newly reachable goals.

Ostrom fixates on the latter interpretation, which constitutes an easier but simplified analytical target. Had he acknowledged the former interpretation more consistently, Ostrom's argument could have differed profoundly from what it actually came to be. Rather than a radical, either/or opposition of paradigms, for example, his argument could have then been framed in developmental terms, as evolutionary rather than as revolutionary. This would have been more faithful to the history of the relevant literatures.

Ostrom rejects the traditional paradigm, however, and does not acknowledge the crucial distinction above. In contrast, he offers four criteria which deal with the complex problems of scale in governmental units in an uncomplicated way. Basically, his goal is to define suitably a "package" for the boundaries of a unit of government such that those outside the boundaries are excluded from the use of the public goods or services provided by that unit. The criteria are sketched in Table 1.

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[2] Ibid., esp. pp. 28–29.

[3] Ibid., esp. pp. 111–12.

A Critical Analysis of the Ideation Underlying Democratic Administration

Democratic administration has received little critical analysis,[4] despite the burgeoning attention devoted to the "public choice" context of which it is both creator and product. To be sure, Ostrom does note that the above criteria "are sometimes in conflict." And he does allow that when the criteria are not applied adequately, the "package" for the production and provision of public goods by the governmental unit whose boundaries are being drawn will be faulty. This will result in "externalities [which] spill over upon neighboring communities."[5]

The critical analysis here is more ambitious than Ostrom's brief qualifications. By way of introduction, the following argument seeks to establish a broader indictment of Ostrom's argument and its underlying ideation, particularly on these issues:

1. The methodology underlying democratic administration is awkward.
2. Major assumptions in Ostrom's argument are at least improbable descriptors and may be seriously incongruent with reality.
3. Critical content or meaning is lacking in major concepts underlying democratic administration.
4. Opposite and simultaneous courses of action are implied by central aspects of Ostrom's system of thought.

Table 1. Criteria for Determining the Scale of Public Delivery Systems[a]

1. *Control.* The broad "boundary conditions" of a political jurisdiction should be so defined as to "include the relevant sets of events to be controlled." Such relevant events are not uniformly distributed in space or time—whether they involve a watershed basin or patterns of human interaction—so these boundaries can only be defined "with more or less precision." Too narrow boundaries disable a public agency and likely will result in a transfer of that agency's function to a governmental unit "scaled to meet the criterion of control more adequately."

2. *Efficiency.* "The most efficient solution would require the modification of boundary conditions so as to assure a producer of public goods the most favorable economy of scale, as well as effective control."

3. *Political Representation.* This criterion requires "the inclusion of the appropriate political interests within [each boundary's] decision-making arrangements." The issues are complex, but the ideal solution—assuming "criteria of responsibility and accountability consonant with democratic theory"—would require that "three boundaries be coterminous." These boundaries are:

   a.) the *scale of formal organization,* or the size of the government unit providing some public good;
   b.) the *public,* consisting of those affected by the provision of that good;
   c.) the *political community,* or those "who are actually taken into account in deciding whether and how to provide" that public good.

4. *Local Self-Determination.* The application of the three preceding criteria to local governments in the United States is usually to be "subordinated to considerations of self-determination"; that is, application is usually to "be controlled by the decisions of the citizenry in the local community."

The basic assumption is that public goods can be appropriately "packaged" or "internalized," so that those outside the boundaries of a local unit of government can be excluded from the public goods produced by that unit. Looked at from another perspective, the assumption is that the "purely 'municipal' affairs of a local jurisdiction, presumably, do not create problems for other political communities." Relatedly, also, this fourth criterion implies that the local citizenry are both capable of and motivated toward holding public officials accountable, in ways that do not create major problems for other political communities.

[a] *Source:* Vincent Ostrom, Charles M. Tiebout, and Robert Warren, "The Organization of Government in Metropolitan Areas," *American Political Science Review,* 55 (December 1961), esp. 835–37.

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[4] For exceptions to the lack of critical attention, see Thomas R. De Gregori, "Caveat Emptor: A Critique of the Emerging Paradigm of Public Choice," *Administration and Society,* 6 (August 1974), 205–28; and Keith Baker, "Public Choice Theory: Some Important Assumptions and Public Policy Implications," in *Public Administration,* ed. Robert T. Golembiewski, Frank Gibson, and Geoffrey Y. Cornog, 3rd ed. (Chicago: Rand McNally, 1976), esp. pp. 42–60.

[5] Vincent Ostrom, Charles M. Tiebout, and Robert Warren, "The Organization of Government in Metropolitan Areas," *American Political Science Review,* 55 (December 1961), 835.

5. Democratic administration can lead to unexpected practical consequences, sometimes the opposites of those intended.

Methodological Concerns. The central discrepancies in Ostrom's position are methodological and constitute slippery targets because his usages (and the usages of those public choice theorists on which Ostrom explicitly relies) are so alluring as to invite incautious acceptance. This section will briefly discuss several of these methodological concerns, and subsequent sections will add substance to this lean argument.

Basically, the public choice position rests upon a methodology that emphasizes a closed-system circularity, while it permits incautious building upon assumptions that are often suspect. These propensities to circularity and incaution raise serious concerns about the argument built on such foundations. The circularity is often explicit in the key questions for inquiry, as in: "When will a society composed of free and rational utility-maximizing individuals choose to undertake action collectively rather than privately?" It comes as no surprise to learn that collective action will occur when (enough?) individuals realize that they can increase their "utility" by so organizing.[6] The line of argumentation is tendentious because "free and rational utility-maximizing individuals" could hardly have done it any other way. Worse still, rather than identifying or describing or predicting, this approach permits only a posteriori classification, as in: Aha, there is a collective action; ergo, someone's utility has been maximized. The lack of caution in argument will be illustrated at several points below, especially in the case of assumptions that conflict with reality.

Four points provide further perspective on this closed-system and convenient methodology. First, prominent public choice theorists seek to have the best of all analytical worlds in casually shifting the base of their arguments. Consider the central notion of "methodological individualism"—on which the public choice argument is typically predicated and which Ostrom tells us he accepts.[7] The concept seeks to explain collective decision making as a simple summation of the individual decisions made by separate and distinct persons; and it is deliberately contrasted with "normative individualism," which Buchanan and Tullock stress is rooted in the "explicit acceptance of certain value criteria."[8] Perhaps the intent is to represent the public choice argument as one of science rather than of personal bias. Whatever the intent, Buchanan and Tullock are not sufficiently restrained by their own tethers. For example, they also announce early on that they "are not directly interested in what *the* State or *a* State actually is, but propose to define quite specifically, yet quite briefly, what *we think* a State ought to be."[9] De Gregori arches an analytic eyebrow: "Methodological or normative?" he asks rhetorically of this elastic aspect of the authors' system of thought.[10]

Second, related major concerns about Ostrom's argument derive from the assumptive and derivative nature of the core ideational infrastructure of the theory of public choice. Numerous evidences and consequences of this assumptive and derivative character will be developed later, but one elementary point is patent even now: the character of the assumptions is vital because it predetermines the range of derivations, given nimble minds and consistent logic. The cruciality of these assumptions does not get sufficient respect in the ideational infrastructure on which Ostrom relies. Why this neglect exists is puzzling, for some of those assumptions are at least controversial, if not in fact denied by some of those involved in the technical development of public choice theory.[11] So flexible is Ostrom's underlying methodology, indeed, that such controversiality or denial is taken in full stride, without apparent conceptual pause. His methodology, in fact, often has a heads-I-win, tails-you-lose quality. When pushed on the issue of the "real-world inapplicability" of one of his assumptions, or about a conflict between assumptions, illustratively, Ostrom only allows that "it might be necessary" to make different assumptions as to the "prevailing human condition."[12]

Norton Long goes for what he sees as the jugular vein of this kind of reliance on those types of assumptions. They may not mirror reality and, hence derivations from them are highly suspect. Referring to two well-known public choice theorists, for example, Long concludes that they "can argue with elegant and impeccable logic about unicorns. The cogency of their logic, however, does not demonstrate the existence of unicorns."[13]

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[6] James M. Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor, Mich.: University of Michigan Press, 1962), esp. pp. 43–48.

[7] Ostrom, esp. pp. 50–51.

[8] Buchanan and Tullock, p. vii.

[9] Ibid., p. 3.

[10] De Gregori, p. 208.

[11] William A. Niskanen, Jr., *Bureaucracy and Representative Government* (Chicago: Aldine-Atherton, 1971), esp. p. 218.

[12] Ostrom, p. 52.

Third, and in equal measure, Ostrom's argument also induces concern about the "direction" of its methodology, as well as that of its ideational infrastructure. The basic thrust is from assumptions about reality to prescriptions about how the real world should be. This directionality makes the public choice theorist more central than the testing of his theories in the real world and provides no defense at all against the human bias to see in reality what we are predisposed to see. Building complex ideational systems on assumptions about reality is hazardous, that is, especially in the absence of major motivation to test those assumptions early in the game. This motivation to test reality characterizes neither Ostrom nor the public choice literature, in the main. Indeed, one careful observer regards "the virtual absence of empirical concern as a major weakness in the collective choice literature."[14] Hence it is unclear how much public choice theory reflects the world or ideological concerns; it is moot as to what degree its theoretic agreements mirror reality or the life experiences, beliefs, or preferences of the theorists. The only certainty is that Ostrom takes no great pains to safeguard his theory against being an analytical Rorschach.

Of course, one may build a theory based on any assumptions that one fancies, for example, a theory of architecture based on the assumption that the only building materials are spaghetti and gum drops. And even very bizarre assumptions can prove useful. Whether that occurs, however, depends not so much on the assumptions as on how the derivative theory is used. One could regard such theory as hypotheses-to-be-tested, as some theorists of public choice are careful to do.[15] Such testing can do multiple duty: it might provide evidence as to whether the assumptions underlying the theory are more or less correct; it might suggest that the original assumptions are so simplistic as to generate theory that ill suits reality; and so on. Alternatively, one could treat such derivative theory as the basis for making action plans for intervening in reality or (worse still) for describing how things should be. This is a very dangerous business indeed. Ostrom's methodological approach encourages the latter treatment of theory as the-end-of-the-road rather than as hypotheses-to-be-tested.

Fourth, neither Ostrom nor public choice theory provides an acceptable place for values. Indeed, in concepts critical to the ideational structure, the value judgment is made that almost all value judgments cannot be made or should not be made. The purpose is to avoid making subjective, nonempirical judgments; but the approach also seeks to legitimate that avoidance via subjective, nonempirical judgments. That does not wash.

The present point has a paradoxical quality. That is, Ostrom's democratic administration is value loaded in critical senses, of which both writer and readers could hardly be unaware. And critics like De Gregori and Baker are quick to emphasize the metavalues of the public choice system: a "conservatism," a clear preference for those exercising economic power as contrasted with electoral or political power, a superior status for preferences reflected in markets compared to those expressed electorally or politically, and so on.[16]

When theoretical push comes to shove, however, the public choice approach variously declares a value judgment that is intended to exclude almost all other value judgments. Consider the Pareto criterion or optimum, which is subscribed to by many public choice theorists, and which proposes that a change is desirable if it makes at least one person better off in that person's own judgment while leaving not even a single person worse off in his or her own judgment. Proponents of the criterion argue that it has two basic attractions: it is ethically appropriate; and the decisions made consistent with it are potentially verifiable by empirical analysis alone. The underlying assumptions are those of logical positivism, which considers as scientifically meaningful only those statements whose truth or falsity can be demonstrated by empirical test, at least in principle. Superficially, the Pareto criterion nicely fits this requirement. As Merewitz and Sosnick explain, "If one adopts the Pareto criterion and then states that a change is desirable, the statement becomes equivalent to two other statements" which, at least in principle, can be empirically verified or rejected: that the change moves some persons to more preferred positions while disadvantaging no one; and that the person making the statement approves the change.[17]

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[13] Norton Long, *Journal of Politics*, 36 (August 1974), 804.

[14] Kenneth A. Shepsle, "Theories of Collective Choice," in *Political Science Annual*, Vol. 5, ed. Cornelius P. Cotter (Indianapolis, Ind.: Bobbs-Merrill, 1974), p. 4.

[15] For example, Niskanen, esp. p. 8.

[16] De Gregori, esp. pp. 207 and 217–19; and Baker, pp. 42–60.

This line of argument walks away from most or all issues that are morally or ethically troublesome or that are likely to be encountered in practice. To explain, the Pareto criterion in effect (and one supposes, by design) seeks to limit attention to these three values: that it is desirable to let people have what they want; that it is undesirable to impose on people anything that they do not want; and that it is legitimate to neglect all else save that which people want or do not want.[18] Most of the interesting issues in choice, and many of the significant ones, are thereby neglected. Hence what seems an unexceptionable criterion may have profound consequences, not all of them desirable for those unwilling to make the value judgment that almost all value judgments cannot or should not be made. Merewitz and Sosnick see "doubtful merit" in the position especially "if the people involved are ill-informed (as with hazardous products or complicated medical care), if the people are incompetent (as with youngsters, oldsters, and morons), or if they have lost self-control (as with addicts, drunks, and the bereaved). Moreover, what the affected persons want may not be everything that matters to the person passing judgment. He may be concerned about national interests, about accurate reporting, about due process of law, . . . and so on."[19]

The Pareto criterion gains internal consistency only at the cost of defining out of its domain many issues of practical moment, if not all of them. For example, it does not deal with the class of situations in which some individuals would be better off and some worse off if an action were taken. The putative reason for the exclusion is that such situations require some evaluation of the relative worth or value of what specifically is gained or lost, which evaluation is not possible on empirical grounds. Hence exclusion of a massive class of situations is the price paid for the convenient finesse of value issues, except for the three detailed above. In addition, the domain of the Pareto criterion is even further narrowed by the requirement that the affected persons and their reactions are known with certainty. The narrowing is especially severe in those cases where persons not yet born will be affected, or when the effects are probabilistic tendencies whose actual impact is dependent upon complex conditions which are themselves uncertain.[20] Patently, this means that the Pareto criterion will apply to very few issues of public policy. It conceptually avoids value judgments by practically avoiding most classes of significant choice.

Not all public choice theorists accept the full conceptual baggage of the Pareto criterion,[21] but Ostrom is not one of those who succeeds in resisting the allures of the extreme simplification of the central issue of values. There is a basic irony here, for Ostrom rejects the notion of any separation of politics from administration and urges that the latter be disciplined by the former. But he otherwise accepts an ideational network that would in effect remove from politics so much of its essence, that tumultuous clash of values inherent in the effort to define the role of the good citizen in the just state.

In Ostrom, the clearest evidence of the simplification of values is the insistent emphasis on consumer preference or utility as sovereign, as in the concept of "methodological invidualism" or "individualistic choice."[22] De Gregori apparently appreciates fully how "preference" or "utility" have become value surrogates in public choice theory, by way of such conceptual inventions as the Pareto criterion. Hence he hammers hard at the fact that Ostrom and the public choice theorists—by their insistence that only market decisions provide acceptable data and appropriate "freedom"—in effect consider one set of tastes or preferences as clearly superior to others such as those reflected in elections. De Gregori sees no theoretical justification for that superiority and, moreover, is troubled by the lack of associated specificity. He asks: "Freedom from what? Or freedom for what?"[23]

De Gregori insists on seeing the full array of values that provide specific content for generic terms like "tastes," "preferences," "freedom," and so on, that is, so priorities may be determined should tastes conflict. He is at once profoundly correct in this insistence, and yet it is irrelevant for public choice theorists. Their methodology and its derivative theory define what he insists upon as not relevant for purposes of analysis and—given some incaution—as nonexistent, period. Recall this central feature of the public choice argument: that almost all value judgments cannot or should not be made. As mentioned above, the three value judgments that can or should be made from the public choice position are: that it is desirable to let people have what they want; that it is undesirable to impose on people anything they do not want; and that it is legitimate to neglect all else save that which people want or do not want.

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[17] Leonard Merewitz and Steven H. Sosnick, *The Budget's New Clothes* (Chicago: Markham, 1971), pp. 79, 80.

[18] Ibid., p. 79n.

[19] Ibid., p. 79n.

[20] Ibid., p. 80n.

[21] Illustratively, the Kaldor-Hicks and Little criteria have been developed to loosen the conceptual limits of the Pareto criterion. See J. R. Hicks, "The Rehabilitation of Consumer's Surplus," *Review of Economic Studies*, 8 (1940–1941), 108–16.

[22] Ostrom, esp. pp. 60–64.

[23] De Gregori, p. 220.

Probably Improbable Assumptions. Concern with Ostrom's argument is especially appropriate because some of his central assumptions are at least probably improbable, which his methodology will fail to highlight.

The point is clear in Ostrom's central commitment to "methodological individualism." Essentially, Ostrom proposes a model of men which seeks to explain public policies in terms of individual rational decisions which act as an unseen hand guiding those market forces that determine the supply of public goods and the demand for them. This basic commitment, in turn, requires a host of definitions/assumptions about how men behave. Thus decisions are considered rational so long as they contribute to the attainment of specific goals or objectives, which are taken to be mutually consistent as well as transitive so that if people prefer decision A to C and B to C, they also will prefer A to C. In sum, the assumptions/definitions are that decision makers also will act consistently with: their own self-interest, rationality, an appropriate quality of available information, law and order, and the choice of a maximizing strategy, a least-cost way of achieving preferences.[24]

It is tempting to be picky with such necessary underpinnings of "methodological individualism." For example, what if the actual expressions of "self-interest" conflict with the assumption of "law and order"? In the "absence of any law and order assumption," Ostrom can only advise, "it might be necessary" to make another assumption as to "the prevailing human condition."[25]

The admission that it "might be necessary" to make other assumptions is a disarming but far too mild formulation, even though on the same grounds it might also be necessary to abandon all the other assumptions/definitions above. The prudent course would have been to make as certain as possible that different assumptions are *not* necessary because the ones used are adequate expressions of our existing knowledge of the world around us. The approach taken was apparently more convenient but also is more troublesome.

Apparent convenience and trouble seem a common pair, as the public choice theorists' common insistence on transitivity in individual decision making illustrates. Transitivity would exist in the case of voting, for example, if a voter who prefers alternative 1 to 2 and alternative 2 to 3, also prefers alternative 1 to 3.

This insistence on transitivity seems a prime example of how the assumed need to preserve ideational integrity can be multiply mischievous. The following three-part argument suggests that transitivity is too restrictive a central condition to build into a theory of choice. First, psychologists have not been able to establish that transitivity is characteristic of human behavior. Oppositely, Weinstein concludes that "most empirical tests of choice behavior appear to reflect intransitive preference patterns."[26] Second, the complex concurrent and multiple majorities characteristic of American politics and political parties do not encourage the posing of policy choices in ways that facilitate or even permit transitive choice.[27] Third, insistence on transitivity in voting typically would lead to consequences diametrically opposed to the values that public choice theorists publicly espouse, including the freedom of the individual decision maker.[28] We cannot here present the lengthy argument that establishes this irony, but we can conveniently quote Dahl on the central point. Dahl in 1956 noted: "Arrow shows that if there are more than two alternatives, any method for making social decisions that insures transitivity in the decisions must necessarily be either dictated by one person or imposed against the preferences of every individual."[29] The last two decades have seen an extensive literature develop around the "voting paradox" but, despite qualifications of subtlety and issues in doubt, Arrow's conclusion still seems more appropriate than not.[30]

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[24] Ostrom, pp. 50–51.

[25] Ibid., p. 52.

[26] Arnold A. Weinstein, "Individual Preference Intransitivity," *Southern Economic Journal*, 34 (January 1968), 335.

For an example of the empirical literature consult Amos Tversky, "Intransitivity of Preferences," *Psychological Review*, 76, No. 1 (1969), 31–48. Tversky begins to specify the empirical conditions under which consistent and predictable intransitivities occur. Those conditions seem to be quite prevalent, which provides but little support for the ideation and argument underlying democratic administration.

[27] Robert A. Dahl, *A Preface to Democratic Theory* (Chicago: University of Chicago Press, 1956), esp. pp. 34–50.

[28] Kenneth J. Arrow, *Social Choice and Individual Values* (New York: Wiley, 1951), pp. 51–59 and 75–80.

The prudent implication is caution about accepting any derivations from such public choice assumptions, an implication reinforced by considering one central aspect of "methodological individualism." Does it intend to deny the independent existence of supraindividual collectivities like "groups"? The answer is, mostly, yes. But some public choice theorists seem to want to have it both ways, as do Buchanan and Tullock in announcing: "Throughout our analysis the word 'group' could be substituted for the word 'individual' without significantly affecting the results."[31]

This denial of the need for multilevel analysis is clearly convenient, but it almost certainly means a major reduction in the explanatory power of any derivative theory. Whether the answer is a straightforward denial of supraindividual collectivities, or pained efforts such as that above to suggest some kind of equivalence between "individual" and "group," reality is too recalcitrant to fit such economizing formulations. To be sure, only individuals can perceive and make decisions. But what of the common experience that the same individuals typically behave in radically different ways following deliberate manipulations of the "climates" of the successive groups to which they belong?[32] Such effects clearly require a supraindividual level of description and explanation. Or better still, what of groups in which all or many members come to decisions which they personally do not prefer, or even abhor, on profoundly serious[33] as well as trivial[34] matters? The alternative to denying multiple levels of social organization is to presume the irrationality of mankind, or to postulate their "herd" or "crowd" quality, which names or vilifies but does not explain.

What does this catalog imply? Clearly, public choice theorists are not sensitive to such dilemmas in their ideation. Clearly, also, Ostrom would like to increase the role of the individual decision maker in public policy choices. All well and good, and many of us applaud that intent. And there is no question but that assumptions like "methodological individualism" provide conceptual support for such an intent. But it appears that too much is made of a convenient thing. Ostrom not only articulates a "should" prescribing an increased role for individual decision makers (which is proper enough), but he also accepts a theoretical infrastructure which conveniently recognizes only separate individuals and thus flies in the face of long-standing experience. As De Gregori concludes, if any theorists assume "that large numbers of people do not consider themselves members of economic and social classes [and consequently that they do not] act politically in these terms (that is collectively), [those theorists] have a sizable body of data to refute."[35] Methodological sleight-of-hand does not change that stubborn fact. The neglect also may imply serious misdirection if, as many argue, the individual needs to define self largely in social interaction, and if freedom is possible only in a special kind of society. Emphasis on the separate individual will in such cases result in not asking many of the appropriate questions.

Critical Gaps in Content and Meaning. Beyond relying on improbable assumptions, Ostrom's position and its underlying public choice theory also lack critical content or meaning. Indeed, this puts the point mildly, as is manifest in the unsatisfying treatment of self-interest and efficiency in Ostrom's formulation, as well as in public choice theory in general.

Whose Self-Interest about What? Public choice theory waffles badly on the issue of self-interest, and consequently finds itself between the analytical rock and a hard place. In an assumed attempt to be comprehensive, it ends up saying little clearly or consistently. De

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[29] Dahl, p. 42n.

[30] Shepsle, esp. pp. 31–42, provides ample documentation of this summary statement.

[31] Buchanan and Tullock, p. 9.

[32] This was the case, for example, in the famous 1930 studies of group atmospheres or climates. See especially Ronald Lippitt, "Field Theory and Experiment in Social Psychology: Autocratic and Democratic Group Atmospheres," *American Journal of Sociology,* 45 (July 1939), 26–49.

[33] Irving Janis, *Victims of Groupthink* (Boston: Houghton Mifflin, 1972), deals with a number of major political and military decisions which fit this characterization.

[34] The classic experimental demonstration is that of Solomon E. Asch, "Effects of Group Pressure Upon the Modification and Distortion of Judgments," in *Groups, Leadership, and Men,* ed. Harold Guetzkow (Pittsburgh, Pa.: Carnegie Press, 1951), pp. 177–90.

[35] De Gregori, p. 210.

Gregori's conclusion is harsher still. Referring to two central figures in public choice theory, he concludes that their writings about "self-interest contain neither a concept of self nor a concept of interest, and we are left with a logical or mechanical construct without content."[36]

The basic point is easy enough to document, whatever the harshness of the overall evaluation. One thing that should be patent about "methodological individuals" is that they operate in terms of their self-interest narrowly defined. However, significant public choice theorists apparently recognize that the narrow definition of self-interest is inadequate for describing reality. In any case, some theorists take care to emphasize that their "methodological individuals" have a broad-band self-interest. Their motivation is not only "narrowly hedonistic," but it can also be (and often is) "egoistic or altruistic."[37]

Ostrom accepts the broad-band notion of self-interest[38] and, however motivated, that acceptance creates far greater theoretical problems than it solves. Indeed, the irony may be that there is no solution, only a dilemma. Perhaps De Gregori is correct in concluding that self-interest as a postulate is either false in its narrow form or trivial as a broad-band notion.

Both falsity and triviality can be established by considering two problems for public choice theory implied by broad-band self-interest. First, the "what" of self-interest is no longer clear, and the predictive power of public choice theory thereby suffers. De Gregori nicely makes the point by observing that, if individuals are assumed to be profit maximizers, it is then reasonable to talk "about a form of behavior in which individuals seek to maximize an empirically definable function that is called economic gain." But this advantage vanishes in broad-band self-interest. De Gregori makes the contrast vividly. "Call the motivating principle 'utility' or 'ego' or whatever, and one has a principle that can be defined only in terms of the ensuing action. . . . It does not explain or predict the action, it merely names it."[39]

The same point, indeed, was apparently clear to Buchanan and Tullock in one of the early and seminal books on public choice. Broad-band self-interest, they recognize, permits an economic assumption of only the "barest essentials." To wit: "The economic assumption is simply that the representative or the average individual, when confronted with real choice in exchange, will choose 'more' rather than 'less'."[40] This is neither a powerful assumption nor a comforting one, in light of the authors' single quotation marks around what are fundamental concepts in their system of thought. The assumption permits only such gentle a posteriori statements as the following: "When I observed an economic choice, the choice was motivated by the chooser's judgment that it provided more of something, but more of what and how much more I cannot now say." Because this statement can never be tested and hence cannot be shown to be false, it provides a delicate foundation indeed for theoretical development.

Second, broad-band self-interest obfuscates another issue that is crystal clear in narrow concepts, since *whose* self-interest becomes a major unknown rather than a given. To explain: The "separate individual's" self-interest is stage center in narrow concepts. In contrast, if altruism (or whatever) is allowed as a motive, the issue of whose self-interest is involved takes on complex proportions. The conceptual door is also opened to various social or collective processes, including group formation, through which individuals can act in terms of the best interests of others as well as their own. Some public choice theorists follow Sen in this regard, who explains that

> the society in which a person lives, the class to which he belongs, the relation that he has with the social and economic structure of the community, are relevant to a person's choice not merely because they affect the nature of his personal interests but also because they influence his value system including his notion of "due" concern for other members of society. The insular economic man pursuing his self-interest to the exclusion of all other considerations may represent an assumption that pervades much of traditional economics, but it is not a particularly useful model for understanding problems of social choice.[41]

Other theorists—including Ostrom—avoid such an admission, presumably because it undercuts the "methodological individualism" so central in public choice theory. Perhaps that is why Buchanan and Tullock go as far as—but no further than—noting that the word "group" in their analysis can be used interchangeably with the word "individual" without significantly affecting their results.[42]

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[36] Ibid., p. 209.

[37] Buchanan and Tullock, pp. 3, 17.

[38] Robert L. Bish and Vincent Ostrom, *Understanding Urban Government* (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1973), p. 18.

[39] De Gregori, p. 209.

[40] Buchanan and Tullock, p. 18.

[41] Amartya K. Sen, *Collective Choice and Social Welfare* (San Francisco: Holden Day, 1971), p. 6.

Efficiency for What Purpose? The gaps in meaning or content in Ostrom's approach are especially prominent in the second criterion in Table 1—efficiency—for determining the scale of public organizations consistent with democratic administration. The deficiencies are conveniently expressed in the context of critical questions with which Ostrom does not deal, a question, for example, such as: Efficiency for what purpose?

Ostrom's assignments of meaning to "efficiency" seem general and unsatisfying. At one point, for example, he notes that consumer preferences are central, perhaps even exclusive, in providing the meaning of efficiency. His proposition (which he italicizes) is: "*Producer efficiency in the absence of consumer utility is without economic meaning.*"[43] This is not analytically satisfying, however. Clearly, it runs afoul of the several considerations raised earlier in connection with the broad-band concept of self-interest. Ostrom's dictum about economic meaning has no tolerably precise meaning.

Ostrom's failure to deal effectively with the values or goals defining efficiency also impacts significantly on his broader argument, as illustrated by the lack of content or meaning in his criteria sketched in Table 1. Recall Ostrom prescribes (in part) that: "The most efficient solution would require the modification of boundary conditions so as to assure a producer of public goods and services the most favorable economy of scale, as well as effective control."[44]

The prescription is vague in the absence of an unambiguous answer to: Efficiency for what purposes? During World War II, for example, it was common that the most monopolistic industries were "most efficient" in a number of then-relevant senses. Thus it was easier for government officials to deal with Alcoa than with the multitudinous producers of feeds and grains, for more or less obvious reasons. Of course, Alcoa also was in a position to be more troublesome if it so chose, but wartime conditions were well-suited to concentrated control and planning of aluminum production, given acceptable combinations of patriotism and whatever. Because short-run control was facilitated, the answer to how big Alcoa ahould be was: the bigger the better, and the greater its share of the market the better. In the immediate post-war period, different considerations became prominent in public policy, and different answers consequently were appropriate. Both before and during the war, however, the central question was the same: Efficiency for what purposes?

The emptiness of the phrase "the most favorable economy of scale" also can be demonstrated in less obvious ways. Consider Ostrom's contrast of two ways of viewing efficiency: "One way views efficiency as being expressed through principles of hierarchical organization," by which he refers to concepts such as "unity of command, span of control, chain of command, departmentalization by major functions, and direction by single heads of authority in subordinate units." Ostrom notes with skepticism that these principles are alleged to be universally applicable, and that this first way of viewing efficiency is direct and uncomplicated. "The greater the degree of specialization, professionalization, and linear organization in a unitary chain of command, the greater the efficiency," Ostrom correctly states the essential implication of the extreme version of the traditional paradigm he chooses to emphasize. The bigger the unitary organization, in short, the better. Ostrom adds by way of contrast: "The other way views efficiency in terms of a cost calculus," which Ostrom asserts will permit the "accomplishment of a specifiable objective at least cost; or a higher level of performance at a given cost."[45]

Ostrom's sentiments clearly rest with the "cost calculus," and his suspicions about efficiency through hierarchical organization are profound. But the argumentation supporting that preference is only briefly developed, and it is not convincing for several reasons. First, there *is no* available "cost calculus" of the kind required. Even the traditional tools and approaches for financial analysis imply serious issues and deficiencies.[46] Worse still are the problems of taking into account a broader range of considerations that are typically neglected in managerial accounting and financial reports, factors such as the motivation or morale of the work force, the long-run vs. the short-run consequences of managerial actions, etc. Perhaps the most that can be said here is that such work is only a little way down the track.[47] Most chronic by far are the problems with "social accounting," which seeks to relate organizational costs to broad goals or ends.[48]

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[42] Buchanan and Tullock, p. 9.

[43] Ostrom, p. 62.

[44] Ostrom, Tiebout, and Warren, pp. 835–37.

[45] Ostrom, pp. 35, 48.

[46] Abraham J. Briloff, *Unaccountable Accounting* (New York: Harper, 1972).

Ostrom's treatment of the "cost calculus" re organization scale does some violence to central notions of public choice theory, moreover, and seems at odds with parts of his own argument. Consistent with Ostrom's emphasis on markets, "methodological individualism," and so on, it would have been seemly to argue that the scale of public organizations on the supply side is, or should be, a function of the same market forces that public choice theorists seek to energize/legitimate on the demand side. In contrast, he introduces the "cost calculus," which implies a cadre of experts, with data far beyond that available to his individual choice makers, and ostensibly with some power or authority to influence the scale of public organizations. Suddenly, Ostrom finds himself squarely facing the kind of problem that PPBS enthusiasts had promised to solve, those very centralizing, rationalist, elitist proponents from whom Ostrom otherwise seeks to distance his argument.[49] Ostrom is not successful at transcending their failure. Indeed, he basically promises a "cost calculus" rather than provides one. Ostrom is in the same boat on this issue as the conceptual enemy. The conclusions applying to PPBS efforts thus apply to Ostrom in this regard. Consider their hope to justify appropriations or expenditures for public programs. Merewitz and Sosnick quash that hope by recalling the harsh and unavoidable realities. "To *justify* an appropriation," they observe, "it is necessary to show that the benefits to be obtained outweigh the benefits that could be obtained by spending the same amount of money for other purposes or by cutting taxes. *And this cannot be shown.*" They conclude: "No procedure is available that will show whether expenditures are or are not justified. The answer is a matter of subjective judgment."[50]

Second, as the reference to social accounting implies and as the excerpt from Merewitz and Sosnick explicitly notes, there *can be no* "cost calculus" for determining the appropriate scale of any organization until an answer to a key question is reasonably in hand. That neglected key question: Efficiency for what purpose?

Third, Ostrom's juxtaposition of hierarchical perfection and cost calculus constitutes a straw man. That is to say, for nearly the preceding half-century, the "principles of organization" have been attacked with great gusto, and even some effect.[51] Although "contingency theorists" will argue that the principles describe the appropriate form of organization under certain technological and product-line conditions, the definite drift of research and commentary argues the appropriateness of organizing guidelines that differ significantly from the "principles." These evolving guidelines were long ago sketched in the public administration literature and also have received fulsome attention in the literatures of generic and business administration.[52] The most common citations in Ostrom predate this work by a decade and usually far longer, with most attention going to the central *Papers on the Science of Administration* (1937) by Gulick and Urwick and to Simon's powerful but early (1946) critique of the "principles" literature.

There is no point in detailing here these emerging guidelines for organizing work, except to note that they constitute an alternative to the "cost calculus" which Ostrom proposes will decide the scale of organizations but on which no one can deliver.

Simultaneously Opposed Courses of Action. Still more troublesome, Ostrom's prescriptions often have the unhappy feature of recommending two or more courses of action that are potentially antithetical. Consider Table 1, in which the scale of public organizations is to be determined, *inter alia,* by the criteria of control and the "democratic ideal of local self-determination." There are two basic difficulties. First, Ostrom provides little or no guidance as to appropriate decisions when choices between the criteria must be made. As Baker asks:

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[47] Consult William J. Burns, Jr., and Don T. De Coster, eds., *Accounting and Its Behavioral Implications* (New York: McGraw-Hill, 1969); and Thomas Burns, ed., *Behavioral Experiments in Accounting* (Columbus, Ohio: College of Administrative Sciences, Ohio State University, 1972).

[48] Bertram M. Gross, *The Managing of Organizations*, Vol. 2 (London: Free Press of Glencoe, 1964), esp. pp. 467–501; and Meinoff Dierkes and Raymond A. Bauer, eds., *Corporate Social Accounting* (New York: Praeger, 1973).

[49] Ostrom, esp. pp. 49–50.

[50] Merewitz and Sosnick, p. 63; emphasis added.

[51] Francis W. Coker, "Dogmas of Administrative Reform," *American Political Science Review*, 16 (August 1922), 399–411; Charles S. Hyneman, "Administrative Reorganization: An Adventure into Science and Theology," *Journal of Politics*, 1 (February 1939), 62–74; Earl H. Latham, "Hierarchy and Hieratics," *Employment Forum*, 2 (April 1947), 1–6; and Herbert A. Simon, "Proverbs of Administration," *Public Administration Review*, 6 (Winter 1946), 53–67.

[52] Robert T. Golembiewski, *Men, Management, and Morality* (New York: McGraw-Hill, 1965); Alfred D. Chandler, Jr., *Strategy and Structure* (Cambridge, Mass.: The M.I.T. Press, 1962); and E. Raymond Corey and Steven H. Starr, *Organization Strategy: A Marketing Approach* (Boston, Mass.: Graduate School of Business Administration, Harvard University, 1971).

> How does one account for the scale of a public enterprise which, in the process of "internalizing" the consequences of producing or providing a public good or service under the criterion of control, exceeds the boundary conditions necessary to achieve optimal economies of scale requisite under the criterion of efficiency? The boundary conditions required for regulation purposes may not coincide with the boundary conditions necessary for obtaining optimal economies of scale. Or in attempting to meet the requisites of the criterion of political representation, how does one *make* coterminous the scale of formal public organization, the public affected, and the political community?[53]

Since these choices always must be made, and since they are often fiendishly complex ones in the bargain, it is not useful to prescribe a decision rule which is mute about them.

Second, it is not clear how a kind of runaway-train effect can be avoided or stopped once it starts. If "control" takes precedence over the other criteria in Table 1, as it well might, there are no obvious limits on the scale of a public agency. The need to prescribe levels of organization compounds this problem. Assume, for example, that it is possible to define some optimum size for a federal land reclamation project in Arizona, using "cost calculus" or whatever. Still open are questions of the size of the regional suborganization of which that project may be a part, of the size of the federal cabinet-level organization of which that region may be a part, and so on. What is clearly "local" to one of these levels of aggregation may be cosmopolitan to some, and yet impossibly provincial to still others. In such a case, moreover, "control" may take on more compelling qualities than "local self-determination." This will especially be the case if one of the "economies of scale" is the ability to compete successfully for funds at the congressional level where, all things considered, being "bigger" may be useful whatever the scale of enterprise consistent with "local self-determination." In addition to monitoring or even coercing lower cadres, adding overhead levels also is motivated by many other factors, including: broader perspective; taking the heat attendant to a decision that should be made but which local officials fear making because it violates some powerful interests or figures; and so on through a very long list.

Some Awkward Probable Consequences. Worse still, Ostrom's approach and that of the public policy theorists can have major consequences opposite those intended. Four main points will help illustrate the broader family of unanticipated consequences:

1. that decentralized government is not necessarily better, more democratic, more moral;
2. that the case for smaller organizations being more efficient and responsive is far from clear;
3. that conditions of resource scarcity or resource affluence are critical in conditioning the choice of a shifting balance of government interventionism or of centralization/decentralization;
4. that what may be called "cycles of governance" can be distinguished as cycles to which public policy must be sensitive.

These examples of unexpected and unintended consequences imply that the ideational infrastructure of public choice is not yet ready to provide a framework for public policy.

Is Decentralization Better, More Democratic, More Moral? One nativist thrust of the public choice position is that "decentralization" is preferable to a unitary hierarchy responsible to one man. The logic underlying this basic bias of public choice theory seems clear enough. Individuals possess only "bounded rationality," which means no one decision maker can pretend to a broad knowledge of utility preferences needed to achieve maximum efficiency. Hence the preference of public choice theorists for decentralized price or market systems, because they imply fewer demands on the decision maker by reducing the scale of the system of choices to be coped with.

There is no objection to this emphasis in public choice theory, if it is not pushed overly far. Indeed, the management literature had come to a similar conclusion long before, beginning at least in the 1920s with such powerful statements as Mary Parker Follett's "The Allusion of Final Authority." Significantly, moreover, Follett's central contribution was reprinted in that seminal collection by Gulick and Urwick, *Papers on the Science of Administration* (1937). It is true, however, that the impact of that conclusion about executive overload had been only partially felt in public administration or political science, where the "imperial presidency" was reaching new heights.

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[53] Baker, pp. 51–52.

Public choice theorists are not restrained in this regard, however, and neither is Ostrom's political theory. "Taste" or "preference" is the basic value surrogate in public choice theory; and decentralization of government plays much the same role in Ostrom's thought. Thus public choice theory avoids this question: Whose preferences about what? And Ostrom usually neglects a similar question: What degrees of decentralization, when, for which purposes? To be fair, there are points where Ostrom argues that a "variety of different organizational arrangements can be used to provide different public goods and services."[54] But Ostrom is on balance not very interested in that "variety."

Decentralization is far from simple, but some brief and meaningful things may be said about it here. Primarily, public choice theorists see centralization and decentralization as opposite extremes of a continuum. More useful is a tripartite notion, such as that shown in Figure 1. Public choice theorists generally, and Ostrom specifically, neglect this distinction. One danger is that their strong preference for decentralization may result in chaotic localism which, in turn, may only make more probable the very centralization which public choice theorists say they abhor. Indeed, the extreme outcome could be a Hobbesian "war of all against all," to which centralization would be among the more benign responses. Given chaotic localism, to explain, effective powers are lodged in small units of government wherein local minorities may be poorly treated, policies can vary enormously between local constituencies, and so on. Such a consequence of championing decentralization without reference to the values to be served thereby, for example, occurred when race relations were considered a local matter. There are two probable outcomes in such cases: the need deprivation will persist over long periods of time; and superordinate units of government will centralize decision making of the issue in question, all the more decisively the longer the status quo persists. Neither outcome is desired by public choice theorists, presumably.

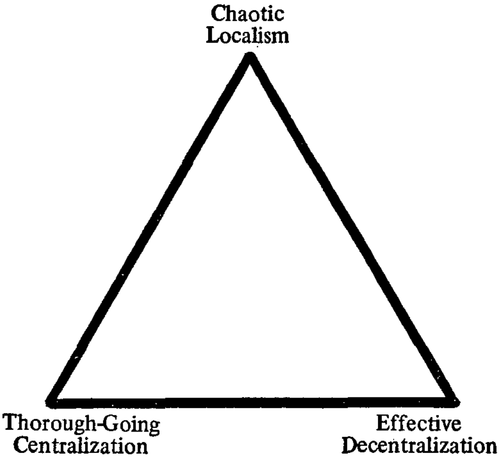

Figure 1. Schema of Forms of Delegation

Neither Ostrom's nor the public choice logic seems to permit any way out. To illustrate, the distinction between the two brands of decentralization—that is, "chaotic localism" and "effective decentralization"—clearly rests on value judgments. However, public choice theory overtly seeks to avoid most value judgments. Hence, by implication, the failure to distinguish between varieties of decentralization rests on the failure to specify values. This may similarly explain the failure to stress that centralization at times might serve important values.

This elemental sketch illustrates the conceptual difficulties about which careful observers like Fesler sought to warn those zealously committed to decentralization as a doctrinal summum bonum. Their warning stresses three points. First, the preference for centralization/decentralization cannot be a matter of either/or, but should be a dynamic, evolving, and oscillating process of fine-tuning institutions to current problems. Failure to recognize the point implies only trouble, if not an Alice-in-Wonderland state where even benign intentions yield unwanted effects. As Friedrich notes: "This continual change is a matter of fact; but unless it is clearly recognized and institutionally provided for, all decentralization of power . . . [is] apt to become [a] source of tension and conflict."[55]

Second, it is specious to link democracy, freedom, and decentralization, as Ostrom does[56] in common with much of the public choice literature. This easy marriage neglects a reality that often has been painfully obvious. As Hart notes, "Democratic processes do not necessarily result from, nor are democratic ideals necessarily maximized by, decentralization. In fact, the opposite has often occurred and independent local political structures have worked against democratic ideas."[57] Moreover, the linkage of decentralization with democracy and freedom equates values with means and also creates major analytical and practical difficulties. We cannot improve on Fesler's formulation: "In extreme principle [such a conceptual linkage] can mean anarchy; and in muddled practice it can be so closely associated with islands of group or community self-determination that subordination of the individual to a local autocracy may be lost from view, as can the potentially liberating influence of a national leadership committed to development of social and economic opportunities for individual achievement of personal goals."[58]

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[54] Ostrom, p. 112.

[55] Carl J. Friedrich, *Man and His Government* (New York: McGraw-Hill, 1963), p. 667.

[56] Ostrom, esp. pp. 75–81.

Third, the doctrinal approach to decentralization neglects the critical role of the central government in facilitating movement toward those goals which decentralization is intended to achieve. Briefly, decentralization implies a viable definition of the value parameters within which local governments can act without becoming so balkanized that the necessary context of unity about fundamentals is destroyed, that a "chaotic localism" comes to be and persists. Without such a "viable definition of value parameters," decentralization could equally serve revolutionary as well as reactionary politics, with the potential *in extremis* for polarization such as that which led to the Civil War. In Fesler's more restrained and detailed language:

> National legislation, overriding local objections and implemented by national administrative action, is often required to democratize the selection of local officials, to establish viable units of local government with the size, resources, and diversity of interests that are preconditions of effective local self-government, to recruit and train skilled staff for local administration, to minimize corruption and regularize fiscal practices, and to provide grants from national revenue to help finance the impoverished communities.[59]

Patently, these interventions by the central government are not consistent with a doctrinal vision of the decentralized polity, even if their motivation (as Fesler notes) is "often to save local self-government from itself."

Are Smaller Public Organizations More Effective and More Responsive? Perhaps the most dominant emphasis in Ostrom's thought is that "smaller government" will bring more effective and responsive provision of services, and Ostrom thereby sets himself a substantial challenge. To be sure, every age seems to have its retrospective vision of an uncomplicated and pleasing "prior condition," a kind of nice sociopolitical predecessor of extant nastinesses and nuisances. Thus there was the Garden of Eden and Marx's vision of preindustrial society; more recently, the vision of a nation of "family farms" sustained many; and there is an important theme of back-to-nature in some of today's communal experimentation.

Such nativist traditions notwithstanding, the vast bulk of the literature gives small scale government no clear edge in being better,[60] more democratic,[61] or more moral,[62] (given a gentle fuzziness about what "small scale" really means); quite the opposite, in fact.

Let us begin with a debator's point or two. For one thing, Ostrom is never clear on what "small" is. Nor does it do for him to praise "smaller" more than "larger," thereby seeming to avoid the issue. For once any organization gets larger than one in which cozy face-to-face interaction is both possible and convenient, major differences in communication and influence patterns quickly develop.[63] Severe problems also relate to Ostrom's very general position that the appropriate scale will vary "with the boundary condition of *different* fields of effects inherent in the provision of *different* public goods and services."[64] The word "inherent" rankles. Only complex mixtures of empirical conditions and value positions will define what is an "optimal scale." There is no more an inherent optimal scale for a public agency than there is an inherent optimal family size independent of complex empirical conditions and especially value preferences. It all depends—within very wide limits.

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[57] David K. Hart, "Theories of Government Related to Decentralization and Citizen Participation," *Public Administration Review,* 32 (October 1972), 606.

[58] James W. Fesler, "Approaches to the Understanding of Decentralization," *Journal of Politics,* 27 (August 1965), 549. For a balanced view deriving from extensive empirical research by one of Fesler's students, see Douglas Yates, *Neighborhood Democracy* (Lexington, Mass.: Lexington Books, 1973).

[59] Ibid., p. 549.

[60] Herbert Kaufman, *The Limits of Organizational Change* (University, Ala.: University of Alabama Press, 1971), pp. 101 ff.

[61] Fesler, esp. pp. 539–46.

[62] George Graham, *Morality in American Politics* (New York: Random House, 1952), esp. pp. 42–53.

[63] Arnold S. Tannenbaum et al., *Hierarchy in Organizations* (San Francisco: Jossey-Bass, 1974).

[64] Ostrom, p. 118; emphasis in original.

But let us put aside such initial if basic concerns. What is Ostrom's argument for small (or smaller) public units? The preference in public choice theory and Ostrom for smaller units is offered almost as a matter of fact, a given beyond any serious question or qualification. Or if that is too strong, the arguments of public choice theorists like Ostrom are far from convincing, and the available empirical literature provides (at best) mixed support.[65] In the main, Ostrom refers to three basic public service areas—education, police, and fire—and provides some data about the last two in seeking to establish that smaller is better. Reviewing his evidence will be instructive.

Big city education is in a turmoil, Ostrom notes, which he attributes mostly to large and centralized school systems which are unresponsive to citizen preferences or tastes. This attribution is minimally buttressed, with an unseemly neglect of factors such as race, dislocated families, decaying center cities, plummeting tax bases and escalating public services, and so on. Not mentioned, also, is the fact that a common rationale for larger school systems rests on the perceived failure of smaller systems to do the required job.

Ostrom's contention that smaller is better in big city education is coupled with a straightforward remedy, but not a convincing or even an entirely consistent one. As for the remedy, Bish and Ostrom note that the "big city school systems are too large, and they should be broken up into smaller systems serving children in more homogeneous neighborhoods." Decentralized school districts, they conclude, "would give center city residents control over their own schools such as that presently enjoyed by suburban residents."[66] The remedy has some troublesome features, however. For example, research suggests that voter turnout is lowest in the smallest jurisdictions,[67] hence smaller scale may generate effects counter to those Ostrom hopes for and expects. This contrary consequence is no doubt due to many factors, including the media's regional and national focus.

Decentralization of schools—whether by enhanced delegation of authority within a system or by making several smaller systems out of one larger one, a distinction Ostrom does not make consistently—does not clearly affect such factors as media biases or rates of participation in campaigns. And such factors need direct confrontation by those presenting decentralization as a more or less unqualified good. Moreover, Ostrom's remedy is basically inconsistent with the massive Coleman report which—despite its own internal limitations—convincingly and persistently traces educational outcomes to the quality of the home and of the immediate environment of students.[68] Finally, Ostrom does not follow his own advice in some particulars, at the cost of possible inconsistency. "Accompanying decentralization would be expanded area-wide or state-wide financing," Bish and Ostrom note, "so that poorer districts would be able to provide a greater level of support for education. Wealthier districts would be free to tax at higher rates and provide more educational inputs above that general level."[69] Bish and Ostrom do not deal with possible inconsistencies arising between area-wide or state-wide financing and greater local control over schools.

Ostrom's case against centralized police service is similar,[70] but is presented with more supporting data. Three empirical studies are referred to, and their results indicate that services of larger police departments (in the small sample used) cost more per capita,[71] and that residents of small, independent communities seem more satisfied with the service their police departments provide.[72] These empirical efforts are to be applauded and encouraged, but they do not yield unequivocal interpretation. The central problem is that the underlying cost/benefit analysis is quite incomplete. Larger police departments studied did cost more per capita in two of the three cases, but no one knows whether or not they yielded commensurate benefits. It does seem that residents or larger jurisdictions have less favorable attitudes toward their police but, as important as this measure may be, it clearly falls far short of a complete measure of benefits. Calculating these benefits would be very difficult, but that alone can permit specific comparisons of the kind required. Moreover, it is anybody's guess as to how stable such attitudes are, how deeply they are rooted in experience and reliable information, and to what degree individual judgments of police efficiency/effectiveness are made in terms of values or goals that relate to the full range of police work. Such differences should be important in political debate and dialogue.

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[65] For example, Harry P. Pachon and Nicholas P. Lovrich, "The Consolidation of Urban Public Services," *Public Administration Review,* 37 (January 1977), 38–47, provide evidence which contradicts Ostrom's position with respect to large vs. small police systems. Such studies reinforce the notion that any relevant comparisons between large vs. small public agencies are likely to be very complicated, contingent on intervening variables, and so on. This certainly has been the case with industrial and commercial organizations. For example, substantial evidence implies that total organization size is likely to be less salient than the size of component "managerial units," a managerial unit being defined as that portion of an agency monitored by some authoritative agent who can make reasonable decisions about some total "flow of work." For the conceptual argument and a review of substantial evidence, see Golembiewski, *Men, Management, and Morality,* pp. 113–15, 133–38, 144–47, 234–43, 268–71, and 279–83.

Size of managerial unit is *not* dependent on size of organization, but will vary with the degree of decentralized delegation in an organization. A decentralized organization, however, could be huge in total size.

[66] Bish and Ostrom, p. 38. See also pp. 36–37.

[67] James A. Reidel, "Citizen Participation: Myths and Reality," *Public Administration Review,* 32 (May 1972), 211–20.

[68] James S. Coleman et al., *Equality of Educational Opportunity* (Washington, D.C.: National Center for Educational Statistics, 1966).

[69] Bish and Ostrom, p. 38.

[70] Ibid., pp. 41–46.

[71] Elinor Ostrom et al., *Community Organization and the Provision of Police Services* (Beverly Hills,

The evidence about fire services is limited to one study, which supports a conclusion opposite the one advertised: smaller is not clearly better. To explain, Bish and Ostrom cite Ahlbrandt's study[73] of fire protection in Scottsdale, Arizona. Ahlbrandt provides important evidence supporting the efficacy of unorthodox arrangements, for Scottsdale's fire services are provided by a business firm which seems to do an innovative and economic job when its costs are compared to reasonable estimates of what the costs would have been if the service were provided by a traditional fire department. Ostrom proposes that this is due to the fact that the firm can be unbuckled by Scottsdale if it does poorly, a pressure for performance not felt so directly by a traditional fire department. Perhaps this is so, although one could imagine alternative scenarios in other similar cases. Sweetheart arrangements between a firm and local politicians are possible, for example. Such possibilities aside, moreover, note that at least part of the firm's advantage comes from the fact that it serves not only Sclttsdale, but "several other cities and unincorporated areas in Arizona."[74] Smaller is not necessarily better, that is to say, and it is incautious to compare the firm's costs with those of a traditional fire department restricted to Scottsdale. Moreover, one could argue that the fire service was efficient precisely because it followed traditional principles in the sense of serving multiple cities, even as Scottsdale chose not to respect the principles by opting against including a municipal fire-fighting service in its organizational structure.

Indeed, when all is said and done, large scale is not the only (or perhaps even the central) issue for Ostrom. Thus in big city education he himself identifies three "causes of problems . . . professionalization, large scale, and uniformity."[75] At other points, he observes that the "combination of professionalization and unionization has [made] organized teachers . . . relatively invulnerable to the directives of elected officials."[76]

There is a wicked probable paradox in all this: that the emphasis on smaller scale not only may fail to achieve Ostrom's goals, but that it also may be counterproductive in his own terms. The point is meant in several senses. Even if we grant—though it is neither obvious nor necessarily true—that the development of narrow professionalization, unionization, and uniformity are causally related to size of school systems, it is doubtful whether those conditions would disappear if the size of (let us say) school systems were reduced. Will the scale or policies of professional associations or unions, especially at state or national levels, be very much affected by such reductions? Not likely. The more probable consequence, in fact, is a substantial increase in the power of professional associations, unions, and government bureaucracies vis-à-vis the elected or appointed heads of the now smaller school systems. This is precisely opposite Ostrom's stated intentions, of course. Similarly, it is not clear that smaller scale implies less "uniformity," especially if by that term Ostrom excludes nonuniformity of the chaotic localism variety. Indeed, some evidence suggests that a substantial take-off size is required for many diversified educational

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Calif.: Sage Publications, 1973); and Elinor Ostrom and Gordon Whitaker, "Black Citizens and the Police" (paper prepared for the Annual Meeting of the American Political Science Association, Chicago, 1971).

[72] S. T. IsHak, "Consumers' Perceptions of Police Performance: Consolidation vs. Deconcentration" (Ph.D. Dissertation, Indiana University, 1972).

[73] Roger S. Ahlbrandt, Jr., *Municipal Fire Protection Services* (Beverly Hills, Calif.: Sage Publications, 1973).

[74] Bish and Ostrom, p. 47.

[75] Ibid., pp. 40–41.

[76] Ibid., p. 36.

programs,[77] such as those for exceptional children, for organization development in schools, for various forms of experimentation with curricula, forms of governance, and so on. Finally, "uniformity" need not always be an evil to be avoided, as in determined consensus about the pursuit of high quality educational outcomes.

The rigorous comparison of larger vs. smaller governmental units is extremely difficult, so it is understandable that Ostrom provides no convincing comparisons. But it is inadequate to argue, as Ostrom basically does, that smaller is preferable to larger in governmental units because, *inter alia,* the latter lack a market with its "competitive force," thus are less likely than businesses to be aware of "diseconomies of scale," and hence will grow large rather than wisely or well.[78] That may be so, although some would argue that the business of government is different enough to preclude strict comparison. But it would have been useful to do more than proclaim the point, as in explaining why an organization like AT&T—which lacks a market in Ostrom's sense—is not only mammoth but effective as well as innovative. Much might be learned from such obvious cases. Or perhaps Ostrom could have explained the substantial differences in the effectiveness of similar-sized units within federal agencies like the Social Security Administration, since neither those units nor SSA possess markets. Such analyses would be critical in developing a full-fledged theory of the covariants of the effectiveness of public organizations, which is what we need.

In contrast to recommending such complex and demanding questions for analysis, Ostrom basically presumes what is at best a very complicated proof, and without acknowledging that "business effectiveness" does not necessarily equal "government effectiveness." And he is content with such general statements as "opportunities for improvements in efficiency through increased size will have been exhausted *for many services* in communities that reach a size of 50–150,000 population."[79] Among the signs he reads as portents of institutional failure of massive proportions, and which he attributes to large size,[80] Ostrom notes that streets "can no longer be kept clean in many areas" of New York City and that Los Angeles school administrators "can no longer cope with cockroaches."

Is Noninterventionism Clearly More Appropriate When Resources Are Growing Scarcer? Perhaps the most subtle snare in the public choice argument inheres in its assumption of relative resource affluence and in the related prescription for governmental nonintervention. Despite a variety of conceptual and practical attractions, the public choice argument seems well designed to generate awkward unintended consequences, even opposite those desired. This could happen because that argument basically prescribes a decision making pattern appropriate to an extractive or exploitative phase of economic development where great or even apparently limitless resources are there for the taking.

The proof of this broad contention requires some stage setting. Consider the methodological individual intent on "individualistic choice." As Ostrom explains:

> Individualistic choice occurs whenever each person is free to decide for himself in the pursuit of his own interest. Individualistic choice is characteristic of the market and occurs whenever the only requirement is the willing consent of those individuals who freely agree or contract with one another to exchange some good or undertake some action.[81]

This multiple higgling and piggling is crucial, for Pareto optimality can be achieved only through market dynamics. A Pareto optimum is defined to exist after some transaction if and only if, in their own judgments, at least one party of the transaction is better off and not even a single party is worse off as a consequence. The essential argument is not complex. A Pareto optimum is the ideal outcome; hence a market is indispensable and thus also is the minimum of government intervention the preferred strategy.

The summary does not do full justice to this central position of the public choice argument, but it does suffice to illustrate several of its related weaknesses. Four of these weaknesses will be introduced here, by way of brief illustration of the far broader family.

First, Pareto optimality seems increasingly unapproachable in the world as we are coming to know it: with potentially chronic energy deficits compounded by OPEC policy; apparently shrinking reserves of some commonly used raw materials; with that "revolution of rising expectations" of so many more than ever expecting so much more than before in terms of wages, goods, and public programs; and with the demands of some efforts (e.g., at pollution abatement) being at cross-purposes with historic expectations about reasonable prices or costs, return on investment, levels of employment, uses of energy, etc. That is to say, a Pareto optimum implies a sufficiency of resources, if not an overabundance. Otherwise, a Pareto optimum would be unlikely or impossible, and individuals pursuing it might succeed only in impoverishing one another. For if resources are scarce—and especially if they are growing scarcer and/or more expensive quickly—log-rolling or market dynamics will typically result in many or even all parties to a transaction being worse off than before. Thus Shepsle notes that the only cases in which the Pareto rule applies are "free of conflict," which hardly constitutes "a very political situation."[82] Similarly, van Dalen concludes that the rule is a "fair-weather friend" which has the limiting quality of applying only "when times are good, *very* good."[83]

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[77] It is also correct that larger urban schools do pose more challenging targets for change, in part because of the organizing concepts prevalent in them, as Ostrom argues. See Richard A. Schmuck and Matthew B. Miles, eds., *Organization Development in Schools* (Palo Alto, Calif.: National Press Books, 1971), esp. pp. 236–37.

[78] Ostrom, p. 59.

[79] Ibid., p. 119; emphasis added.

[80] Ibid., p. 121.

[81] Ibid., p. 56.

The point can be approached from another angle, with implications far beyond the Pareto optimum. The politics of bargaining-incrementalism require a definite "morality of restraint," with restraint being defined in terms of the delicate balance between expectations that are realistic or even modest in light of existing resources. As Gawthrop notes, "Self-control, moderation, and circumspection are the cardinal virtues of prudential politics," and these qualities cannot be limited only to professionals. Broadly, the required condition is that bargaining-incrementalist "expressions would be—indeed, must be—made within the given limits of the rules of the game."[84] Present conditions—more individuals and group than ever before making greater and more insistent demands for resources that are not being increased rapidly enough, and are even being depleted—are precisely those which encourage testing, or breaking, those "given limits" of self-control, moderation, and circumspection. Individualistic choice and its associated Pareto optimum thus add fuel to what is already a roaring blaze of newly heigntened expectations, which the times may little need.

In fact, Ostrom recognizes a similar special case, and he graphically sketches the probable consequences of an unrelieved emphasis on individualistic choice. His special case is a "common-property resource with a renewable yield or supply," such as a water supply. Individualistic choice, Ostrom informs the reader, "has great advantages in reducing the costs of entrepreneurship so long as supply exceeds demand." The "so long as" is crucial. When demand exceeds supply, and as each individual pursues narrow self-interest, so will the supply diminish and so also will the social costs of the community of users increase. Should voluntary actions to limit demand be agreed to by some, this would only encourage others to pursue a hold-out strategy and thus to profit disproportionately from the limited usage by others. Bad can easily lead to worse, Ostrom advises; not only will the community of users suffer a loss, but other more tragic consequences may be expected. "Individuals in weak economic positions will be forced out," Ostrom notes. "The neighborhood effects which are generated may include poverty, deprivations, threats, and even violence."[85]

A second weakness is that public choice theory's strong preference for decentralization also is ill-suited to growing—and even less to chronic—resource scarcity. Allocations of relatively plentiful resources among people with moderate, even if growing, expectations as to what they have a legitimate claim on constitute a tame class of problems, and it is to this class that public choice theory applies more directly. In contrast, really scarce resources will (as Baker notes) "require more direct policy responses to the question of who gets what,"[86] which implies critical value issues and the differential weighting of contending tastes and preferences that exist, that might come to exist, or whose development should be encouraged.

Third, a pursuit of Pareto optimality implies a very restricted range of reactions to problem situations. Consider the case of the polluter, as developed by De Gregori. He explains that a Pareto optimum could be achieved only if the victims of the pollution "voluntarily assessed themselves and bribed the polluter not to pollute or to pollute less." De Gregori explains:

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[82] Shepsle, p. 30.

[83] Hendrik van Dalen, "Some Theoretical Perspectives on Government's Role in Consumer Protection" (paper prepared for the Southwestern Public Administration Meeting, Phoenix, Arizona, Nov. 12, 1975), p. 12.

[84] Louis C. Gawthrop, *The Administration Process and Democratic Theory* (Boston: Houghton Mifflin, 1970), pp. 439, 439–40.

[85] Ostrom, pp. 56–58.

[86] Baker, p. 59.

> All actions would have been voluntary, and consequently everyone would have acted so as to make their condition better, not worse. Any solution that required the polluter to pollute less would not fit Pareto because the polluter would be worse off, because he would have either had to absorb the cost directly or, in passing the increased costs on, lower the quantity demand of his product and therefore lower his profit.[87]

There are paradoxes in such "solutions." For example, this approach to alleviating pollution would be likely to aggravate it. For if you bribe a polluter to pollute less or not at all, it may encourage him initially to pollute a great deal in order to increase the bribe. Indeed, the improbability of such voluntary action implies that the probable action in such cases will be no action, at least until matters get very bad.

Fourth, the theme of government nonintervention is not meant lightly in public choice theory, as in the Pareto optimum. Baker concludes that the theory's basically noninterventionist approach to making public policy is based "upon the perspective of the representative individual in combination with a stated preference for decentralized markets as the best means of producing and providing public goods and services." The theory, Baker notes, prescribes "unfettered forces of a laissez-faire market for the purpose of determining the substance, scope and direction of public policy."[88]

Some public choice zealots push noninterventionism even further, much further. Consider government interventions intended to get manufacturers to produce safe products and to accept liability. Some public choicers argue that even such interventionism is ill advised, since (in McKean's words) the "only thing that would happen to the consumer's position is that he would be denied the opportunity of taking the risk." The underlying logic is instructive. McKean explains that "some consumers, especially . . . the poor," would prefer the risk rather than the greater cost of a safer product. Any shift toward safer products, he concludes, would then work "to the detriment of the poor."[89]

The bias of public choice theory toward noninterventionism at least bypasses major questions historically at the heart of the debate over what constitutes the just state. These include: Are all "preferences" or "tastes" to be evaluated equally? Does the state direct attention to the consequences for others when some act on their own preferences? What is to be done when one segment of the citizenry comes to feel that their preferences conflict in serious ways with the preferences of others? Etc., etc.

An extended comparison will develop the point that something of substance is lost in this bypass, which derives basically from the simplification of values at the center of the public choice argument. In public choice theory, a kind of "unseen hand" is said to be generated by individual choices which determine what public goods and services are provided. What is to be done when some historical policy consequences have developed in unattractive or undesirable ways, as in racism or sexism or thalidomide or whatever? Baker worries that public choice theory would be impotent in doing much about such issues, or perhaps not even recognize them. More broadly still, Baker concludes that public choice theory "allows, if not encourages, racial, sexual and other forms of discrimination." If no distinctions are to be made between preference patterns, it appears that the theory provides no way of distinguishing (let us say) a preference for "my kind of soap" from a preference for "my kind of people," with all the prejudicial possibilities implied thereby.[90]

Do "Cycles of Governance" Make a Difference? Another major source of probable but unintended consequences for public choice theory inheres in its broad assumption that there is a one-best approach to governance, which in turn presumes a kind of standard set of governmental issues or challenges.

Granting the convenience of this assumption, it appears to be seriously inadequate. In Kaufman's terms, for example, it is necessary at a minimum to distinguish three major emphases in our public administration history: (1) representativeness; (2) politically neutral competence; and (3) executive leadership. "None of these values was ever totally neglected in any of our past modifications of governmental design," Kaufman notes, "but each enjoyed greater emphasis than the others in different periods." And that emphasis on one value at a time, in effect, allows discontents to develop in areas poorly served by some one value. As Kaufman explains: "At different points in time, enough people (not necessarily a numerical majority) will be persuaded by one or another of these discontents to support remedial action—increased representativeness, better and politically neutral bureaucracies, or stronger chief executives, as the case may be." The condition is far from static. Fixation on one of the classes of discontent allows other discontents to accumulate and, sooner rather than later, this motivates a major change in emphasis. As Kaufman concludes, "no totally stable solution has yet been devised. So the constant shift in emphasis goes on."[91]

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[87] De Gregori, p. 210.

[88] Baker, p. 54.

[89] Roland McKean, "Product Liability," *Quarterly Journal of Economics*, 84 (November 1970), 623. Most public choice theorists incline to somewhat more government intervention, as in providing information about product safety. See W. Y. Oi, "The Economics of Product Safety," *Bell Journal of Economics and Management Science*, 5 (Spring 1973), 26.

[90] Baker, p. 55.

These shifts in the cycle of governance are responses to broadly felt changes in central clusters of problems. Approaches and structures sensitive to one cluster of problems are likely to be unresponsive—if not seriously counterproductive—to the second or third clusters. Adaptive slippage is likely to increase under two conditions: when the tasks of governance are seen as homogeneous and constant over time; and when zealous allegiance is given to approaches and structures appropriate for only one of the three cycles of governance.

To translate, Ostrom's prescriptions seem intended to emphasize representativeness as a central value. But those prescriptions are variously out of potential kilter with the goal of a competent public service and more seriously at odds with the central value of executive leadership.

Zealous commitment to public choice theory, that is to say, may prove too much of a good thing as conditions change, which they almost certainly shall. It is well and good, consequently, for public choice theorists to propose for argument and debate that the American public economy was intended to avoid concentrations of power, that it has prospered because of that basic structural bias, and that this bias should continue where it exists, be reinstituted where backsliding has occurred, and be inaugurated wherever it has not existed. But it is awkward to advocate the bias without conditions, as awkward as it is to argue that reliance on markets or decentralization is always utopian and unrealistic. The awkwardness has two roots: Sensitive adjustment to changing conditions may be complicated, if not precluded, by the emphasis on *a* structuring bias, and, relatedly, participants may become so intense about either/or arguments that the goals of attack/defend get more attention than truth seeking and problem solving. Waldo nicely captures the essence of both awkward consequences. He worries about an outcome like that in classical tragedy, "wherein all the protagonists are 'right', but tragedy ensues." Waldo urges that we seek to avoid, "from the conflict of two legitimate principles, a denouement that could be tragic for all."[92]

Conclusions

The preceding analysis should suffice to support three major points.

1. Sufficient methodological inelegancies encourage great caution in accepting Ostrom's democratic administration and the ideational infrastructure on which it rests.
2. Moreover, sufficient reason exists to look critically at any prescriptions for action derived from public choice theory. Not only are a number of its major assumptions either questionable or clearly inadequate as empirical descriptions of existing conditions, but also it seems that public choice theory is likely to generate major unanticipated consequences, some of them directly contrary to intended consequences.
3. Finally, this critical survey does not support the either/or contraposition attempted by Ostrom. Indeed, Ostrom himself at times comes close to advocating a contingency approach. Thus he observes that, "It is the exigencies of national defense that peculiarly require a system of one-man rule inherent in the prerogatives of the commander in chief—not the exitencies of community assistance programs, rural development, poverty, or the public security of neighborhood streets."[93] But Ostrom does not go on to detail the classes of exigencies to which various approaches/structures could be shown to be variously appropriate by a range of empirical tests. This would have directed attention away from polarizing and polemical formulations, and would have encouraged describing the conditions under which differing approaches to providing public goods and services would lead to which specific consequences. This would have put the emphasis where it belongs, in short.

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[91] Herbert Kaufman, "Administrative Decentralization and Political Power," *Public Administration Review*, 29 (January 1969), 3 and 4.

[92] Dwight Waldo, ed., *Public Administration in a Time of Turbulence* (Scranton, Pa.: Chandler, 1971), pp. 259–60.

[93] Ostrom, p. 125.

Some Problems in Doing Political Theory: A Response to Golembiewski's "Critique"*

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Robert T. Golembiewski's "A Critique of 'Democratic Administration' and Its Supporting Ideation" serves a useful purpose. It challenges some assumptions of public choice theory as well as some of my arguments about democratic administration as an alternative to bureaucratic administration. Golembiewski's "critique" is representative of various criticisms that have been made of public choice theory. A response to each point raised is not feasible in this essay. I shall respond only to major issues.

Several issues are raised that are central to the task of doing political theory. These include the general issue of methodological individualism, the related assumptions about self-interest and preference orderings, and the place of values, efficiency, and Pareto optimality. These issues are, in turn, imbedded in a more general problem: that of using language as a tool for theoretical inquiry and testing the usefulness of different conceptual languages for generating inferences and researchable hypotheses.

The language problem gives rise to a serious potential for misunderstanding. Many conclusions that Golembiewski attributes to me, for example, are not my conclusions and do not follow from the conceptual language that I use. In translating some of my arguments into his language, Golembiewski says something different than I have said. In some cases the virtual antithesis is asserted. In other cases, targets of convenience in public choice theory or economic theory more generally are used to condemn by association without critically examining the relevant issues in my own work. These problems will become apparent in the course of this essay. Before turning to some methodological issues in doing political theory, I need first to establish the context for Golembiewski's "critique" and my response.

The Context

Golembiewski refers to three works that I have authored or co-authored. His primary reference is to *The Intellectual Crisis in American Public Administration*, published in 1973. There, he focuses almost exclusively upon the third chapter—"The Work of the Contemporary Political Economists"—and, within that chapter, upon the two pages concerned with a model of man. His Table 1 is derived from "The Organization of Government in Metropolitan Areas: A Theoretical Inquiry," co-authored with Charles M. Tiebout and Robert Warren and published in the December 1961 issue of the *American Political Science Review*. The third reference is to *Understanding Urban Government: Metropolitan Reform Reconsidered*, co-authored with Robert Bish and published in 1973.

The only reference Golembiewski makes to the chapter on "Democratic Administration" in the *Intellectual Crisis* is a footnote to an assertion that "it is specious to link democracy, freedom, and decentralization as Ostrom does in common with much of the public choice literature" (1977:1500). That chapter focused largely upon formulations advanced by Alexander Hamilton and James Madison in *The Federalist*, and by Alexis de Tocqueville in *Democracy in America*. Golembiewski identifies the supporting "ideation" for democratic administration exclusively with public choice theory. Instead, I observed:

> The work of Hamilton and Madison and of Tocqueville involved the articulation of a theory of democratic administration when measured in terms of the criteria specified by Max Weber. The American experiment, based upon a theory of democratic administration, can thus be viewed as a turning point in pioneering a new course of human development. Democratic administration, through a system of overlapping

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> jurisdictions and fragmentation of authority, acquired a stable form which provides an alternative structure [i.e., to bureaucratic administration] for the organization of public administration (V. Ostrom, 1974:97–98).

My intellectual debt to the work of contemporary political economists or public choice theorists is substantial. Only after several years of collaborating with political economists was I able to reformulate self-consciously my own conceptual language to a point where I could effectively apply economic reasoning to problems of political organization. Much to my surprise, I found upon rereading many seventeenth, eighteenth, and nineteenth century classics in political theory that a similar language and mode of reasoning was used by Alexander Hamilton and James Madison in *The Federalist* and by Tocqueville, Hobbes, Locke, Rousseau, Hume, and Smith among others. Many traditional political theorists were political economists who used economic reasoning to think through problems of political organization. They saw people as using a cost calculus to choose from among alternative possibilities.

The Political Theory of a Compound Republic, published in 1971, was my effort to expound the basic theoretical argument in *The Federalist,* on the assumption that its authors used economic reasoning to analyze problems of constitutional choice confronting the American people in the 1787 to 1789 period. Indeed, I am persuaded that the intellectual developments inherent in a theory of federalism and constitutional rule are as fundamental to the potential development of a political science as Adam Smith's *Wealth of Nations* was to the development of economics. The only difference is that many political scientists, dedicated to the application of natural science methods to the study of artifactual phenomena,[1] have failed to recognize a major intellectual development of Copernican proportions in the history of political thought.[2]

The failure to recognize this fundamental development in political thought can be illustrated by the modern reading of *Federalist* No. 10. Madison's conception of "a republican remedy for the disease most incident to republican government" lay in "the extent and proper structure of the Union" (Hamilton, Jay, and Madison, n.d.:62). This assertion has been widely translated to refer to the "extended republic," not the "compound republic." The qualification pertaining to *proper structure* has been ignored. If this contention about the *extent* of a republic were true, we should expect the Soviet Union to be a less tyrannical republic than the United States.

The puzzle addressed in the *Intellectual Crisis* is: Why have most American students of public administration in developing their conceptual tools ignored the *theory inherent in federalism and constitutional rule* and opted for a *theory of bureaucracy* organized in accordance with what Max Weber has called the "monocratic" principle?[3] (Rheinstein, 1967:349–50.) This puzzle is explained as involving a fundamental paradigmatic choice by early students of political science and public administration when these subjects were first being developed as professional "scientific" disciplines. Woodrow Wilson, as a young and influential political scientist who set the course for the study of public administration, quite explicitly rejected the formulations of Alexander Hamilton and James Madison as "literary theories" and "paper pictures" that did not reflect the "realities" of American politics (Wilson, 1956:30–31).

Public choice theory, with its emphasis upon the nature of goods, added a critical element to contemporary political analysis. Institutional arrangements as one set of variables need always to be related to particular types of goods and services: choice procedures need to vary with the type of goods. The decision-making arrangements characteristic of markets, for example, will predictably fail to cope with the provision of public goods and the management of common-property resources. An explicit theory of public goods assumed that such goods come in many different sizes and forms and gave a new conceptual impetus to developing a theory of public administration fully consistent with a theory of federalism (Olson, 1969; V. Ostrom, 1969; Tullock, 1969).

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[1] I do not accept the logical positivists' position, contrary to Golembiewski's allegation. Rather, I assume, with Thomas Hobbes, that commonwealths or other forms of organization are artifacts. As artifacts created by human design, organizations necessarily entail consideration of both fact and value. A value-free political science is, in my judgment, an impossibility. Empirical evidence is, however, pertinent to knowing the performance characteristics of an artifact.

[2] Rousseau, Montesquieu, Locke, Hume, and others anticipated elements in this development; and Proudhon recognized its fundamental importance when he wrote, "The twentieth century will usher in an age of federations, or else humanity will fall back into purgatory for another thousand years" (Simon, 1973:30). Europe is today fashioning a new community of concurrent regimes, while America builds a new imperialism.

[3] The bias against federalism and constitutional rule is reflected in the assumption that the primary source of institutional failure in the American political system is associated with overlapping jurisdictions and fragmentation of authority. Federalism necessarily entails overlapping jurisdictions; and constitutional rule necessarily entails fragmentation of authority. The municipal reform and administrative reorganization movement sought to eliminate overlapping jurisdictions and fragmentation of authority.

Tocqueville's powerful analysis of the system of "decentralized" administration found in America in contrast to the system of centralized bureaucratic administration that he analyzed in *The Old Regime and the French Revolution* reinforced my own views. Tocqueville would insist that *there is an essential association* between democracy, what he calls "decentralized administration,"[4] and freedom or liberty. Thus, the supporting "ideation" for my thesis regarding democratic administration derives as much or more from Thomas Hobbes, David Hume, Alexander Hamilton, James Madison, Alexis de Tocqueville, and Max Weber as from the work of contemporary political economists or public choice theorists. Each has made fundamental contributions to my consideration of democratic administration as an alternative to bureaucratic administration.[5]

Some Methodological Issues in Doing Theory

The enterprise of doing theory poses a number of difficulties or problems. Golembiewski seems to assume that the human animal can directly perceive and know "reality" as such. If this were possible, there would be no need for theory. Instead we are forced to rely upon language as a tool for reasoning about and knowing something of the human potential and the universe in which we live. The theoretical enterprise is, therefore, subject to severe limitations. Recourse to theory or "general ideas" as Tocqueville expressed it, is "no proof of the strength, but rather of the insufficiency of the human intellect; for there are in nature no beings exactly alike, no things precisely identical, no rules indiscriminately and alike applicable to several objects at once" (1945:II, 13). Tocqueville recognized that the use of language "always cause[s] the mind to lose as much in accuracy as it gains in comprehensiveness" (1945:II, 13). The use of words or terms in a language of discourse always implies simplification. Human thought as mediated through language systems can never comprehend "reality," only simplifications of "reality." We see the shadows in the cave, not "reality" itself.

While recognizing these severe limitations inherent in doing theory, we still have the problem of how do we orient ourselves to the subject of our inquiry? Where do we begin? What elements do we take into account? What constructs do we use to develop different units and levels of analysis? How do we use basic terms to reason through solutions to problems? How do we anticipate the probable course of events that is likely to follow if theoretical conceptions are acted upon? The usefulness of conceptual language systems can be tested by treating inferences as hypotheses and determining which explanation best holds in anticipating the future course of events. Theory is not an end in itself but a tool that enables human beings to use processes of symbol manipulation and reasoning to solve problems and cope with the exigencies of life.

As a point of departure, I assume that the subject matter of political inquiry is the allocation, exercise, and control of decision-making capabilities among people in human societies.[6] Decisions are ordered by reference to rules. Rules are artifacts devised by human beings to create order and predictability in human relationships and to enhance the well-being of those who share in communities of rule-ordered relationships. The order, predictability, and well-being shared in common are public goods—i.e., goods that are subject to joint use where separate individuals cannot be effectively excluded. Since rules are not self-promulgating and self-enforcing, human beings must rely upon the agency of some of their fellow creatures to formulate and enforce rules in relation to the decisions that are taken by others. Serious puzzles about the relationship of rules, rulers, and ruled arise in all human societies and in all forms of human organization. Studies in political theory are broadly concerned with the structure of rule-ordered relationships and the implications that follow from variously structured rule systems.

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[4] Tocqueville in using the term "decentralized administration" refers to the townships, counties, and other local instrumentalities. State legislatures were the relevant centers where control was exercised by reference to the general provisions of state law. Tocqueville's reference is to the American republics—i.e., the states. He has only one chapter that considers the institutions of the national government.

[5] Among earlier twentieth century scholars who made important contributions are John R. Commons, John Dewey, and Mary Parker Follett.

[6] I thus view the major concern in political science to be with institutional variables. I would distinguish this from economics where the subject matter is concerned with the production, exchange, and consumption of goods and services. If similar modes of reasoning can be applied to both types of problems, we have a net gain in developing consistency across the social sciences.

A basic methodological problem in doing political theory is the task of simultaneously taking several elements into account. Public choice theory always requires that attention be given to (1) individuals or other units of analysis, (2) the institutions that order relationships among individuals or other units of analysis, and (3) the nature of the goods or events that are involved. Any one of these elements may vary. A change in the characteristics of any one element may alter conclusions. In a sense, public choice theory can be viewed as a contingency theory where each statement about institutions, for example, is contingent upon stipulated conditions about the nature of the good or service involved.

Administrative or organization theory, by contrast, usually treats institutional aspects without giving simultaneous attention to the nature of the good or service involved. Instead, a general nomenclature that refers to values, objectives, or goals is used without a taxonomy for distinguishing variable types. Golembiewski fails to discern any predictive power in public choice theory because he fails to give simultaneous attention both to institutional arrangements and to the nature of the goods. Given a common-property resource and free or unconstrained access by many individual users, it is easy enough to predict what Garret Hardin has called the "tragedy of the commons" without having to rely upon crude tautological after-the-fact justifications (Golembiewski, 1977: 1496).

Any effort to do political theory requires that certain assumptions or stipulations be made about the basic elements to be used in thinking through or solving problems of political organization. It is these assumptions that Golembiewski finds objectionable. Consideration will be given, in turn, to methodological individualism, self-interest, the ordering of preferences, and values, efficiency, and Pareto optimality.

Methodological Individualism. When I rely upon methodological individualism, I assume that individuals are the basic units of analysis in doing political theory. One begins by taking account of some of the essential characteristics of human beings. It is individuals who perceive, think, evaluate, choose, and act. Organizations are nothing more than aggregations of individuals to realize some joint advantage or common good. The basic approach in using "methodological individualism" is to take the perspective of representative individuals and think through the implications that follow when they are confronted with the opportunities and constraints inherent in a set of decision rules and in view of the potential payoffs associated with characteristic goods.

Thomas Hobbes is a methodological individualist when he assumes that man is both the *"matter"* and the *"artificer"* of commonwealths (Hobbes, 1960:5). The first task for Hobbes in explaining the nature of commonwealths is to treat human nature, as he does in the first 16 chapters of *Leviathan.* Alexander Hamilton insists that the design and construction of a Federal government must be based upon an individualistic conception of political experience. A Federal government, Hamilton says, "must carry its agency to the persons of the citizens" (Hamilton, Jay, and Madison, n.d.:98). Hamilton further suggests that justice cannot be done unless the actions of government relate to persons in their individual capacities. To apply sanctions to collectivities involves guilt by association.

Golembiewski concedes the essential point in methodological individualism when he observes: "To be sure, only individuals can perceive and make decisions" (1977:1495). If this is so, he is required to take account of individuals as basic units of analysis in his efforts to build a theory of organization. Failure to discipline oneself to take explicit account of individuals as basic units of analysis leads many political and administrative analysts to take on the perspective of omniscient observers. Then they assume that all collective goal-oriented behavior is a rational means-end calculus where people can be ignored. Methodological individualism is quite different from "individualistic choice" despite Golembiewski's identifying the two as equivalent (1977:1493). Individualistic choice refers to decision rules where each individual is free to decide for himself. Methodological individualism can be used to analyze behavior even in the absence of any authority on the part of an individual to decide a course of action. As an individual, I am, for example, not competent to make the decision to involve the United States in a war. War is not a matter of individualistic choice. My response to collective decisions, however, will be affected by my individual calculations.

Using the individual as a basic *unit* of analysis does not mean that one is confined to that *level* of analysis.[7] The task is to explain

[7] This point is correctly emphasized by Phillip M.

why and how individuals aggregate themselves into collectivities and associations of varying sorts and complexities. I expect aggregation rules to affect the nature of the organization or collectivity that is created. These rules structure the choices and behavioral characteristics of the individuals who function in an organization or collectivity. Aggregation rules are rarely simple summations of individual decisions made by separate and distinct persons. Instead, aggregation rules are those used in an organization to authorize joint action and constrain individual decisions accordingly. In turn, interorganizational relationships result from the internal structure of different organizations and the patterns of interaction among organizations. These are also structured by reference to explicit or implicit rules. Most forms of collective behavior are affected by at least two sets of rules: those governing relationships within collectivities and those governing relationships among collectivities.

In Chapter 3 on "The Work of the Contemporary Political Economists" I drew upon Mancur Olson to demonstrate that "unrestricted individualistic choice in relation to common-property resources or public goods can generate destructive competition where the greater the individual effort the worse off people become."[8] I then showed how individuals can escape this type of prisoners' dilemma by constituting a collectivity that replaces individualistic choice with collective decision rules of less than unanimity (Buchanan and Tullock, 1962). This solution can be reiterated to derive a federal solution (V. Ostrom, 1973). *The system of administration that results can only be understood as a system of multiorganizational arrangements.* Much of my own work with its emphasis upon polycentricity (1972), public-service industries (1968, 1971a), intergovernmental relations (V. Ostrom and E. Ostrom, 1965), and federalism (1969) more generally has been preoccupied with the multiorganizational level of analysis and not with the individual level of analysis. Yet, individuals remain the basic units of analysis.

Failure to recognize that Ostrom, Tiebout, and Warren is primarily concerned with the organization of government in metropolitan areas as a polycentric order leads Golembiewski and his colleague Keith Baker into serious confusion. The criteria specified in that article for considering problems of scale in public organization were used to compare two different models of public organization: gargantua and a polycentric system. We argued that the conflict inherent in scale criteria can be resolved in gargantua if "field" and "area" organizations were created to "recognize the variety of smaller sets of publics that may exist within its boundaries" (1961:837). These conflicts can also be resolved in a polycentric system by separating the *production* of a public good or service from its *provision* in the sense of arranging for its joint consumption. Diverse criteria can then be taken into account in the multiorganizational structure of a polycentric order. Baker's puzzle can be resolved if he shifts his focus from a single organization to a polycentric order. He recognizes that the level of analysis is not directed at individuals per se. But he focuses exclusively upon the single-organization level of analysis that he identifies as "*a* public enterprise" (Golembiewski, 1977:1499; my emphasis added). He does not recognize the existence of a multiorganizational level of analysis.

Methodological individualism is only the beginning point; other units and levels of analysis are built upon that foundation.

Self-Interest. In taking the perspective of methodological individualism, I assume that any individual has preferences. These preferences will systematically affect the decisions the individual makes. Preferences are assumed to vary among individuals. In the absence of an ability to read one another's minds, I further assume that each person has limited information about others' preferences except as they provide information or engage in transactions that reveal their preferences.

Gregg (1974) in an essay on "Units and Levels of Analysis: A Problem of Policy Analysis in Federal Systems." Gregg contends that much contemporary research in comparative policy analysis fails to give proper attention to different units and levels of analysis. Gregg states a number of hypotheses derived, in part, from work criticized by Golembiewski that applies to the multiorganizational level of analysis (pp. 79–80).

[8] This is not a special case which indicates the "unrelieved emphasis on individualistic choice" (Golembiewski, 1977:1505) in dealing with all potential resources or goods and services. Rather, it is a paradigm case where we would expect market failure and where the failure of exclusion would require recourse to nonmarket (public) organization. Golembiewski apparently fails to understand the distinction between paradigms as ways of thinking and decision structures as ways of ordering activities or doing things. One paradigm does not entail recourse to only one type of decision structure. Public choice theory is a way of thinking that can have reference to all types of decision structures as a key variable among its analytical elements. The preoccupation in public choice theory is not with markets but with public decision-making arrangements: therefore, *public choice.*

The assumption of self-interest combined with the other restrictive assumptions related to the nature of private goods and the structure of a competitive market can be used to derive tightly reasoned inferences about how individuals in the aggregate will behave. This assumption, in combination with a less restrictive assumption about the nature of other goods and other institutional arrangements, does not permit one to derive as rigorous inferences as is possible with the perfect market model. Even less rigorous applications, however, save one from the error of assuming that humans can be perfect automata in the sense of being perfectly obedient servants in a bureaucracy. In using the assumption of self-interest, I would never suggest, for example, that human organizations or institutions can be subject to "fine-tuning" as Golembiewski does (1977:1489, 1500).

The problem of using the assumption of self-interest to think through inferences about how individuals will behave in light of different conditions can be illustrated by Hobbes' analysis. Hobbes uses a narrow conception of self-interest: individuals will seek their own physical preservation. He demonstrates, however, that *unconstrained* or *unlimited* pursuit of self-interest in a world of scarce resources will lead to a state where each individual is at war with every other individual. Because of the interaction that occurs among individuals, individuals find that instead of realizing their own preservation, as they would prefer, they are each threatened with their own extinction. Hobbes conjectures that individuals who find themselves confronting such a puzzle will then resort to reason and think through the conditions—the moral precepts—that will enable them to realize a state of peace rather than war. Peace is valued not as an altruistic good, but as a condition that enables individuals to better preserve their own lives and pursue their own good while others do so, too. Hobbes bases his analysis upon implications that follow from recognizing the essential capabilities and desires of other individuals. He assumes that individuals will be prepared to order their preferences to realize the benefits of peace so long as others do so, too. This argument lays the foundations for Hobbes' theory of the state.

Tocqueville recognizes much the same point when he refers to "self-interest, rightly understood." In the revolutionary era that marked the decline of aristocracy and monarchy, Tocqueville observed that the poor man "adopted the doctrine of self-interest as the rule of his actions without understanding the science that puts it to use; and his selfishness is no less blind than was formerly his devotion to others" (1945:I, 11). Blind or unlimited pursuit of self-interest will lead to tragic consequences. Self-interest rightly understood depends upon the enlightenment of a right understanding. Learning occurs, and self-interest becomes enlightened. I assume that this right understanding for Tocqueville is consistent with the moral precepts contained in Hobbes' laws of nature. Enlightened self-interest can still be a useful assumption so long as the relevant choice situation is made explicit—i.e., the rule structures and the nature of the goods are specified.

In economic reasoning, the law and order assumption serves as a proxy for Hobbes' more elaborate argument and Tocqueville's right understanding. Hobbes himself makes a similar assumption for assessing relationships within a commonwealth when he uses law as the appropriate measure of justice and propriety. Self-interest constrained by law creates a presumption that each individual will take account of the interests of others to the extent that is consistent with the moral requisites of a legal system and can be enforced as positive law.[9] If crime and the criminal justice system are the subject of theoretical analysis, it would be foolhardy to treat the law and order assumption as anything more than a contingency to be taken into account in cost calculations.

Relying upon a law and order assumption in a theory of constitutional choice presents some obvious limitations. The task is that of creating a lawful order. The essential problem in the design of a self-governing democratic system of government, as David Hume, for example, saw it, is how to devise rule structures so that it is in the interest "even of bad men, to act for the public good" (Aiken, 1948:296). Not any structure will do. There must be checks and potential controls so that officials can monitor one another and citizens can lawfully resist the usurpation of authority by officials. A theory of constitutional choice for a self-governing democratic society needs to be grounded in a right understanding that can be used by self-interested individuals both to design political institutions and to assess their performance. This is the way that values get built into societies as human artifacts; and human beings can be said to govern themselves by institutions of their own choosing. These problems are treated in *The Political Theory of a Compound Republic* where I explicitly state how normative considerations enter into the design of political institutions.

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[9] In a one-paragraph discussion of the law and order assumption in the *Intellectual Crisis,* I conclude the paragraph in the following way: "In the absence of any law and order assumptions *it might be necessary* to assume a Hobbesian state of war as the *prevailing human condition"* (1974:52). The words in italics are used in quotations by Golembiewski in the following statement: "When pushed on the issue of the 'real-world inapplicability' of one of his assumptions, or about a conflict between assumptions, illustratively, Ostrom only allows that 'it might be necessary' to make different assumptions as to the 'prevailing human condition' " (1977:1491, cf. 1494). I had no sense of being pushed by anyone when I wrote the above statement; I have never used the term "real-world inapplicability"; and Golembiewski's remark, in addition to its all-too-typical misleading use of quotations, seems meaningless on its face. Although I do not myself know for certain what the human condition would be in the complete absence of civil order, Hobbes' argument, that it would be a war of each man against every man, is sufficiently compelling that it cannot be dismissed lightly. For my own part, I consider the argument fundamentally sound.

Theory is never spun out of an assumption of self-interest alone. The self-interest assumption is useful only when we analyze how hypothetical individuals might confront choices in specifiable situations defined by reference to rule structures and the potential payoffs inherent in particular types of goods and services.

Ordering of Preferences. In using the perspective of methodological individualism, it is necessary to take a stand on whether one assumes that individuals are essentially rational or irrational in their behavior. I make a simple one-sentence assertion in *Intellectual Crisis:* "Rationality is usually defined as the ability to rank all known alternatives available to the individual in a consistent manner" (V. Ostrom, 1974:51). One cannot speak of individuals as being able to make up their own minds without assuming some capacity to order preferences in a consistent way: mindless individuals are not rational.

I did not make the argument regarding transitivity though many economists would take that position. This assumption is especially important for those economists who assume that preferences can be translated into utiles and all utiles can then be calculated on a master dial called utility.

I have reservations about this formulation. I believe that W. R. Ashby in *Design for a Brain* formulated a better solution where "essential variables" or "values" (Ashby, 1960:41–42) can be ordered by a configuration of readings on multiple dials that have reference to areas of acceptability and limits rather than to cardinal numbers.[10] However, I would still assume that individuals can order their preferences in a consistent way. The precise nature of this consistency I do not know, though it seems all but certain that it is not a simple transitivity principle.

Golembiewski goes on to confuse the problem of preference orderings as they apply to the choices that *individuals* make with the Arrow problem regarding *social choice.* Arrow demonstrates that it is not possible to make a simple summation of individual preferences and derive a collective choice[11] that will meet what Arrow poses as a set of simple conditions for a rational collective choice. Arrow's impossibility theorem is not addressed to the issue of preference orderings at the individual level, but to the problem of social or collective choice.

In considering the problem of preference orderings, I make explicit reference to the problem of information and the problem of learning. When all possibilities are not known, the unknown possibilities cannot be consistently ordered in an unequivocal way. Uncertainty exists. Once uncertainty is postulated, I believe that it is necessary to introduce an assumption about learning. This led me to conclude: "Where learning occurs, the assumption of rationality may also have to be modified to allow for a reordering of preferences as the individual learns more about the opportunity costs inherent in different alternatives" (1974:51). Since learning occurs in a context that involves time, we can still assume that individuals are able to order preferences within limited time horizons.

Values, Efficiency, and Pareto Optimality. I assume, following Hobbes, that all human choices involve two sets of calculations. The first set is grounded in positive knowledge and pertains to the calculation of the probable consequences that are associated with alternative courses of action. The other set of calculations involves a weighing of the alternative possibilities and a selection of a course of action from the larger set of possibilities. I presume that the weighing and selection process involves evaluation or a consideration of values.

In doing positive theory it is necessary to handle the value problem by stipulation. The usual maximization postulate is simply a stipulation to the effect that an actor will choose those alternatives that will yield the greatest net benefit. This can be stated equivalently in a language that speaks of least-cost in foregone opportunities; or it can be stated loosely as Madison states it when he speaks of choosing the greater good or the lesser evil. Positive theory always includes such a meta-normative element.

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[10] Ashby's formulation can also be applied to the maximization problem. It is not necessary to maximize multiple values; multiple values can also be dealt with in relation to areas of acceptability and limits.

[11] The phraseology here is chosen to reflect Golembiewski's explanation of methodological individualism (1977:1491) rather than the language that Arrow uses in *Social Choice and Individual Values.*

A different type of intellectual venture is involved when the question is posed as to what criteria should be used to evaluate or guide decisions–i.e., to weigh and select from alternative possibilities.[12] This type of venture can be extended to conceptualize different variations for dealing with a particular type of criterion. Evaluative criteria can be used both to inform the process of design in the creation of artifacts and to assess performance. We might, thus, use evaluative criteria to assess the performance of two different systems of organization quite apart from having used evaluative criteria in the design of organizational arrangements.

In *Intellectual Crisis* and much of my other work I have used efficiency as the relevant criterion for measuring or evaluating changes in human welfare.[13] For the last century, arguments over reorganization and reform have focused largely upon efficiency and economy. The basic definition of efficiency is specified as "the accomplishment of a specifiable objective at least cost; or, a higher level of performance at a given cost. . ." (1974:48). Many variants of this criterion can be stated. Benefit-cost analysis often relies upon the minimal criterion that benefits exceed costs. Having reference to a cost calculus, however, has nothing necessarily to do with cadres of experts. Ordinary people can make cost calculations.

In dealing with economies of scale in a technical sense, an efficient solution is one that derives the lowest average cost for producing a given type of good or service: the appropriate scale being the production level that yields the lowest average cost. In Ostrom, Tiebout, and Warren, we were concerned that economies of scale on the production size be treated as one of the relevant criteria for considering scale problems for the organization of governments in metropolitan areas. This is not inconsistent with the basic definition stated above.

Much of the literature in the traditional theory of public administration treats efficiency as being synonomous with perfection in hierarchical organization. Woodrow Wilson assumed that perfection in hierarchical organization would also maximize efficiency as the least-cost solution to accomplish policy objectives. Basic ambiguities exist in Max Weber's analysis because of this double meaning in the use of the term "efficiency." I would argue that these two usages are not consistent with one another. Tullock's *Politics of Bureaucracy* (1965) provides the logic for such an argument.

A variant in formulating the efficiency criterion is Pareto optimality. A Pareto optimum exists when no change could occur without making someone worse off. The Pareto criterion is an "ideal" measure. Human experience will never attain that ideal. If the Pareto optimum existed no transaction would occur. A Pareto-efficient move is justified in the limiting case where someone could be made better off but no one would be made worse off. The condition for a Pareto-efficient move can be viewed as the equivalent of relying upon a decision rule of unanimity where individuals would be free to veto any action that left them worse off.

Golembiewski and those he quotes seem not to have understood the argument advanced by Buchanan and Tullock about the Pareto criterion. Buchanan and Tullock make the basic association between a Pareto-efficient move and the rule of unanimity. This is why they use the unanimity rule as the foundation for their analysis. With the introduction of their cost calculus and certain other assumptions incuding the assumption that individuals will have an equal probability of finding themselves among either winners or losers in future collective decisions, Buchanan and Tullock were able to show that it would be a Pareto-efficient move for individuals at the constitutional stage to opt for a set of decision rules that *does not* meet the condition of Pareto efficiency in taking collective actions. This rather brilliant piece of normative analysis apparently escaped Golembiewski's attention. Buchanan and Tullock would expect cost calculators to opt for a set of decision rules that would minimize interdependency costs rather than insist upon a rule of unanimity. Buchanan and Tullock do not expect collective decisions to be Pareto-efficient.

We are still left with the basic problem in political theory that the instruments of coercion necessary for realizing mutually productive relationships can also be used to dominate the allocation of values in a society and oppress those who are subject to such instruments of coercion. As soon as the rule of unanimity is relaxed, the opportunity arises for some to exploit others. If most significant issues of public policy do not admit to Pareto-efficient moves, we cannot be confident that public policy decisions necessarily represent improvements in human welfare. Too frequently policy decisions rather clearly appear to contribute to the erosion of human welfare. Since I expect many decisions to be grossly Pareto-inefficient, I have no grounds for accepting the status quo as optimal.

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[12] John Rawls' *Theory of Justice* represents an intellectual venture of this type as does Plato's *Republic.*

[13] This means that I have given less explicit attention to justice and error-correction as important evaluative criteria. I assume, without having provided the demonstration, that the evaluative criteria of efficiency and justice are consistent with error-correction as an evaluative criterion.

Unfortunately, in reading Golembiewski's essay I have not been able to unravel what he means by value or values. He says approvingly that "De Gregori insists on seeing the full array of values that provide specific content for generic terms like, "tastes', 'preferences', 'freedom', and so on. . ." (1977:1493). "Freedom" is not of the same logical class as "tastes" and "preferences." To provide "specific content" for terms like "tastes" and "preferences" would presumably require reference to all potential goods and services. I cannot discern whether Golembiewski's repeated reference to "values" has the same meaning as "goods."

The problem can be indicated if we refer to Lasswell and Kaplan's definition of a value as a "desired event" (Lasswell and Kaplan, 1950:16). This definition couples desire or preference with the event that is the object of that preference. Public choice theory considers the nature of goods and services–i.e., events for which people have preferences–to be a primary element in analysis. The *Intellectual Crisis* distinguishes three types: private goods, public goods, and common-property resources.[14] Criteria for evaluation and choice can be considered independently of the good, objective, or goal that is being evaluated or selected.

I am simply left puzzled by Golembiewski's allusion to "efficiency for what." Consistent application of the criterion of efficiency rightly understood will enhance human welfare, assuming that individuals are presumed to be the best judges of their own interests. But this is a simple tautology.

Golembiewski tells us that he is rankled by the word "inherent" when I indicated that "the appropriate scale [of organization] will vary 'with the boundary condition of different fields of effects inherent in the provision of different public goods and services' " (1977:1501). Goods come in different sizes and forms; therefore, they have different fields of effects. I simply mean, for example, that the use, organization, and management of a ground water basin in a metropolitan area will involve different, but not entirely independent, domain and boundary conditions from the organization and management of a flood control program. Movements of ground water involve tangibly different fields of effects from surface flood flows. One is clearly a common-property resource; the other is a potential threat. The reduction of the potential threat can be viewed as public good for the community of people that may be affected. I am at a loss to understand his unhappiness with the word "inherent" in this case.

I am also puzzlied by Golembiewski's assertion that "[public choice] theory, Baker notes, prescribes 'unfettered forces of a laissez-faire market for the purpose of determining the substance, scope and direction of public policy' " (1977:1506). Both Baker and Golembiewski must realize that a public choice theorist would expect market failure in a public-good situation. It would make no sense to prescribe a "laissez-faire" market solution. Neither I nor, to my knowledge, any other writer whom he criticizes, does so. Indeed, some public choice theorists would deny the possibility of a "laissez-faire" market–i.e., a market that existed without a supporting structure of public institutional arrangements. It takes a sophisticated political system to maintain an effective, competitive market economy.

Perhaps the major conceptual innovation developed in my work with Tiebout and Warren was to recognize that the task of organizing for the collective consumption of a public good or service can be treated independently of the task of organizing for the production of a public good or service. Once a collectivity is organized to tend to the problems associated with collective consumption that collectivity can arrange for production processes to be performed in different ways including contracting with private vendors or other collectivities.[15] This buying and selling of public goods and services permits a market-like or quasi-market condition to exist, but that condition is radically different from anything that might be called a "laissez-faire" market. I personally have been careful to avoid using the term "market" or "market model" and have consistently used the terms "quasi-market" or "market-like" organization.

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[14] A more general typology is provided in V. Ostrom and E. Ostrom, 1977.

[15] This argument is more fully developed in V. Ostrom and E. Ostrom, 1977.

The existence of such possibilities clearly implies that nonbureaucratic coordinating mechanisms can exist in the public sector.

Golembiewski can unquestionably find some neoclassical economists who would view the market process as the ultimate arbiter of values. The contractarian formulations from Hobbes onward treated the problem of an ultimate arbiter of values in an explicitly political or social context long before Adam Smith's formulation of market theory. Smith, a professor of moral philosophy, was as much concerned with justice as efficiency and never viewed the market process as having priority in determining social values (Billet, 1976). The implicit contractarian position with which I would associate myself is that the basic criteria for human action depend upon general agreement or consensus among the people that comprise self-governing communities. These criteria pertain to enlightenment (error correction), justice (equity or fairness), and welfare (efficiency), among others. They form the basis for stipulating the basic constitutional terms and conditions of governance so that all authority is subject to limits. People acting collectively retain the basic prerogatives of constitutional decision making in relation to the diverse collectivities in which they participate. People acting individually retain basic constitutional prerogatives to govern their own affairs. All individuals participate in multiple political communities and share in testing and evaluating the conceptions and criteria of constitutional choice that are being acted upon in the conduct of different political experiments under changing conditions. A process of inquiry, contention, debate, and deliberation shapes decisions that are subject to review and reconsideration so that no essential interests are ignored. Tyranny in the small is no more justified than in the large. The interests of the poor and the disadvantaged are as essential as the rich and powerful. The ultimate arbiter of values, for me, is the process by which conflicts are articulated, processed, and resolved in mutually enlightening and mutually productive ways rather than repressed. General agreement and consensus about the constitutional order are maintained. This process requires access to multiple decision structures reflecting diverse communities of interest where all authority is subject to limits and dominance by any single center of authority is foreclosed.

The Use of Political Theory

In considering different approaches to theory, Golembiewski characterizes public choice theory as resting upon a methodology that "emphasizes a closed-system circularity, while it permits incautious building upon assumptions that are often suspect" (1977:1491). He pleads for assumptions that are more realistic, alleging that the "motivation to test reality characterizes neither Ostrom nor the public choice literature, in the main" (1977:1492). Rather, he contends, "Ostrom's methodological approach encourages the . . . treatment of theory as the-end-of-the-road rather than as hypotheses-to-be-tested" (1977:1492). He later asserts, "Ostrom accepts the broad-band notion of self-interest and, however motivated, that acceptance creates far greater theoretical problems than it solves" (1977:1496).

Indeed, there is a good bit of closed-system circularity in public choice theory—doubtless too much in places. But a degree of "closed-system circularity" is one of the basic characteristics associated with the rigorous use of analytic methods. In my own work I prefer to use assumptions of uncertainty, fallibility with capability for learning, and what Golembiewski calls "broad-band" self-interest in the belief that such assumptions are more realistic than assumptions of certainty, perfect information, or blind self-interest. I do so precisely because I believe that such assumptions are more useful in generating researchable hypotheses. The cost entailed is a significant loss in logical rigor. I see little purpose in solving logical puzzles unless the intellectual effort can be used to enable human beings to cope more successfully with practical problems of organization.

The essential problem in public choice theory is to derive the implications that follow when (1) self-interested individuals choose maximizing strategies within (2) stipulated organizational arrangements when applied to (3) particular structures of events that can be viewed as yielding payoffs and having the characteristics of particular types of goods and services. Based upon such relatively simple elements, it is possible to extend a structure of inferential reasoning with the introduction of new concepts or terms to derive a wide variety of implications and conclusions that apply to different units and levels of analysis.

These chains of reasoning can be used to generate researchable hypotheses. Researchable hypotheses can also be derived from other theoretical traditions. When contradictory conclusions are reached from different theoretical traditions, it is possible to formulate the structure of inferential reasoning into competing hypotheses. Research can then be undertaken to secure evidence to test competing hypotheses (Chamberlin, 1965; McDavid, 1976).

If repeated tests of hypotheses yield results that support one mode of reasoning as against another mode of reasoning, we can have some measure of confidence in treating the one theoretical formulation as the more useful analytical tool for reasoning through solutions to problems of public policy and political organization.

The method of competing hypotheses, if he had used it, would have placed upon Golembiewski the burden of demonstrating that an alternative theory offers more "realistic" assumptions, relies upon less "broad-band" definitions, derives tighter or more rigorous inferences and conclusions, and yields hypotheses that are better supported by empirical evidence. This he does not do. Instead, he objects that I have not defined "small," for example, in a precise way (1977:1501). He wants research results that yield "unequivocal interpretations" (1977:1503). He insists upon a "complete measure of benefits" (1977:1503). Etcetera. Etcetera. Etcetera. No research in the social sciences can meet these demands including the research in Golembiewski's favorite version of organizational theory.

Anyone who has done empirical research on human communities that manifest varying patterns of organization will realize that a single effort to test an hypothesis can involve a substantial expenditure of time and effort. To extend such an effort to a series of tests under varying circumstances involves a substantial magnitude of work. Research done in limited time horizons will necessarily be selective in treating some hypotheses and neglecting others. But the suggestion that there is a virtual absence of empirical research associated with the theoretical work that I have done must appear erroneous to any serious student of the literature.

Since Ostrom, Tiebout, and Warren, the problems of scale in public organizations have stimulated considerable interest among students and colleagues. The argument is seriously misrepresented when it is stated that smaller is better. Rather the argument is that the problem of scale is related to the nature of goods. We expect different forms of public goods and common-property resources to manifest different fields of effects. Varying scales of organization will be advantageous. Mancur Olson states this conclusion in the following way:

> Only if there are several levels of government, and a large number of governments, can immense disparities between the boundaries of jurisdictions and the boundaries of collective goods be avoided. There is a case for every type of institution from the international organization to the smallest local government (quoted in V. Ostrom, 1974:70).

In Ostrom, Tiebout, and Warren we explicitly argued that the publics implicated by different types of potential public goods might vary in size from neighborhood to global proportions. Contamination of the atmosphere with nuclear waste, for example, is a global problem and cannot be controlled by action only at the national level. We explicitly argued that a large-scale metropolitan unit of government—gargantua—is an appropriate scale for many public services.

> The provision of harbor and airport facilities, mass transit, sanitary facilities, and imported water supplies may be most appropriately organized by gargantua. By definition, gargantua should be best able to deal with metropolitan-wide problems at the metropolitan level (1961:837).

But we anticipated that other types of services will impinge upon much smaller communities of interest. Here we would expect disadvantages to accrue when *only* large-scale units of government are available to provide services where community preferences and environmental conditions vary in relation to such services. We expect an advantage to accrue if varying sizes of governmental units can operate concurrently with one another and with substantial autonomy from one another.

These conclusions can be juxtaposed to an argument advanced in the metropolitan reform literature which contends that greater efficiency and economy will be realized by the merger and consolidation of smaller units of government in a metropolitan area into a single dominant unit of government for the area as a whole. These two arguments can be used to derive competing hypotheses.

My colleague, Elinor Ostrom, has explicitly used contrary arguments to derive competing hypotheses in which size of jurisdiction and number of jurisdictions within a metropolitan area are used as independent variables and the level of output is used as a dependent variable (E. Ostrom, 1972). In 1970, she and colleagues in the Workshop in Political Theory and Policy Analysis began a series of studies in Indianapolis, Chicago, and St. Louis that specifically examined the effects of jurisdiction size on the supply of police services to individual neighborhoods in metropolitan areas. Several national commissions have argued that all police services would be more effectively supplied in metropolitan areas if smaller police agencies were consolidated into a single, large-scale police agency for each metropolitan area (e.g., the President's Commission on Law Enforcement and Administration of Justice and the National Advisory Commission on Criminal Justice Standards and Goals). On the other hand, public choice theory would lead one to expect substantial diseconomies of scale in responses to individual calls for service and the production of general area patrol. Hypotheses derived from two theoretical traditions predict *opposite* consequences of using *large-scale* police agencies to produce neighborhood-level police services.

In each study, one data source has been a sample survey of citizens in which citizen experiences regarding criminal victimization, calls for service, speed of response, degree of follow-up, being stopped by police, knowing someone mistreated by police, and citizen evaluations of police services were used as multiple indicators of performance. In the St. Louis study other indicators were used including the proportion of warrants issued to warrants applied for, ratings given to police departments by a sample of police officers, and activity indicators. Similar-systems research designs have been used so the socioeconomic variables and other ecological variables could be treated as parameters by selecting neighborhoods matched on these variables. The size of the police department serving similar neighborhoods was consciously varied. By using multiple indicators of performance and a similar-systems research design, comparisons can be made without a complete benefit-cost calculus that relies upon a single common denominator.[16] Conclusions can be reached about grossly inappropriate scales of organization even though we cannot expect to specify a precise optimum.

In the Indianapolis study, three small (13,500 to 16,500 population), independent jurisdictions served by their own police forces (varying from 18 to 25 officers) were compared with matched adjoining neighborhoods within the Indianapolis Police District (population 485,750) served by the Indianapolis Police Department with 1,100 full-time officers (E. Ostrom et al., 1973). The Chicago study involved a comparison of two small poor black communities in south suburban Cook County with three similar neighborhoods within the city of Chicago (E. Ostrom and Whitaker, 1974). A more comprehensive study was undertaken in St. Louis involving 44 neighborhoods served by 29 different police departments (McDavid, 1974; E. Ostrom, 1976; Parks, 1976; and Smith and E. Ostrom, 1974). When grouped into classes of small (1 to 10 officers), medium (11 to 76 officers), and large (2 departments of 440 and 2,200 officers), small departments performed better on some indicators and medium-sized departments performed better on other indicators. In *no* instance did the large departments have better performance measures than either the small- or medium-sized departments.[17]

Similar studies were replicated by Samir IsHak (1972) in Grand Rapids, Michigan, and by Bruce D. Rogers and C. McCurdy Lipsey (1974) in Nashville-Davidson County, Tennessee. The consistent finding in all of these studies is that the largest departments have the poorest performance on most indicators.

A reanalysis of NORC data from a national survey undertaken for the President's Commission on Law Enforcement and Administration of Justice indicates similar results (E. Ostrom and Parks, 1973). Had the commission analyzed its own data on the effects of police agency size, it would have found evidence to reject one of its own principal conclusions.

These findings are consistent with the hypotheses derived from the theoretical tradition within which I have been working. When large-scale organizations are used to produce services that are highly sensitive to localized community conditions, they are unlikely to perform as well as small- to medium-sized agencies serving similar areas. If all local police services were consolidated, we would expect to see the quality of some of these services deteriorate, to see costs rise, or to see both poorer service and increased costs.

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[16] The development of indicators and measures of performance is the answer to Golembiewski's charge: "... there *can be no* 'cost calculus' for determining the appropriate scale of any organization until an answer to a key question is reasonably in hand. That neglected key question is: Efficiency for what purpose?" (1977:1498)

The problem is to develop appropriate indicators or measures of performance where there is not a single, homogeneous, packageable, and quantifiable product. The problem is difficult, but not so difficult that we are forced to throw up our hands and proclaim to the world that there can be *no* such indicators or measures of output or performance. When comprehensive data are not available for simple calculations of efficiency, it is sometimes necessary to rely upon weaker criteria for evaluating performance such as effectiveness or responsiveness (E. Ostrom, 1971, 1975a, 1975b). The task of developing operational measures and indicators is a major one in empirical research that goes much beyond the development of logical constructs in theory.

[17] Again, Golembiewski concedes an essential point in dealing with size as a variable: "For once any organization gets larger than one in which cozy face-to-face interaction is both possible and convenient, major differences in communication and influence patterns quickly develop" (1977:1501). Communication and influence patterns are what politics and collective decision making are all about. The larger the group, the greater the loss of information and control, and the less influence exercised by *any one person.* This becomes especially important where users of services function as essential coproducers, as in education, welfare services, police services, etc. (V. Ostrom, 1977; Whitaker, 1976).

Pachon and Lovrich (1977) using data from the Survey Research Center, report similar findings for the Detroit and Cleveland areas. However, they argue that, when controls for socioeconomic variables are introduced into their analysis, the basic relationships are reduced or reversed. A careful reading of their footnotes reveals that they rely upon data derived from such small samples and use such questionable statistical techniques that substantial doubt exists about the validity of their statistical analysis.

We do not expect to find that smaller is better as a general rule. Rather, we expect advantage to accrue from diverse scales where critical attention needs to be given to the type of service involved. Response to calls for service and community police patrol, for example, involve quite different scale problems than metropolitan highway patrol, radio communication, detention facilities, crime laboratories, etc. We would expect units serving larger areas to derive an advantage in supplying such services. But, a public-service industry composed of a large number of units operating at several different levels might be expected to supply such services as efficiently or more efficiently than a public-service industry composed of a single dominant agency serving a comparable area.[18] The scale problem can easily become a monopoly problem (Bish and Warren, 1972; Savas, 1974). These speculations become more hypotheses for research at the interorganizational or industry level.

Elinor Ostrom, Roger B. Parks, and Gordon P. Whitaker (1974), and other colleagues in the Workshop in Political Theory and Policy Analysis have developed methodologies for rigorously defining and quantitatively measuring structural variations in the interorganizational structures of public-service industries, including a description of organizational structures for police industries in 80 metropolitan areas (1977a, 1977b). Studies concerned with measures of performance for police agencies operating in differently structured local police industries are currently being undertaken.

A variant upon the industry-structure problem is to ascertain whether contracting for services can derive advantages over the production strategy of relying exclusively upon a municipal department to render a local service. Contracting or traditional departmental service can be conceived of as institutional variables. Roger Ahlbrandt's *Municipal Fire Protection Services* (1973) tests a hypothesis that compares contract service with that supplied by traditional fire departments. Werner Pommerehne and Bruno Frey (1977) have done a comparable study of residential refuse collection in 103 Swiss cities. The evidence in both cases supports the conclusions that private production under contractual arrangements is more efficient.[19] But, as Pommerehne and Frey emphasize, this result can be expected to hold only as long as public policy maintains competitive pressures and constrains tendencies toward collusion among private suppliers. E. S. Savas reports similar findings in a major study of solid waste disposal in American cities (Savas, 1976).

This is only a small fraction of the empirical research that has relevance for the competing hypotheses which can be derived from public choice theory and from the more traditional theory of public administration. The implications of a public choice approach for studying the provision of public services has led a number of other scholars to pursue research on a wide variety of organizational conditions and settings (Baden and Stroup, 1972; Bish, 1971; Bish et al., 1975; Boschken, 1977; Hardin and Baden, 1977; Hawkins, 1976; Loveman, 1976; Martin and Wagner, 1978; Niskanen and Levy,

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[18] Golembiewski contends that "some evidence suggests that a substantial take-off size is required for many diversified educational programs. . ." (1977: 1503–04). He seems to proceed on the assumption that diversity in service mix can be attained only through the comprehensive school. Special services can be supplied through specialized agencies serving any given area. Several agencies acting jointly as an alternative to one agency rendering comprehensive services. Which type yields the better service as measured by performance indicators is an empirical question worth investigating. My colleague Herbert Kiesling informs me that New York State relies upon special Boards of Cooperative Educational Services (BOCES) that supply supplementary services on a contractual basis so that small districts can procure diversified educational programs for their students.

[19] Comparison across different types of institutional arrangements for supplying equivalent services under comparable circumstances was, for example, inherent in the idea of using the TVA as a "yardstick" for measuring the performance of private electric utility companies. Private producers can also be used as a "yardstick" for measuring the performance of public producers.

1974; Oakerson, 1973; O'Brien, 1975; Rich, 1977; Sabetti, 1977; Sproule-Jones, 1973, 1975; Sproule-Jones and Hart, 1973; Thomson, 1977; Warren, 1964, 1966; and Weschler, 1968.) Indeed, since 1961, proposals for two-tier solutions have been advanced among the traditional advocates of metropolitan reform. But the usual rhetoric about "overlapping local units causing a confusing maze" indicates that the two-tier solution is a politically expedient one without being appropriately grounded in theory (C.E.D., 1970:10).

Golembiewski's New Centrism

In his discourse on centralization, decentralization, and chaotic localism[20] it is interesting to note that Golembiewski relies exclusively upon a language that turns upon the concept of a unitary centralization rather than on federalism or polycentricity. He simply ignores the concepts of federalism and polycentricity which I consider to be the central thrust of my own work. In doing so, he fails to see my interest in variety (1977:1500). Instead, he adopts the conceptual language of Woodrow Wilson and ignores the conceptual language of *The Federalist.* I prefer to associate myself with the work of Daniel Elazar (1971), Charles Lindblom (1965), Martin Landau (1973), and Aaron Wildavsky (1976) who treat federalism as fundamentally different from centralization and decentralization. There are mechanisms of partisan mutual adjustment, cooperation, and conflict resolution for noncentral coordination of relationships in federal systems that cannot properly be characterized as centralization and decentralization (V. Ostrom, 1976c). As Wildavsky puts it: "Federalism requires mutuality, not hierarchy, multiple rather than single causation, a sharing instead of a monopoly of power" (1976:95).

Golembiewski seems to associate himself with the "camp [that] maintains that effective centralization must precede effective decentralization" (1977:1489). If I read him correctly, he suggests that only after first attaining effective centralization is it possible to determine "the scale of component units" and define "a rationalized system of differentiated/integrated subsystems" (1977:1489). A rational social order apparently depends upon the exercise of monopoly power by an omniscient observer or body of omniscient observers who can assign people to their proper places and functions. The natives must be kept in their place, or chaotic localism will reign supreme.

Golembiewski approvingly quotes Fesler to the effect that "national legislation, *overriding local objections and implemented by national administrative action*" is necessary to (1) "democratize the selection of local officials," (2) "establish viable units of local government with the size, resources, and diversity of interests that are preconditions of effective local self-government," (3) "recruit and train skilled staff for local administration," (4) "minimize corruption and regularize fiscal practices," and (5) "provide grants from national revenue to help finance the impoverished communities" (1977:1501; my emphasis). The role of the states has apparently been eliminated in this new centrism. Democracy means that those who control national legislation and administration know what is good for the people. All legitimate interests can and must be defined in a national context.

National legislation is clearly appropriate for dealing with problems of racial and sexual discrimination and for dealing with a wide range of problems other than the organization of local government. Public choice theory provides no justification for encouraging "racial, sexual, and other forms of discrimination" as Baker presumably alleges and Golembiewski reiterates (1977:1506).

In Golembiewski's new centrism, national authorities have full competence to make constitutional decisions about the general structure of local government. National authorities are to control the allocation of power in society. He has forsaken the logically necessary conditions for the maintenance of a system of government where the conduct of officials can be limited by a system of enforceable constitutional law. He has taken us back to Hobbes' solution where those who exercise sovereign prerogatives at the center of government are the source of the law, are above the law, and cannot be held accountable to law. Sovereign authorities reign supreme and the prosperity of the people, to paraphrase Hobbes, depends upon their obedience and concord not upon their form of government.

The revolutionary intellectual development of Copernican proportions that occurred in America between 1776 and 1789 was the formulation of a theory of constitutional choice where it was possible for people to create, through processes of constitutional decision making, a system of government in which all officials and all persons exercise an authority that is subject to the effective limits of an enforceable system of constitutional law (V. Ostrom, 1976a, 1976b). This theory of constitutional choice can be reiterated to allow for numerous units of government and several levels of government. All are constrained by positive rules of constitutional law. All people share in multiple communities of interest with access to concurrent governments in a compound republic where no one government exercises a monopoly over the legitimate use of force in society. Citizens maintain open public realms where their freedom of speech, assembly, and voluntary actions cannot be impaired by those who would destroy local government to save it from the vulgar influences of people.

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[20] I am amused that Golembiewski does not even perceive the possibility that his typology might include something called "chaotic centralization." The problem has been commented upon many times where central decision makers are the source of continuing surprises and disruptions in society. Uganda might be identified as an extreme contemporary case.

I see no evidence in Golembiewski's "critique" that he has any better understanding of public choice theory than Woodrow Wilson had of the political theory expounded in *The Federalist*. Until Golembiewski learns the rudiments of economic reasoning used by Hobbes, Hume, Hamilton, Madison, and Tocqueville among others, he will understand neither public choice theory nor the theory of constitutional choice that was used to design the American political system. Instead he will use his lack of understanding to project false images as though these theories were nothing more than Rorschach ink blots.

Unfortunately, this is a problem that applies to all of us. The generality of the problem is indicated in a recent paper by Larry D. Spence where he writes:

> The political writings in the history of Western civilization have become a series of Rorschach ink blots on which contemporary political theorists can project their aspirations and their values (Spence, 1977:12).

I simply do not know how we can solve this dilemma of potential misunderstanding other than to assume that we are all fallible creatures who can only hope to correct false images through an effective dialogue with those with whom we disagree. When we comprehend one another's arguments in a way that is consistent with the author's meaning, we then have the possibility of formulating competing hypotheses rather than competing misstatements. Unless we can test competing hypotheses, we can never sort out the wheat from the chaff. Until we have grounds for discarding some ideas, all ideas will have equal merit. Political science will be an evolutionary accumulation of everything. I find that to be a dismal prospect. Instead, the task of doing political theory should be one of arraying arguments so that disciplined choices can be made from among contending arguments rightly understood and competing hypotheses properly tested. The criterion of error-correction should guide those choices. That is what it means to speak of a "discipline" of political science.

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Observations on "Doing Political Theory": A Rejoinder

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Although rejoinders to responses normally do not interest me, Ostrom's comments broaden the issues for analysis, while they highlight aspects of what my critique had to say about *The Intellectual Crisis.*

Two Preliminary Points

Let me draw early attention to two issues—one narrow, the other central and global. A definitional difference may help explain why Ostrom takes the absence of the word "federalism" in the critique as evidence of my desire to eliminate it, or worse, an argument recapped in the final section below. Ostrom urges distinguishing federalism and decentralization (e.g., p. 1521),[1] so as to distance his argument from the critique's discussion of an over-reliance on decentralization. I see a federal pattern as a combination of both centralized and decentralized modes, with rules for variously blending them.[2] The critique argues, in effect, that different problems may require greater or lesser blends of decentralized with centralized modes of governance and administration. Federalism provides a useful institutional framework for such blendings.

Now for a central problem. Many of Ostrom's usages set up straw-men, irritate more than inform, attribute extreme positions to me by remarking that I "apparently" or "seem to" take them, and use easy victory over such an extreme position as a kind of proof of its polar opposite. Ostrom raises all four concerns when he observes: "Golembiewski seems to assume that the human animal can directly perceive and know 'reality' as such (p. 1510). The nuances of that charge play variously on my mind, because I never even knew anyone who would take such a position, and now I "seem" to be one. However, the truism that we "know" only through our concepts, theories, perceptions, and so on, does not constitute a license to develop empirical theory based on whatever assumptions happen to strike one's fancy.[3]

Relatedly, Ostrom's intellectual agendas often elude me. Consider this example (p. 1513): "In using the assumption of self-interest, I would never suggest, for example, that human organization or institutions can be subject to 'fine-tuning' as Golembiewski does." Does that mean that organizations and institutions, in Ostrom's usage, are only "individuals in the aggregate" and hence—by definition—only individual self-interest is involved? If so, that contradicts much that we know about organizations, as well as Ostrom's observations (e.g., pp. 1511–12 and 1518 that structural variations can generate important differences in outcomes. Or does Ostrom intend only that self-interest saves "one from the error of assuming that humans can be perfect automata in the sense of being perfectly obedient servants in a bureaucracy" (p. 1513)? If so, many other constructs do that easy job—consider only the venerable notion of "informal organization" and its implied view of humans as essentially social beings—and without the problems attendant upon self-interest. Moreover, I would have expected Ostrom's self-interested individual to fine-tune organizations and institutions, perpetually and insistently. And if Ostrom does not believe that organizations and institutions "can be subject" to stimuli for adjustments to the needs and conditions of those affected, that undercuts the main thrust of *The Intellectual Crisis*—that is, "to advance and serve the interests of . . . individual persons."[4]

Three Central Perspectives

Beyond these preliminary concerns, Ostrom's response suggests that several points at issue can be sharpened from three perspectives.

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[1] All page references in the text of this rejoinder refer either to my critique or to Ostrom's response.

[2] This usage follows Peter F. Drucker, *Concept of the Corporation* (New York: Day, 1946 and 1972).

[3] Robert T. Golembiewski, Keith Billingsley, and Samuel Yeager, "Measuring Change and Persistence in Human Affairs," *Journal of Applied Behavioral Science,* 12 (June 1976), 133–57.

[4] Vincent Ostrom, *The Intellectual Crisis in American Public Administration* (University, Ala.: University of Alabama Press), p. 131.

Perspective 1. Some Points of Agreement Differently Viewed. How basic agreements get treated provides useful perspective on any discussion of differences. Let me develop four points briefly to illustrate Ostrom's style of argumentation.

First, Ostrom correctly notes (p. 1508) that the critique places emphasis on "public choice theory," while he urges that he derives at least as much intellectual guidance from Hobbes, Hume, and others (p. 1510). Ostrom expresses concern that the critique thus fails to do justice, but it merely follows his lead. That is, Ostrom acknowledges his initial reliance on public choice theory as *the* model for reformulating his "conceptual language to a point where I could effectively apply economic reasoning to problems of political organization" (p. 1509). And he points to his discovery of similar "language" and "a mode of reasoning" in various writers about politics (p. 1509). A fair reading of this intellectual indebtedness certainly places public choice theory strategically in Ostrom's thought, I conclude. Hence the critique in effect retraces Ostrom's path of intellectual development in analyzing the ideational basis for his reformulation, especially to draw attention to problems with its "mode of reasoning" or methodology, from which Ostrom can hardly allege immunity.

Second, both Ostrom and my critique (p. 1508) agree on the grave limitations of traditional organization theory, in different but complementary ways. Ostrom emphasizes incompatibilities between the theory and important parts of our political heritage; and my earlier intra-organizational emphasis established that incompatibilities exist between that theory and several tenets of a "Judeo-Christian ethic," often resulting in costly effects on productivity and quality of working life.[5]

Ostrom does not provide an effective alternative to the traditional theory of organization, however, a point that can be developed in terms of how and why the bureuacratic model gained and maintained acceptance. In sum, spiraling economic development fueled the reputation of the traditional theory for getting results; empirical research but slowly established the common negative effects of the bureaucratic model on output and satisfaction; few if any cogent structural alternatives existed, especially those generated in the public sector, until the last 20 or 30 years; and even when alternatives became relatively clear, no technology existed until recently for facilitating changeover from the traditional structure. Revealingly, analogs of bureaucracy have been with us at least since biblical days; and persuasive learning processes even today acquaint middle-class children at age eight or so with its rudiments which they apply consistently in choice situations.[6]

The tradition critical of bureaucracy[7] needed two key elements to give it real force: the development of a methodology for empirical inquiry, and a heightened sense of the value and ethical consequences of different modes of organizing.

The critique argues that *The Intellectual Crisis* neither provides nor supports these key elements. To review, Ostrom neglects extant empirical research relevant to alternative structural forms. Moreover, the methodology underlying his work is variously inconsistent with or unsupportive of empirical research. Notably, the method underlying Ostrom's work has the same character as that underlying the bureaucratic model—derivations from assumptions about nature and from preferred conditions, which assumptions and preferences can be seriously challenged when made explicit. Finally, problems concerning both methodology and values make it improbable that Ostrom's work will encourage, let alone generate, an "applied science" necessary to provide efficient guidance for bringing alternative structural arrangements to life.

I acknowledge the several publications cited by Ostrom (pp. 1518–20), some of which were available when the critique was submitted for review in 1975. Review of subsequent efforts does not encourage any change in my judgment as to the likelihood of *The Intellectual Crisis* inducing a tradition of cumulative empirical research. The section below on Ostrom's way of "doing theory" presents detail on the point.

Third—although the critique nowhere so notes, nor does its argument require the note—I happen to believe that our "federal system" provides a serviceable framework for governance. To paraphrase Churchill, it is the most faulty basic form, except for all the others devised by humankind. Ostrom comes to a very different conclusion about my beliefs for reasons I do not find convincing, as the last section of this rejoinder details. Here let me make a limited point only to reinforce what should be apparent: the critique can reject Ostrom's assumptions, methodology, and aspects of this argumentation while it need not reject federalism. Ostrom sees an identity between them, I suppose, even though linkages between political institutions and Ostrom's assumptions/reasoning often strike me as quite loose, even casual. In any case, arguments favoring the federal system can rest on bases very different from those Ostrom advances. Briefly, to illustrate, one can argue that *the* basic mode of expressing the human spirit—good or bad or indifferent—is through association, in "factions" or "groups" or whatever, whose bases can be kaleidoscopic. Hence a multi-tiered and polycentric system—with easy egress/access, and with some extensiveness and heterogeneity—is prescribed as a practical way of institutionalizing geographical and functional barriers to the spread of mischief via association, while also permitting (even encouraging) individual and especially sub-national experiments which will help distinguish humbug from the locally appropriate as well as from the systemically useful or necessary. The contrasts with Ostrom should be patent: factions as well as individuals, eclectic and self-interested decision-making, collective attitudes or values as counter-weights to individual preferences, multiple social testing as constraints on individualistic choice, and so on.

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[5] Robert T. Golembiewski, *Men, Management, and Morality* (New York: McGraw Hill, 1965).

[6] Herbert Wilcox, "The Culture Trait of Hierarchy in Middle Class Children," *Public Administration Review*, 28 (May 1968), 222–35.

[7] See critique, fn. 51, for a brief list of a half-century of major criticisms of the traditional theory. Most attention was fixated on supporting what became the "imperial presidency," although there were periodic bursts of attention to "grass roots administration" and experiments such as TVA.

Fourth, Ostrom's response unequivocably (e.g., fn. 1) urges the importance of values, and the critique could not agree more. Any prescriptive organization theory must be rooted in specific values. And so must "efficiency," "freedon," and a whole panoply of similar notions.

But problems remain. Consider (1) that Ostrom's response cites the need for a value-loaded political science; (2) that he uses the term in *The Intellectual Crisis;* and (3) that "value" is found often in his response. I follow Ostrom's usages; and I believe the critique is clear enough about values. Yet he pleads inability to "unravel" (p. 1516) what I mean by "values," and in that way dismisses a number of significant points. More troublesome still, the public choice literature is shown by the critique to have serious problems associated with values, and Ostrom at once ties his work to that theory while his response also seeks to distance itself from public choice theory in that fundamental regard. Yet again in his response, Ostrom notes (p. 1514): "In doing positive theory it is necessary to handle the value problem by stipulation." This is precisely the general point raised by the critique—which discusses the inadequacy of the implicit "stipulation" underlying public choice theory—and does not square with the crucial role of values Ostrom acknowledges in his response.

Finally, my concern on the value issue is heightened by Ostrom's way of dealing with value-loaded issues, as in his expressed puzzlement about this question: Efficiency for what? Ostrom proposes that "Consistent application of the criterion of efficiency *rightly understood* will enhance human welfare, assuming that individuals are presumed to be the best judges of their own interests" (my emphasis, p. 1516). Then Ostrom adds: "But this is a simple tautology" (p. 1516). Precisely, Ostrom himself correctly identifies the character of his argument. My comments about values are directed to the profound issues involved in what "efficiency rightly understood" means in collective settings.

Perspective 2: Some Basic Disagreements About "Doing Theory." Ostrom's intent to provide a comprehensive paradigm—and his way of "doing theory"—also play prominent roles in his response. They shed light on some issues in serious contention that go beyond the scope of the critique.

Three points sketch my position as to Ostrom's paradigmatic goal. First, no firm basis exists for comprehensive paradigms in the social sciences. The 1960s temporarily forecast a brave new world in this particular, but even Kuhn—who inspired so many, including Ostrom—observes that: "It remains an open question what parts of social science have yet required such paradigms at all."[8] Paradigms are something to be worked toward, not posited and worked from, at our present level of development.

Second, Ostrom fails to distinguish between types of theory and their features. This increases my doubt that Ostrom's approach could resolve our paradigmatic problems, even if that were a feasible way to go today. Briefly, if Ostrom's work dealt with in the critique were consistently envisioned and presented as normative theory—as an expression of preferences and values—no major problems would exist. However, Ostrom presents his work in broader terms (p. 1510), as what I call a goal-based, empirical theory, that is, as a statement of how to approach some desired end-states via knowledge of empirical regularities. Such theory must be anchored both normatively in an explicit set of values, as well as empirically in a network of mid-level theories that relate tested concepts and operations suitable for approximating the critical dimensions of reality.

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[8] Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), p. 15.

Ostrom's approach *as goal-based, empirical theory* has serious difficulties at both anchoring points. Thus he does not deal specifically with his basic goals or values, except where he uses generic "preferences" as surrogates for specific values. This is awkward, especially because the number of goal-based, empirical theories is limited only by the alternative sets of goals or values that can be envisioned. Moroever, Ostrom leap-frogs to general theory, with hardly a mention of the available networks of mid-level theory relevant to organizations that touch on leadership, atmosphere or climate, specific structural properties, and so on through a very long list. Basically, Ostrom fills lacunae of knowledge with a "mode of reasoning" that emphasizes assumptions or stipulations. Both of these are required in any theoretical enterprise, as Ostrom notes (p. 1511). Just as clearly, too much can be made of even the most useful things. The critique argues that Ostrom does precisely this, in both empirical and normative senses.

This confusion of types of theory implies the indeterminancy of tests of the general hypotheses Ostrom associates with his theory. To illustrate: Ostrom's theory is not grounded in available empirical research, so detailed theoretical linkages remain unspecified, and this implies major trouble for the commendable empirical work (pp. 1518–20) listed by Ostrom—e.g., as to whether size of public-service agencies is related to user perceptions. The available literature indicates that "size," for example, is by itself not a very useful indicator. Thus "size" is neither conceptually nor operationally simple; and it may not be a critical variable, however conceived and measured.[9] Moreover, Ostrom's failure to specify numerous intervening variables or processes will very likely generate inconsistent and contradictory results in any hypothesis-testing, however extensive. Ostrom's global orientation and method cut him off from precisely such specificity on which goal-based, empirical theories must rest. Indeed, Ostrom's reaction (p. 1518) to the critique's comments about "size" implies that he is unaware of the available literature, its mixed results, and the complex linkages that must be specified before reasonably coherent predictions can be made.

In sum, Ostrom's lack of specificity undercuts a basic defense of his approach (e.g., pp. 1517–18) that he is developing theory which will generate hypotheses, whose test will imply the theory's adequacy. No such optimism seems appropriate. Paramountly, *the* issue is not the absence of empirical research inspired by Ostrom—which he says (p. 1517) is a "suggestion" in the critique. Rather, the straightforward emphasis is on the method and the theory encouraging the research, as well as on the degrees to which the theory is grounded in available empirical theory and to which the research specifies and tests intervening variables. Relatedly, Ostrom in concept ties "the problem of scale" to the "nature of goods," as Table 1 in the critique indicates. And Ostrom is clearly correct in noting (p. 1517) that his argument should not be taken to imply "that smaller is better," *except* where the "nature of goods" is identical or similar. The critique (pp. 1518–21) deals only with the latter case—although it is not as specific on that point as it should be—but it makes that simplification only because it relates to the literature cited by Ostrom which deals with comparisons of "similar" organizations providing "similar" goods. And here's the critical rub: in the absence of specificity about the "nature of goods" *and* organization characteristics, such comparisons of effects are treacherous. Police departments of similar scale, illustratively, are definitely not replicas in terms of their concepts of service, stewardship, and so on.[10] Moreover, what we know about organizations with even "identical missions" is that they can and do differ profoundly in terms of output and quality of working life.[11]

Third, Ostrom challenges (pp. 1517–18) that part of my obligation requires developing an alternative approach. I can comply, although not here: A forthcoming two-volume set attempts precisely that task, focusing on the theme *Public Administration as A Developing Discipline*.[12] And Ostrom's *The Intellectual*

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[9] See John R. Kimberly, "Organizational Size and the Structuralist Perspective: A Review, Critique, and Proposal," *Administrative Science Quarterly*, 21 (December, 1976), 571–97.

[10] James Q. Wilson, *Varieties of Police Behavior* (Cambridge, Mass.: Harvard University Press, 1968), esp. pp. 227–77, distinguishes three "styles" of "similar" departments, relating them to demographic, political, and historical differences.

[11] The major impact of variations in managerial style on output and satisfaction—other things being more or less equal—illustrates the present point. See James D. Thompson, "Authority and Power in 'Identical' Organizations," *American Journal of Sociology*, 62 (1956), 29–301.

[12] Robert T. Golembiewski, *Public Administration as A Developing Discipline* (New York: Marcel Dekker, 1977): Vol. 1: *Perspectives on Past and Present;* and Vol. 2: *Organization Development as One of A Future Family of Mini-paradigms.*

Crisis played a role in motivating that effort, for which I thank him.

Patently, the full argument of these two volumes can only be touched on here, but the books provide historical and empirical support for the basic proposition that the present condition of the field can be remedied by following guidelines prescribing work that:

- has both "pure" and "applied" aspects
- which as pure science is *ab initio* rooted in available empirical research, deals with specific processes and dynamics, and is directed by a methodology that tests increasingly comprehensive linkages of variables embodied in growing networks of theory
- which as applied science rests on an explicit set of values that prescribe the character of the "good organization"
- which has multiple analytic levels
- which includes a technology for facilitating efficient adoption of preferred structural variants
- which constitutes a reasonable "next bite" in coping with the issues of organization and governance.

In contrast to a "comprehensive paradigm," then, the two volumes array a range of thought and research regarding one mini-paradigm devoted to building experience and consciousness concerning both empirical and normative aspects of organizational analysis.

Perspective 3: "Golembiewski's New Centrism." Ostrom's argumentation in his response is nowhere better illustrated for me than when he assigns me paternity of a "new centrism," or hegemony over it. His rationale rests on my alleged self-association with those who propose the "effective centralization must precede effective decentralization."

Even acknowledging the honor of having a centrism named after me, I must decline, respectfully but nonetheless truthfully. Ostrom's generosity has no basis—either in my own words printed above, in quotations of others which I use approvingly, or in my other work.

Ostrom opens strongly in attributing to me the "new centrism," on which I am said to rely "exclusively" and whose goal is nothing less than to "keep the natives . . . in their place" (p. 1521). Then he goes on to observe that I only "seem to associate" myself with those centrists who stir his concern, that I only "suggest" their nefarious doctrine, and that we are all only "apparently" devoted to "monopoly power." And Ostrom also adds: "If I read him correctly. . ." (p. 1521).

Well, Ostrom does not read the critique correctly. For example, the paragraph at issue (critique, p. 1509). several times signals its interest only in "those supporting the traditional paradigm," "its proponents," and so on. Somehow, Ostrom not only enlists me in those ranks, but also puts me at the head of "Golembiewski's new centrism." This goes beyond guilt by association. You are what you describe, seems the operating rule.

Such business leaves me very cautious about Ostrom's posture toward analysis or argument, and four related points reinforce that concern. First, the critique strongly denies any preoccupation on my part with centrism (pp. 1508–09, and numerous other points through p. 1519, n.).

Second, the proposition that "effective centralization must precede effective decentralization" does not inspire my confidence. Effective centralization might have such an effect, or it might lead to heavy-handed autocracy. It all depends. If forced to bet, and given only one choice, I would give odds that effective centralization would result in a continued and probably heightened centralization. In my consulting experience, an *ineffective* centralization is most likely to inspire attempts to decentralize. Incidentally, the version of the critique originally submitted to the *Review* noted about that old chestnut of a proposition which Ostrom calls the "new centrism"—in a section that made the cutting-room floor, as they say—that "the proposition is not face-valid, of course."

Third, the bulk of my published work defies such attribution. In stark opposition, that research deals with the conceptual elaboration[13] and empirical testing[14] of specific alternatives to centralized modes of organizing, as well as with a technology[15] for developing attitudes, behaviors, and skills suitable for functioning in other-than-centralized modes.

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[13] At the intra-organization level, see my "Civil Service and Managing Work," *American Political Science Review,* 56 (December, 1962), 961–73. For a current effort to tailor organization policies to one common time-bounded set of individual needs, see Golembiewski, "Mid-Life Transition and Mid-Career Change," *Public Administration Review* (in press).

[14] For example, one recent line of research seeks to establish the empirical and value advantages of an apparently-simple policy change at work, implemented in accordance with a specific set of values. See Robert T. Golembiewski, Rick Hilles, and Munro Kagno, "A Longitudinal Study of Flexi-Time Effects," *Journal of Applied Behavioral Science,* 10 (December, 1974), 503–32; and Golembiewski and Hilles, "Flexi-Time in a Large Firm," *Monthly Labor Review,* 100 (December, 1976), 65–69.

Fourth, Ostrom does not use the exact text of the critique to make his point. On p. 1501, I quote Fesler's balanced words that "National legislation, overriding local objections and implemented by national administrative action, is often required to democratize the selection of local officials, to establish viable units of local government with the size, resources, and diversity of interests that are preconditions of effective local self-government," and so on through to "help finance the impoverished communities."

People of good will may differ, of course, about the precise degree and kind of penetration of the federal government into state and local affairs. But the principle seems pretty well established by now; and I would argue that central action *often* has been required—in economic development, in matters of racial justice, segregation, civil rights, organized crime, and so on through a long list. Ostrom might disagree, and we could debate the issues with zest. I would enjoy that. The argumentation took a very different turn, however. Thus Fesler's qualifier "is often required" becomes "is necessary," in Ostrom's telling, and this list of astounding conclusions is then made (p. 1521).

1. "The role of the states has apparently been eliminated."
2. "All legitimate interests can and must be defined in a national context."
3. "National authorities have full competence to make constitutional decisions about the general structure of local government."
4. "National authorities are to control the allocation of power in society."

Good grief.

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[15] E.g., Robert T. Golembiewski, *Renewing Organizations* (Itasca, Ill.: F. E. Peacock, 1972); and Robert T. Golembiewski and Alan Kiepper, "MARTA: Toward an Effective, Open Giant," *Public Administration Review*, 36 (January, 1976), 46–60.

Developing Public Policy Theory: Perspectives from Empirical Research*

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There has been considerable interest in the development of theories of public policy formation, but theoretical efforts to date have not demonstrated adequate recognition of the distinctive qualities of the dependent variable as a focus of research. Facets of public policy are far more difficult to study systematically than most other phenomena investigated empirically by political scientists. Our attempt to test hypotheses with some rigor demonstrated that public policy becomes troublesome as a research focus because of inherent complexity–specifically because of the temporal nature of the process, the multiplicity of participants and of policy provisions, and the contingent nature of theoretical effects. We use examples of policy making taken from the case study literature to show concretely how such complexity makes it essentially impossible to test apparently significant hypotheses as they are presented by Lowi, Dahl, Banfield, and others. Our effort here is to enhance theoretical development by carefully specifying and clarifying the major shortcomings and pointing out the apparent directions of remedy.

The emerging discipline of public policy studies is characterized by a growing disjunction between theory and research. While there are many provocative and potentially important theories, systematic empirical research to test them has largely been lacking.[1] Most importantly, there seems to have been little progress in the critical intermediate stage of refining and then operationalizing the important variables.

In the course of our own research–an attempt to employ aggregated case-study data in order to test quantitatively a number of important public policy hypotheses[2]–we experienced enormous difficulty at this intermediate stage. We attempted to translate leading hypotheses into operational variables suitable for employment in quantitative analysis. The difficulty we experienced did not lie in the question of whether the hypotheses were supported or not, but in whether they were testable or not. Although the theories seemed perfectly applicable to the few cases used by their authors to illustrate them originally, the propositions did not fit so neatly when applied to a number of examples not expressly chosen for explanation and illustration. As we attempted to deal with problems of operationalization in connection with a variety of theories and dozens of cases, we noted that many of the problems recurred. While the theories were plausible, they seemed to fall into certain traps that appear to be endemic to the systematic study of public policy. This led us to adopt the purpose of the present paper–to comment on

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[1] By "systematic empirical research" we mean the use of quantitative data gathered from a number of instances of policy making in order to test specific hypotheses. Examples of recent work in this direction are provided by Robert K. Yin and Douglas Yates, *Street-Level Governments*, R–1527–NSF (Santa Monica: Rand, 1974); William A. Lucas, *The Case Survey Method*, R–1515–RC (Santa Monica: Rand, 1974); and Robert K. Yin and Karen A. Heald, "Using the Case Survey Method to Analyze Policy Studies," *Administrative Science Quarterly*, 20 (September 1975), 371–81.

[2] For a description of that project, see our "Case Study Aggregation and Policy Theory," *Proceedings*, 1973 Annual Meeting of the American Political Science Association, New Orleans, La., September 4–8, 1973.

these peculiarities of public policy theory and describe these pitfalls, as an avenue toward both building better theory and illuminating some of the significant characteristics of public policy itself.

The plausible and provocative theories of public policy formation offered by Lowi, Banfield, Froman, Gamson, Dahl, and others, appear in general to be no more or less adequate than those in any area of political science. *But the kinds of phenomena that the theories seek to explain* are radically different from most of the other phenomena we study. It is this difference in the objects of analysis that is the source of the difficulty.

In brief, public policy as a focus of systematic comparative analysis is *more complex* than such phenomena as electoral votes, legislative roll calls, incidents of political violence, and elite ideologies. It is more complex on at least four counts, which we will elaborate and illustrate in the discussion to follow. These are:

1. The policy process takes place over time, sometimes over a long period of time. This leads to difficulty in explaining "the process" as a simple unit. Even if one attempts to explain specific outcomes, the explanatory forces invoked almost invariably involve characteristics of this long and shifting process. Two sorts of difficulty arise:

a.) As the process proceeds over time, it can involve a large number of decision points, e.g., the decision of a subcommittee chairman, a Senate roll call, a presidential compromise, and the decision of an appellate court. The contents of each of these outputs might be called "public policy" and might be predictable by public policy theory. But we do not want theories to be oriented toward or tested upon inconclusive or tentative decisions. Nor do we want them constructed so as to predict the characteristics of the rubber-stamping process. We want somehow to focus only on "significant" outputs.

b.) The idea of a predictive theory of public policy demands that the values of the predictors be determined at some beginning point. Such values, however, are likely to change with the unfolding of the process itself, their final status being achieved only at its termination. Many presumably predictive theories are thereby weakened substantially, and become, in final analysis, post hoc explanations.

2. Any given policy proposal, or "output," or "outcome"[3] is in itself complex; it may have several important aspects. This multiplicity can make the whole policy extremely difficult to place in any single category, as is demanded, for example, by the categorization schemes that currently abound in public policy theory.

3. As a focus of analysis, policy making is complicated by the presence of a large number of participants. When a characteristic of the participants becomes a variable of interest, as it often does, variation among participants with regard to that characteristic causes difficulty. The difficulty takes two forms:

a.) Subjective. The state of the world as perceived by participants yields many important policy-analytic variables. But perceptions vary considerably, of course, depending upon the participant consulted or described.

b.) Objective. Still more variables are generated in existing theory by "objectively" determined participant characteristics—as determined, that is, by the researcher, interviewer, casewriter, or other outside observer. Ambiguity is introduced when the heterogeneous group of all participants, or heterogeneous subcollections of participants, must be assigned a single score on such a characteristic (e.g., level of involvement, or point of access to decision makers).

4. Lastly, public policy as a research focus is complex because the process cannot be described by simple additive models. On the contrary, the forces interact; the impact of one depends in large measure upon the value of another.

In sum, "public policy" is almost never a single, discrete, unitary phenomenon. Indeed, the appeal of public policy studies as a focus of intellectual endeavor lies precisely in its richness; the complexity of the unit of analysis simply and appropriately reflects the fact that an action of government is rarely meaningful if conceived of as a discrete, disembodied event, and that the impacts of a single government action on society are not understood properly if taken in isolation from one another.

In this essay, then, we seek to specify in some detail the kinds of difficulties these characteristics create, illustrating them by exploring the problems arising from attempts to apply specific theoretical propositions to concrete events taken from the case-study literature. We hope that our conclusions will not only provide some assistance to developers of public policy theory who are concerned to avoid problems of conceptualization and operational definition, but will also highlight certain important characterisitcs of public policy itself that have heretofore been inadequately understood or explained.

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[3] Our use of the terms "output" and "outcome" follows Ranney's definition of those terms. See his "The Study of Policy Content," in *Political Science and Public Policy*, ed. Austin Ranney (Chicago: Markheim, 1968), pp. 8–9.

An Initial Illustration

Many of the problems arising from the complexity of using "policy" as a unit of analysis can be illustrated by the work of Theodore Lowi. No single theoretical construct has been more important to the development of public policy studies than Lowi's categorization scheme,[4] yet the way in which he defines his fundamental terms is seriously weakened by the problems inherent in the unit of analysis, which make his hypotheses almost impossible to operationalize meaningfully in order to test them empirically. Since these problems of operationalization bear upon several of the challenges inherent in developing public policy theory, and since Lowi's idea is so well known and so well received, we refer to his work by way of general introduction, to be recalled briefly at several points in the more schematic discussion to follow.

The heart of Lowi's argument is that "policies determine politics." He developed, in a series of articles, a typology containing "regulatory," "distributive," "redistributive," and (subsequently) "constituency" policies and argued that policy processes will differ significantly depending on the policy type involved. As a result of those differences in process, he suggests, relationships among important concepts or variables that may be quite strong when policies of one kind are involved may be much weaker or even totally absent when other types of policies are concerned. Instead of attempting to find relationships that hold across the entire range of public policy, he argues that one should focus one's investigation within one of the four policy classifications.

Lowi generates a number of important hypotheses from his typology. For example, he predicts that congressional committees will be able to retain control of the process of coalition building on distributive amendments and that, therefore, few amendments will be offered to committee bills on the floor.[5] He also predicts that peak associations can be expected to be more cohesive when confronted with redistributive issues that unite their memberships than when they are faced with regulatory issues that divide them.[6]

The explicitness of the predictions Lowi derives from his policy classification is not matched, however, by a similar explicitness in explaining how one can determine the correct policy type for a given policy. Since the correct determination of policy type is obviously central to the predictive ability of Lowi's theory, this limitation has crucial importance. Lowi contends that distributive decisions are made without reference to their implications for other decisions; that regulatory decisions imply a direct choice "as to who will be indulged and who will be deprived"; and that redistributive decisions involve the greatest interconnection, since they imply choices among broad classes of individuals.[7] But the "decisions" of which Lowi is speaking are not simple and discrete; they are *policy* decisions. Many can be expected to contain at least some characteristics of each of Lowi's types, and there is little guidance for determining how a policy is to be classified in any but the simplest cases.

The problem of classifying a policy correctly thus becomes a central stumbling block to the empirical testing of Lowi's hypotheses. For instance, policies often have distributive programmatic characteristics but redistributive or regulatory financing mechanisms, as the case of the Chicago Transit Authority aptly illustrates. The CTA approached the state of Illinois for subsidization—a handout, precisely in the distributive tradition of the pork barrel and the pre-1960s tariff. The matter might easily have been processed and settled as a distributive issue, but the method for financing the subsidy became controversial. The proposal was eventually defeated, largely because of the dispute on regulatory or redistributive concerns—the question of whose particular pockets the money was to come from.[8] More generally, for *any* policy lacking explicit income transfers, there is some financing method either built in or implied that may or may not fall into the same category (in Lowi's typology) as the policy's other characteristics.

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[4] We refer, of course, to the ideas presented in the series of three articles: "American Business, Public Policy, Case-Studies and Political Theory," *World Politics*, 16 (July 1964), 677–715; "Decision Making vs. Policy Making: Toward an Antidote for Technocracy," *Public Administration Review*, 30 (May-June 1970), 314–25; "Four Systems of Policy, Politics, and Choice," *Public Administration Review*, 32 (July-August 1972), 298–310.

[5] This is why Lowi found the '60s tariff to be so different. In earlier times, tariff policy was distributive, but "The true nature of tariff in the 1960s emerges as regulatory policy. . . . Issues that could not be thrashed out through the 'group process' also could not be thrashed out in committee but had to pass on to Congress and the floor," Lowi, "American Business," p. 701. Lowi presented data on amendments to bills in Congress from the floor in "Decision Making vs. Policy Making," pp. 321–22.

[6] "If there is ever any cohesion within the peak associations, it occurs on redistributive issues, and their rhetoric suggests that they occupy themselves most of the time with these" (Lowi, "American Business," p. 707).

[7] Ibid., pp. 690–91.

James Q. Wilson discusses this difficulty at some length. He maintains that a single policy may have aspects of each policy type, and he offers several examples. "A bill barring discrimination in public accommodations," for instance:

> could be seen as a measure regulating the use of hotels and restaurants or as one redistributing a benefit (access to hotels and restaurants) from one social stratum to another.... Urban renewal programs regulate the use of land, redistribute the housing supply, and distribute benefits to certain contractors and labor unions. Monetary and fiscal policy has both regulatory and redistributionist implications depending on whether one thinks of it as simply controlling the interest rate or as benefiting creditors at the expense of debtors (or vice versa).[9]

Lowi never specifically addresses the problem of how to classify a policy correctly when it has attributes of more than one of his policy types, but one can infer from the general tone of his articles that he does not expect classification to present a major problem. His basic argument in this respect appears to be that expectations based on past experience with similar issues objectively structure policy choices for the entire policy process. Thus he might well respond to Wilson by contending that one particular aspect of any of the policies Wilson cites can be expected to dominate the expectations of the actors in the process, so that there will be no problem in making the classification. Hotel and restaurant accommodations policy, for example, would undoubtedly evoke regulatory expectations from virtually all those connected with the policy process, based on their past experience and the past debate on similar issues.

That answer, however, is adequate only when expectations are virtually unanimous. Yet the policy process is as likely to be characterized by multiple perceptions as policies are likely to be characterized by multiple attributes. Whenever there are any significant disagreements among perceptions of a policy, the problem in classification according to Lowi's typology has simply shifted from one of determining "which aspect" to one of determining "whose perceptions." In the CTA example, it would be quite difficult to decide whose views should predominate—those concerned with mode of financing or those concerned with the costs and benefits of the transit service. Moreover, as Wilson notes, the problem becomes even more difficult in the cases of new or innovative policy, where there is little relevant past experience to structure the perceptions of any of the participants.[10]

Variations in perception of a policy are especially likely to occur when participants in the process actively seek to redefine the issue. Lowi himself suggests that "one of the important strategies in any controversial issue is to attempt to define it in redistributive terms in order to broaden the base of opposition or support."[11] In classifying a policy in which this strategy has been employed, Lowi's method will work only if the strategy has been almost a complete success (almost all perceptions have been redefined) or almost a complete failure (almost no perceptions have been redefined). If the strategy succeeds in altering some perceptions but not all, one has no guidance for deciding which perceptions will provide the basis for classification.

Take the controversy over Medicare.[12] Initially, since the proposal called for an additional tax on workers to provide benefits for the elderly, one would probably classify the policy as redistributive. The AMA, however, attempted to redefine the issue as one of government regulation of physicians and the practice of medicine. The AMA's argument undoubtedly influenced the perceptions of at least some individuals on the issue, but it certainly did not succeed in restructuring the perceptions of all. There is simply no objective way to determine which set of perceptions should be dominant in classifying a policy issue when there is substantial disagreement among the participants themselves about what is at stake.

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[8] Edward C. Banfield, "The Chicago Transit Authority," in *Political Influence* (New York: Free Press, 1961), pp. 91–125.

[9] James Q. Wilson, *Political Organizations* (New York: Basic Books, 1973), p. 329.

[10] Ibid., p. 339.

[11] Lowi, "American Business," p. 707, footnote 28.

[12] Theodore R. Marmor, "The Congress: Medicare Politics and Policy," in *American Political Institutions and Public Policy*, ed. Allan P. Sindler (Boston: Little, Brown, 1969), pp. 3–68.

The problems in operationalizing Lowi's hypotheses illustrate the special difficulties of theory construction and testing in the field of public policy studies. His provocative ideas cannot be meaningfully operationalized without considerable effort by the researcher to add greater specificity and precision, a process in which the researcher must often, without adequate guidance, make important assumptions about what the theory is really trying to say. As it stands, Lowi's theory is not testable because the basic concepts are not operationalizable; in order to operationalize them, researchers must make a number of guesses and assumptions that create a situation where they can no longer be sure just whose theory they are testing.

The discussion of Lowi's paradigm illustrates the obstacles one encounters in attempting to apply policy theory across many cases, each of which is a full-scale instance of the operation of a policy-making system. We will now consider somewhat more explicitly the forms of complexity enumerated earlier, after which we will conclude with a brief listing of implications.

Problems of Temporality

Multiple Outputs. Several important hypotheses become mired in ambiguity for want of a time-stopping criterion. A whole host of variables is difficult to operationalize without being arbitrary until some decision is reached as to when, over a protracted period of development and struggle, "policy is made." For example, many of the most important hypotheses attempt to predict, as a dependent variable, the success or effectiveness of employing various resources or strategies in influencing outputs. Both Gamson and Dahl have discussed this problem at length, and both suggest measuring success by comparing the output of the process with the intentions of the relevant actors.[13] The output, however, keeps changing! There are simple cases, to be sure, such as those studied in Gamson's research on fluoridation, but there are complex cases as well. In fluoridation, points of beginning and ending were generally clear; someone at some time initiated the controversy with a proposal to fluoridate the water and policy was "made" when a community voted. More frequently, though, policy making consists of an ongoing process in which beginning and ending states are unclear, and in which both outputs and intentions are continually modified.

Policy struggles are usually preceded by a background period during which intentions and opinions are being formed. Later, outputs are issued, modified, and remodified as administrators respond to continuing pressures or as losers initiate appeals to higher authorities. At times, reviews by higher authorities create real opportunities to change results; at other times the decision is merely rubber-stamped. At what points are researchers to slice into the ongoing policy process to measure the relevant intentions and outputs? How many and what sorts of appeals are to be considered before a decision process is regarded as complete? These questions are important because the measurement of such variables as the number of participants, the duration of conflict, the resources employed, etc., will be significantly altered depending on the time period over which they are measured.

An illustration can help clarify some of these difficulties. In the case study of "The Glavis-Ballinger Dispute,"[14] the controversy progressed through several stages. At stake was the validity of the Cunningham claim to large tracts of public land in Alaska. At first there was an administrative determination that the claim was valid. Then Glavis, an official in the Bureau of Land Management, began a routine investigation into the validity of the claim, ultimately recommending that the land not be turned over to Cunningham. Secretary of the Interior Ballinger overruled Glavis. Glavis then appealed to President Taft, who supported Ballinger. But Forest Service Director Gifford Pinchot then helped Glavis publicize the issue and take his case to the Congress, and ultimately a law was passed which prohibited the sale of public lands in the future, but which still did not resolve the Cunningham dispute. Several years later a federal court found against Cunningham.

It may seem obvious that the court decision decided the case, but is this the most important point to examine? And what if the court had found *for* Cunningham? The determinative decision might then be considered to have been either that of Ballinger, or Taft, or Congress, or the court, with all of the subsequent decisions essentially ratifying the first. Similarly, one might argue that the case began at any of several different points: congressional authorization of the sale of land; Cunningham's violation of the law in amassing his claim; Glavis's investigation; or the rejection of Glavis's recommendations. Depending upon the points chosen, a number of important variables, such as the degree of conflict in the dispute and the number of participants, would vary.

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[13] William A. Gamson, *Power and Discontent* (Homewood, Ill.: Dorsey, 1968), p. 71; Robert A. Dahl, *Modern Political Analysis* (Englewood Cliffs, N.J.: Prentice-Hall, 1963), pp. 39–54.

[14] [Winifred McCulloch,] "The Glavis-Ballinger Dispute," in *Public Administration and Policy Development*, ed. Harold Stein (New York: Harcourt, 1952), pp. 77–87.

Treatment of this particular quandary not only affects decisions about the analysis of an individual case, such as the Glavis-Ballinger dispute, but might also artificially predetermine certain results of a quantitative analysis. Defining the duration of policy conflicts in such a way as to be as inclusive as possible, for instance, might well yield the finding that most important administrative decisions are determined in the courts, while in fact one suspects that there are many appeals in which there is no real hope of overruling an administrative determination—appeals that are taken largely pro forma in order to symbolically satisfy aggrieved interest groups. Another artificial result of inclusiveness would, no doubt, be the finding that congressional committees hardly ever have the final say about anything—although in fact they make an enormous amount of policy—simply because their decisions are routinely passed upon by the full chamber, conference committees, and the president. On the other hand, defining the time parameters of the process too narrowly—by attempting, say, to single out each discrete decision—would force the systematic analyst to consider thousands of decisions, jamming any data set with useless, unimportant, and misleading information.

It is thus essential, both when policy hypotheses are proposed and when they are tested, to have in mind some reasonable criteria for demarcating the process temporally. The reader and writer must agree somehow on the outputs that are theoretically crucial, otherwise doubt and confusion must inevitably arise regarding the consistency of theory and data.

Postdictive Theory. Another complication of temporality arises from the annoying tendency of variables not to stand still as the process unfolds. The duration, scope, or complexity of a given policy struggle are rarely strictly determined at the outset; not only are outcomes often unpredictable, but so too is the process. Thus predictive hypotheses that fail to account for this contingency often become extremely difficult to apply to concrete cases and are often best tested—and thus rendered most meaningful—by being converted into postdictive hypotheses. But that change may seriously dilute their significance.

An example is provided by the concept of "requisite actions" suggested by Banfield in *Political Influence*. Banfield introduces the concept as a way of explaining why some policy proposals are adopted and others are not. He employs the following definitions:

> Performance of a specified set of actions by specified actors, or by a specified number of, or proportion of, the actors who constitute a specified group, constitutes *adoption*, i.e., adoption is defined as the performance by these actors of these actions, which will be called *requisite* actions. . . . An actor who can perform a requisite action has *authority* over the action. He may perform it or not as he likes, or, in the language to be used here, he may *give* or *withhold* it from the system of activity being concerted toward adoption of the proposal.[15]

In any situation, certain actors will be controlled while others are autonomous—that is, free to withhold the performance of requisite actions. In highly centralized systems, there are few autonomous actors; in highly decentralized systems there are many. Adoption of proposals in a highly decentralized system is uncertain, according to Banfield, because it cannot be predicted whether or not autonomous actors will perform the requisite actions.

Banfield goes on to derive a number of interesting and nontrivial hypotheses from his argument. For example, he argues that, "as the number of autonomous actors in a situation increases, the probability of adoptions decreases,"[16] and that "corruption will tend to increase as the distribution of authority widens."[17] Banfield believes that these derived propositions can be tested: "If when tested—and some of them cannot be tested by any data in this volume—these derived hypotheses prove false, doubt will be cast upon the factual premises from which they were deduced."[18] But in order to test Banfield's model, it is necessary to identify and count the number of requisite actions that must be concerted in order for a policy to be adopted. And while Banfield asserts that it is uncertain that any given requisite action will be performed, it must be possible, in order for his theory to have predictive value, to at least specify what the requisite actions are before the policy struggle begins.

The number of requisite actions may, however, change dramatically during the course of a policy struggle, as controversy heats up; and it may become clear that some actions were in fact requisite actions only after the policy struggle is over. Take the case, *Defending "The Hill" Against the Metal Houses*, as an example.[19] A developer wished to erect a questionable type of housing in a lower-middle-class neighborhood. Given the legal requirements of the jurisdiction, certain zoning and building clearances would undoubtedly have to be obtained. Are there other requisite actions? How long and harsh will the decision be in the making? Can we predict the probability of adoption from our initial count of the requisite actions?

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[15] Banfield, p. 309.

[16] Ibid., p. 318.

[17] Ibid., p. 322.

[18] Ibid., p. 308.

If we are to count the neighborhood residents as "an autonomous actor," the struggle could be protracted, but the residents of such neighborhoods typically do not organize to make their wishes known. Thus, predictively, the number of requisite actions would seem to be, say, two or three. As the particular case turned out, however, the residents of the neighborhood did organize, and as a result—though not even then a clearly predictable result—a substantial number of aldermen, the mayor, the city's corporation counsel, the state board of health, and the opposition candidate for mayor all eventually became autonomous actors as well. The developer was defeated, but only after a long and emotional struggle.

The specific dynamics of the policy process, in this case at least, thus determined not only whether or not significant actors would give or withhold requisite actions, but how many requisite actions there would have to be. This is not to say that Banfield's theory is not correct as a post hoc generalization about the extent to which two interesting attributes of the policy process covary; but the implied causality in the original hypothesis becomes impossible to test empirically.

It is now clear that Lowi's central hypothesis, that characteristics of the process can be predicted from characteristics of an input (policy type) is fettered by this same difficulty. The policy type and perceptions of it by relevant actors often are determined well after the process has begun. Of course, one might predict new characteristics of the process continuously as a policy changes or is perceived to change from distributive to regulatory, regulatory back to distributive, and so forth, but this complicates the job of comparative analysis to the point of impracticability.

The Problem of Multiplicity of Policy Aspects

A public policy is only rarely the result of a simple binary decision, or even a chain of such decisions. Even the simplest government policy is likely to spring from a complex chain of causes and relationships and to have a set of subtly interrelated consequences for the general social network. Unless there is adequate awareness of this complexity, and adequate precautions are taken to cope with it, even those theories of public policy which seem most sensible are likely to evaporate on close inspection into a cloud of ambiguity. We have already discussed at some length the problems of complexity arising from the temporal nature of policy making. The point of this and succeeding sections is that policy research would be complex even if the process were instantaneous.

Lewis Froman, for instance, hypothesizes that homogeneous communities will adopt "areal" policies, while heterogeneous communities will adopt "segmental" policies. He defines areal policies as those which affect the total population of a city simultaneously by a single action, and segmental policies as continuing policies which affect different people at different times in separate sections of the city.[20] In his segmental category, Froman thus seems to take cognizance of the fact that the same policy can have different impacts in different places over time. Yet that awareness is of no help to a researcher trying to fit a policy such as, for example, "promoting industrial development," into Froman's categories. Industry may locate in a carefully zoned, narrowly circumscribed area, but (1) its pollution may affect surrounding neighborhoods; (2) it may provide jobs to residents of a much larger area; and (3) its property taxes may pay for services uniformly consumed throughout the whole jurisdiction. There is *no* a priori way for the researcher to determine which of its impacts are to be considered in deciding whether the policy is "areal" or "segmental."

The same problem is illustrated by Alan Altshuler's description of an intercity freeway dispute: should the freeway be built along a southerly route through a black neighborhood

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[19] William K. Muir, Jr., *Defending "The Hill" Against the Metal Houses*, ICP Case #26 (University, Ala.: University of Alabama Press, 1955).

[20] Lewis A. Froman, Jr., "An Analysis of Public Policy in Cities," *Journal of Politics*, 29 (February 1967), 94–108.

or along a railroad line to the north?[21] The route through the black neighborhood was "scientifically chosen" for the shortest and best traffic patterns. The benefits from that route certainly appeared to be areal. The highway would be used by virtually everyone, and even those who didn't use it would benefit from the generally improved transportation and commerce in the area. More importantly, state and federal assistance was available for that route, lowering the tax burden for everyone in choosing it. On the other hand, the costs of the decision were quite clearly segmental. Many families were displaced from the area immediately surrounding the construction, with little or no counseling or provision for replacement housing. The displaced residents moved primarily into nearby buildings, seriously increasing already severe congestion and suffering the noise and pollution consequences of their continued proximity to the freeway. From the point of view of the winners, then, the freeway location decision was areal; from the point of view of the losers, it was segmental. From the point of view of a researcher trying to assign the policy to one of Froman's types in order to test his hypotheses, the decision was not easily classifiable.

The difficulty we encountered above in treating Lowi's stimulating hypothesis in its illustrative application to the subsidy for the Chicago Transit Authority also fits into this category. The policy proposal took on at least two separate and salient aspects, service and financing, which made difficult its classification into one and only one category of Lowi's policy typology.

The Problem of Multiple Participants

Even if the policy-making process were both instantaneous and unitary, it would still be complex as a focus of analysis because of the multiplicity of participants involved and the towering importance of participant characteristics, both subjective and objective, as elements of policy theory.

Subjective. The perceptions of relevant actors are important determinative variables in many theories about public policy, but policy theories rarely specify whose perceptions are to be taken into account. An illustration of the necessity of specifying from whose point of view a concept is to be defined is provided by hypotheses in which the status quo is an important concept. Gamson, for example, argues that it takes fewer resources to defend the status quo successfully than to bring about change.[22] While the status quo seems to be an objective characteristic of a state of the policy process, it may often be defined only in terms of participants' perceptions, which may in fact differ substantially from one another. Take the following example: in the case study, *Shooting Down the Nuclear Plane,*[23] all agreed that the specific purpose of an existing $75 million appropriation was to carry out a small development program. From the Air Force's perspective, however, that appropriation was only a first step and constituted a commitment to the actual construction of a nuclear plane. To the Air Force, it was that commitment which defined the status quo. Failure to expand the program and build the plane would be a negation of the commitment and thus a serious departure from the status quo. From the perspective of the members of the congressional appropriations committees, on the other hand, the small development program represented only a limited venture; from their point of view, confining the future program to reactor development, for example, would represent no change whatever in the status quo. In a situation of this sort, how is one to evaluate Gamson's hypothesis? Each protagonist, the Air Force and the Congress, thought that it alone was defending the status quo and that the other was opposing it; there is no objective criterion on which the outside observer can base a decision as to whose perception was correct. It might well be considered sound to decree in this case that the most relevant perception of the status quo is that of the Congress. Its perception anchors the concept of "change" in Gamson's hypothesis; *Congress* must be changed. The generalization to be recognized, however, is that such a determination must be made on a priori theoretical grounds for each variable and each hypothesis subject to this kind of ambiguity in research.

Again, the example helps to pinpoint a problem noted in our earlier discussion of Lowi's typology. Lowi argues explicitly that the perceptions of actors determine the category into which a given policy must be classified, but he does not treat theoretically the question of whose perceptions must be dominant in the event that there are differences.

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[21] Alan Altshuler, *Locating the Intercity Freeway,* ICP Case #88 (New York: Bobbs-Merrill, 1965).

[22] Gamson, p. 63.

[23] W. Henry Lambright, *Shooting Down the Nuclear Plane,* ICP Case #104 (New York: Bobbs-Merrill, 1967).

Objective. In many hypotheses, the independent variable seeks to describe a subjective characteristic of participants–that is, a state of the world as seen through a particular lens. When theorists fail to specify adequately whose subjectivity is to be measured, the problem described in the previous section arises. Other hypotheses, however, seek to link *objective* characteristics of participants–their resources, explicit attitudes, or demographic characteristics–to aspects of process or output. But those hypotheses are of little help in determining *which* participants the characteristics describe. It is useful to keep subjective and objective characteristics of participants distinct. Hypotheses regarding the former often are deceptively simple, because they refer explicitly to impersonal conditions (e.g., the status quo, the divisibility of the benefits), which in reality are subjective perceptions of conditions; whereas the latter tend to refer quite obviously to people (e.g., resources used, strategy used). Both, of course, are troublesome for the same basic reasons–the multiplicity and heterogeneity of participants.

In *Democracy in the United States,* Dahl suggests that, "how severe a conflict is depends on how much is at stake."[24] Empirically, we might estimate how much is at stake ourselves as outside observers, or we might depend on the relevant actors' own views of what is to be lost or gained. Even if we elect the former, "objective" course, however, so that perceptions of actors are not relevant, we are still left with the unanswered question: What is at stake *for whom?* Variation is possible here, just as it is for factors such as social class or resources employed. The following concrete instance illustrates how answering that question may be critical to an empirical test of Dahl's hypothesis.

The issue in the case of *The Florida Milk Commission Changes Minimum Prices* was a proposal for the complete deregulation of milk prices, replacing a system of controls at all levels from the dairymen to the consumer.[25] Three different groups were primarily affected. Consumers could be expected to benefit in the short run from increased competition within the milk industry. The "big three" dairies would be hurt in the short run, since they would have to lower prices and curtail profits. Most seriously affected of all, however, would be the independent distributors, who would not be able to compete with the "big three" and might eventually be forced out of business altogether, if past history in the market area were any guide. In the long run, the "big three" would then benefit from the reinstitution of an oligopolistic market in which they each had a larger market share. Conversely, consumers could expect the long-run outcome to be neutral at best, and possibly negative.

Dahl's hypothesis could thus be applied to this case in at least three different ways. If one looked primarily at the consumers, one would predict very little conflict; for them, the stakes were quite small. For the "big three," the stakes were large but not overwhelming. For the independents, the issue was virtually one of life or death, since they could not compete in either buying power or production efficiency if prices were deregulated. The choice of which group to focus on is thus crucial. If one takes the party with the most at stake, one would expect the conflict in this case to be quite severe; if the party with the least at stake were used, very little conflict would be expected. To take an average would similarly be to predict a relatively low or at most a moderate level of conflict. More to the point–unless one attaches a way of surmounting the problem to the original formulation of the hypothesis–the theory cannot be tested. It remains, in an important sense, incomplete.

The Problem of Interaction

Interaction among independent variables in determining an outcome is of course common in social theory and in research findings. By "interaction" we mean that the existence or strength of an effect is contingent upon some other condition or the value of some additional variable; for example, the effect of financial resources upon success in a policy struggle may depend upon the arena of decision–legislative, executive, or judicial. The phenomenon is quite obviously not peculiar to public policy. We suggest, however, as the complexity emphasized repeatedly in the foregoing discussion would indicate, that interaction is endemic to public policy–it is perhaps its most salient characteristic–yet, it is rarely recognized in theoretical offerings.

Many important theories about public policy are probably incorrect as generalizations encompassing everything that falls within *anyone's* image of policy making, while essentially accurate for that subset of examples which theorists implicitly take as their *own* definition of the universe of applicable instances. The failure to demarcate that universe is potentially a failure to recognize important statistical interaction. We will offer a specific illustration in a moment, but we note in passing that the general definition of "policy" itself may be a contingent condition upon which the applicability of a theory is meant to depend. There exists in the literature a rather astounding number and variety of suggested boundaries (or lack thereof) about the concept, "policy": all government action;[26] a program of goals, values, and practices;[27] the impacts of government activity;[28] general rules to subsume future behavioral instances;[29] the consequences of action and inaction;[30] important government decisions;[31] and "a particular object or set of objects which are intended to be affected . . . [together with] a desired course of events . . . a selected line of action . . . a declaration of intent . . . and an implementation of intent. . . ."[32] It is perhaps too much to ask at this stage that we all agree on our usage of the term "policy," but in the absence of such agreement, it is well to recognize that hypotheses might receive more or less support if tested on all government decisions and actions or on some one of the many alternative subsets referred to as "policy" in current theoretical writings.

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[24] Robert A. Dahl, *Democracy in the United States,* 2nd ed. (Chicago: Rand-McNally, 1972), p. 303.

[25] Harmon Zeigler, *The Florida Milk Commission Changes Minimum Prices,* ICP Case #77 (University, Ala.: University of Alabama Press, 1963).

To illustrate the problem of unspecified interaction more specifically, we will consider hypotheses about group size as a political resource. Pluralists in general postulate that group size is an important potential resource. Murray Edelman, on the other hand, argues that large groups are more likely to be bought off with symbolic reassurances than small, well-organized groups.[33] E. E. Schattschneider argued still a third position: that group size may be of relatively minor significance because the relevant group may be able to involve wider publics or disinterested government officials in a dispute.[34] It is easy to think of examples to illustrate the persuasiveness of each of these conflicting hypotheses, and, indeed, each author provides several. None, however, attempts to place his propositions in the context of more general theory by specifying the conditions under which they are valid. One is thus left with three opposing theories, all of which may very well be valid for a broad class of decisions, but with no clue as to when they are valid and when they are not, and why.

By way of contrast, it might be useful to offer the work of one theorist who appears to have adequately stated the conditions under which his hypotheses can be expected to be supported. In his *The Logic of Collective Action,* Mancur Olson takes exception to the pluralist position concerning group size as a resource, contending that large groups may be ineffective in pursuing their interests in comparison with smaller groups.[35] Although his hypothesis is derived from more general economic theory, Olson also relies heavily on the kind of illustrative material used by Edelman and Schattschneider. Yet Olson carefully delimits the scope of his theory by specifying the circumstances under which the behavior he predicts is most likely to occur. Large groups, for example, will be able to organize effectively when they are seeking collective goods if their members benefit disproportionately, or if they are able to coerce their memberships or provide members with side payments of selective goods. Because Olson makes such contingent conditions explicit, his hypothesis is testable without requiring further refinement—without the researcher having to guess at the theorist's intentions.[36]

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[26] Lowi criticizes Dror for defining policy as simply any output of any decision maker in his book *Public Policy Making Reexamined* (San Francisco: Chandler, 1968). Dror never formally defines policy but his discussion indicates Lowi is correct. See Lowi, "Decision Making vs. Policy Making," p. 317. Thomas R. Dye defines public policy as "Whatever governments choose to do or not to do," *Understanding Public Policy* (Englewood Cliffs, N.J.: Prentice-Hall, 1972), p. 1.

[27] Harold D. Lasswell and Abraham Kaplan define policy as "a projected program of goal values and practices," *Power and Society* (New Haven: Yale University Press, 1970), p. 71. See also Carl J. Friedrich, *Man and His Government* (New York: McGraw-Hill, 1963), p. 70.

[28] For example, Easton writes: "Arriving at a decision is the formal phase of establishing a policy; it is not the whole policy in relation to a particular problem. A legislature can devise to punish monopolists; that is the intention. But an administrator can destroy or reformulate the decision by failing either to discover offenders or prosecute them vigorously. The failure is as much a part of the policy with regard to monopoly as the formal law. When we act to implement a decision therefore we enter the second or effective phase of a policy." *The Political System,* 2nd ed. (New York: Knopf, 1971), p. 130.

[29] This is our own preferred definition of policy. See our "Case Study Aggregation and Policy Theory," p. 11.

[30] Dye includes the consequences of inaction as well as action, whether intended or not, in his definition of policy. Dye, p. 2.

[31] Lowi suggests the need to look at only important substantive government decisions, "Decision Making vs. Policy Making," p. 317.

[32] Ranney, p. 7.

[33] Murray Edelman, *The Symbolic Uses of Politics* (Urbana: University of Illinois Press, 1974).

[34] E. E. Schattschneider, *The Semi-Sovereign People* (New York: Holt, 1960), pp. 3–77, passim.

[35] Mancur Olson, *The Logic of Collective Action* (Cambridge, Mass.: Harvard University Press, 1965).

Implications

We have argued from our own research experience that the systematic study of public policy is seriously complicated by the nature of the beast. "Policy" is complex—because of temporality, because of multiplicity of aspects and participants, and because of interaction. Yet complexity need not prohibit systematic empirical research. So long as it is accounted for and confronted directly, so long as hypotheses are sufficiently precise, theoretical models may adequately represent reality. We suggest that most of the lessons to be learned have to do, not so much with the conduct of data analysis, as with the formulation of hypotheses and the elaboration of theory.[37] Specifically, we suggest the following:

1. **Temporality: Multiple Outputs.** Optimally, we should all agree on how to identify key developmental points in the policy process, such as the beginning point and the point at which it might be said that policy was indeed "made." Such agreement is undoubtedly premature. In its absence, however, systematic comparative analysis still requires that key stages be identified. In this one instance, it is the data analyst who perhaps has more to contribute than the theorist. We can learn by experience which identification criteria are reliably operationalizable and which among them seem to yield fair tests of hypotheses. In our own work, we have found it both practical and productive to identify a "point of last significant controversy" and, somewhat less importantly, a "point of first significant controversy." The terms are almost self-explanatory, although an elaboration of their meaning and use is available elsewhere.[38] We commend these criteria to others for consideration and trial and urge the formulation and trial of alternatives as well. Most importantly, we emphasize that objective comparisons cannot be made unless a criterion is consistently applied to each case in order to determine which of its many decision points establishes "policy."

2. **Temporality: Postdictive Theory.** One must simply be sensitive to the problem and avoid creating predictive hypotheses that fall into the postdictive trap. Specifically, one must not attempt to predict characteristics of the policy *process* either (a) by variables (such as number of requisite actions or resources committed to the struggle) whose value or score for a given case is not known until too late in the process itself, or (b) by variables (such as "policy type") that are offered predictively by theorists as inputs but that in actuality are characteristics of outputs. For a policy hypothesis to be predictive, the causal variable must obviously be observable and measurable at a point in time before the effect and should not be subject to significant change beyond that point of evaluation.

3. **Multiplicity of Policy Aspects.** The general implication of this problem is that we must either avoid variables that may differ significantly in value depending upon the aspect of a given policy to which they are applied, or that we must provide a means of selecting the aspect that is most relevant for the operationalization of a given variable. Two guidelines on this issue stand out in our experience to date. One is that any typology of whole policies runs a substantial risk of ambiguity at the stage of operationalization; typologies can be extremely valuable, but they should optimally be offered along with well-considered ground rules for classification. The other guideline is that a major (but not exclusive) source of ambiguity lies in the possible divergence of the benefits from the costs or the substantive from the financial aspects of a public policy. In creating hypotheses, it may be profitable for the theorist to consider whether the variables are subject to

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[36] This does not mean that Olson's theory is necessarily correct. There may be additional contingent conditions which he failed to state that would further refine or modify his theory. John Chamberlin, for example, has recently suggested some additional contingent conditions under which large groups are likely to provide large amounts of a collective good in contradiction to Olson's predictions. All we want to imply is that the concepts in Olson's theory are reasonably well specified and the theoretical relationships among concepts are stated with reasonable precision, allowing others successfully to re-examine and test them. See John Chamberlin, "Provision of Collective Goods as a Function of Group Size," *American Political Science Review*, 68 (June 1974), 707–16.

[37] For illustrations of how this process can both permit the elaboration of theory and provide operationalized hypotheses for empirical testing, see Jeffrey A. Miller, "Welfare Criteria and Policy Outcomes: An Empirical Assessment" (Ph.D. dissertation, University of Michigan, 1975).

[38] See our "Case Study Aggregation and Policy Theory," p. 14.

differential rating depending upon whether benefits or costs form the basis of evaluation.

4. **Multiple Participants: Subjective.** The question of whether it is most important to consider a given possible state of the world as it in some sense actually or objectively exists or as it is perceived by participants in the policy process is generally given inadequate attention. If there are good theoretical or methodological grounds for choosing the latter, it is essential to consider whether different participants might perceive the status of affairs differently. If so, it is necessary to translate those grounds into guidelines on whose perceptions are to govern the scoring of that particular dimension for research purposes.

5. **Multiple Participants: Objective.** This problem is potentially the most troublesome of all, since characteristics and behaviors of participants emerge so commonly in policy theory as important variables. The theoretical considerations that necessitate testing a hypothesis comparatively—by assigning a value to each policy struggle to represent some objective characteristic or behavior of "participants"—should make possible some decision as to how those characteristics should be operationalized. Theory might provide criteria for selecting *one* participant or homogeneous group as the basis for assigning a value to the variable, or, perhaps, criteria for aggregating the value across one or more groups of heterogeneous participants. We have had substantial success in dividing the participants in each struggle into two opposing camps and, for most participant-oriented variables, giving an aggregate score on the variable to each camp. However, this attempt at a universal method is much less satisfactory for some variables than for others, and for some cases of policy making than for others. Sometimes it does not work at all. Other schemes might be devised, but such tinkering by the data analyst is in general less desirable than explicit criteria generated within the theoretical proposition to be tested; for example: "In predicting the severity of conflict by 'how much is at stake,' the true predictor is the average of the perceived stakes across all major participants."

6. **Interaction.** It is clear that not all hypotheses can be valid for all types of policy making in all kinds of circumstances. To the extent that a theory fails to specify major conditions defining its applicability, it is inadequate theory. If the conditions of validity are specified, not only is the analyst's job made easier and a fair test of the proposition likely to ensue, but also direct benefits result for the development of the content of policy theory itself. Theory should be parsimonious, to be sure, but not oversimplified.

Conclusion

Because of the complexities of public policy as an object of study, we may never be able to obtain hard knowledge of the policy process of the type available in the advanced physical sciences. Yet improving our understanding of policy phenomena is clearly possible, if only through advancing the conceptual sophistication of theoretical formulations.[39] Our own experience has convinced us that such advances will be more rapid and certain as theory encounters systematic empirical data. As the brief comments in this paper illustrate, the collision between theory and data, while perhaps frustrating at first, can have important benefits for both researchers and theorists. Those lacunae in theory, painfully identified by difficulties in operationalization, become foci for efforts at additional theorizing that, however much they may do violence to the intentions of the original theorist, can add considerably to the richness and utility of hypotheses. The necessity of separating aspects from cost aspects in theories of policy type, or the notion of "last significant controversy" in a policy process, are small, but not trivial examples. If present theoretical levels in public policy are to progress, obstacles of the kind identified here should not be viewed as roadblocks. They cannot be wished away, nor can they be evaded. But by grappling with them directly, it is possible to add to the precision and sophistication of theory while proceeding with the essential conduct of supporting empirical research.

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[39] For an illuminating discussion of the role of "hard" research in developing social theory of various types, see the following two papers by Anatol Rapoport: "Various Meanings of 'Theory'," *American Political Science Review,* 52 (December 1958), 972–88, and "Explanatory Power and Explanatory Appeal" (Paper prepared for the Conference on Explanatory Theory in Political Science, Department of Government, University of Texas at Austin, Feb. 19–23, 1968). Rapoport argues that because of the limitations of social science, improved conceptualization is often a more important criterion in judging good theory than predictive power. In this regard, the value of the application of hard methods in social science would not necessarily lie so much in improved prediction as in better conceptualization and the reformulation of thinking about problems.

Power Consciousness: A Comparative Analysis*

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This article, drawing on recent studies of class consciousness and powerlessness explained in individual versus system level terms, develops and analyzes an operational measure of "power consciousness." Power consciousness is defined as a person's evaluation of his or her political power position and his or her explanation of the causes of any inadequacies or advantages perceived in this position. The operational measure of power consciousness arrays responses to two items on political power satisfaction (one of them open-ended) along a dimension which ranges from satisfaction, to dissatisfaction ascribed to personal failures of the respondent, to dissatisfaction explained in terms of problems with the political system. Survey data are utilized for a comparative analysis of whites' and blacks' scores on the power consciousness measure. The following topics receive detailed attention: the place of power consciousness in the matrix of power measures; its relationship to background factors, especially level of education; its relationship to indicators of political discontent; and the impact of power consciousness on levels and styles of political behavior. The analysis is followed by suggestions for the development of future work in this area.

Introduction

With the advent of survey research, American political scientists have devoted considerable effort to measuring and assessing political efficacy. This interest in subjective feelings of political power is grounded in the demonstrated importance of efficacy as a predictor of voter turnout and other conventional forms of political activity, and in the oft reported relationship between feelings of political efficacy and satisfaction with the political system.[1] Political efficacy is even characterized by some as a regime norm which may operate as a critical force to contain frustrations in mass societies and restrain "disillusionment with [the] norm of democracy."[2]

Recent literature on political efficacy is marked by questions about the equivalence, meaning and structure of measures employed by scholars working in this area.[3] Converse's discussion of the widely used Michigan (SRC/CPS) political efficacy measure holds that even it should be partitioned "into at least two components, which might be more precisely labeled 'personal feelings of political competence' and 'trust in system responsiveness'."[4] Balch makes a similar point in his analysis of the SRC/CPS efficacy measure, discussing "two separate aspects of the [sense of political efficacy] concept" and linking the trust aspect with "diffuse system support."[5]

This distinction between individuals' assessments of their ability to influence government

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[1] There is a substantial amount of research reported in the literature. See Kenneth Prewitt, "Political Efficacy," in *International Encyclopedia of the Social Sciences,* ed. David L. Sills (New York: Crowell, Collier, and Macmillan, 1968), Vol. 12, pp. 225–28, for a concise review and bibliography.

[2] David Easton and Jack Dennis, "The Child's Acquisition of Regime Norms: Political Efficacy," *American Political Science Review,* 61 (March 1967), 38.

[3] See, for example, George I. Balch, "Multiple Indicators in Survey Research: The Concept 'Sense of Political Efficacy'," *Political Methodology,* 1 (Spring 1974), 1–43; Philip E. Converse, "Change in the American Electorate," in *The Human Meaning of Social Change,* ed. Angus Campbell and Philip E. Converse (New York: Russell Sage Foundation, 1972), pp. 334–37; and Edward N. Muller, "Cross-National Dimensions of Political Competence," *American Political Science Review,* 64 (September 1970), 792–809.

[4] Converse, "Change in the American Electorate," p. 334.

[5] Balch, "Multiple Indicators in Survey Research," p. 31.

and their trust in the responsiveness of the political system is analogous to the distinction often drawn in the literature on attitude theory between the cognitive and evaluative (affective) components of an attitude. The former is a probability dimension and refers to beliefs about the probability of unprobability that a particular object or relationship exists. The latter is an evaluation of the object or relationship—whether it is good or bad, satisfactory or unsatisfactory, or the like.[6]

The cognitive/evaluative distinction is important in the social-psychological approach to studying alienation in which subjective powerlessness is a key element. Seeman, for example, defines powerlessness in purely cognitive terms as "the expectancy or probability held by the individual that his own behavior cannot determine the occurrence of the outcomes, or reinforcements, he seeks."[7] The evaluative (affective) component is specifically excluded from the definition because, Seeman says, if powerlessness is operationally defined "in terms of a sensed discrepancy between what is, and what is desired (*unhappiness* over one's control), . . . then the measure of powerlessness easily becomes an index of resentment, discontent, or the like—and in the latter case, dependent variables that are also (in one way or another) measures of resentment, despair, or mistrust become unpersuasive as correlates of the alienation index."[8] Another student of powerlessness, Clark, takes exactly the opposite tack and defines alienation as "the discrepancy between the power a man believes he has and what he believes he should have."[9] The notion here, of course, is that the evaluative component is the very essence of alienation and that measures focusing on dissatisfaction with some institution or relationship have sufficient integrity to stand on their own in an empirical analysis.

Without attempting to decide which definition of alienation (powerlessness) is "correct," it is clear that measurements emphasizing either the cognitive or evaluative components are worth constructing in an effort to understand the nature and consequences of subjective powerlessness.[10] This article focuses on an evaluative aspect of subjective powerlessness called *power consciousness.* It is defined as a person's evaluation of his or her power position and his or her explanation of the causes of any inadequacies or advantages perceived in this position. The concept of power consciousness goes beyond the existence of dissatisfaction or satisfaction to the explanation of the situation, the answer to the question "What is going on here?"[11] The why of dissatisfaction or satisfaction—the nature of the explanations given for perceived power deprivation or advantage—is central to power consciousness.

How does the concept of power consciousness resemble the related concept of class consciousness? As Portes notes, Marx, in attempting to understand why exploited groups don't always rebel against their oppressors, developed "the social psychological concept of class consciousness: recognition by the working class of the structural determinants of its situation and correct identification of the classes are major factors promoting the political struggle of the proletariat."[12] Power consciousness borrows directly from Marxist conceptions of class consciousness since it seeks to identify if people see the source of their own powerlessness in the structure of social or political life. Unlike Marx's class consciousness, however, the idea of power consciousness is independent of a theoretical structure identifying the correct way people should feel or interpret their situations. There is no "false" power consciousness for our purposes. The concept is more empirically based (although it does not preclude the superimposition of a general theory including a notion of false consciousness); this allows an exploration of the most interesting questions or propositions which derive from the idea of class consciousness without a commitment to any one theory or view of the world as correct.

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[6] These definitions are drawn from the critique of the relevant literature found in Martin Fishbein, "A Consideration of Beliefs, and Their Role in Attitude Measurement," in *Readings in Attitude Theory and Measurement,* ed. Martin Fishbein (New York: John Wiley, 1967), pp. 256–66.

[7] Melvin Seeman, "On the Meaning of Alienation," *American Sociological Review,* 24 (December 1959), 784.

[8] Melvin Seeman, "Alienation and Engagement," in *The Human Meaning,* pp. 510–11. See also ibid.

[9] John P. Clark, "Measuring Alienation Within a Social System," *American Sociological Review,* 24 (December 1959), 849–52, and John P. Robinson and Phillip R. Shaver, *Measures of Social Psychological Attitudes,* Appendix B (Ann Arbor: Survey Research Center, Institute for Social Research, 1969), pp. 209–10.

[10] A good case can be drawn from Fishbein's "A Consideration of Beliefs" for an approach which explicitly includes an evaluative component. He argues, in brief, that "all beliefs about an object carry some implicit or explicit evaluation of the attitude object" (p. 264). The problem is that "the same belief may have totally different attitudinal significance for different individuals" (p. 263). Therefore, "independent measures of the evaluative aspects of beliefs" (p. 265) should be utilized.

Following the logic of this argument, the power consciousness indicator developed in this paper measures each individual's power satisfaction in an explicit manner. In the section below on "The Place of Power Consciousness in the Matrix of Power Measures" I will examine the relationship between familiar cognitive indicators of subjective powerlessness and power consciousness.

[11] Robert E. Lane defines general political consciousness as the answer to this question in *Political Thinking and Consciousness* (Chicago: Markham, 1969), p. 319.

[12] Alejandro Portes, "On the Interpretation of Class Consciousness," *American Journal of Sociology,* 77 (September 1971), 228. See also Bertell Ollman, "Marx's Use of 'Class'," *American Journal of Sociology,* 73 (March 1968), 573–80, and Seymour M. Lipset, "Social Class," *International Encyclopedia of the Social Sciences,* Vol. 15, pp. 296–315, for reviews of the literature. A somewhat different view is found in Georg Lukács, *History and Class Consciousness*

The following pages analyze survey data on power consciousness collected from a sample of residents of Detroit, Michigan, and several small jurisdictions in its environs. Profiles will be drawn of the responses of blacks and whites to a question measuring satisfaction or dissatisfaction with the amount of political power possessed and to a follow-up question on the explanation given for power dissatisfaction. The follow-up question allows me to discriminate between those who blame an unsatisfactory power situation on problems with the political system and those who place the blame on adequacies or shortcomings they perceive in themselves.[13] A composite of these questions will be constructed, labeled the "power consciousness" measure, and analyzed in relationship to other variables. The following topics will receive detailed attention: the place of power consciousness in the matrix of power measures; its relationship to background factors, especially level of education; its relationship to indicators of political discontent; and the impact of power consciousness on levels and styles of political behavior. Literature relevant to an empirical analysis of power consciousness will be reviewed in each section, but, since racial contrasts are drawn throughout, a brief comment on the reasons for a comparative analysis is best made at this point.

Racial comparisons of power consciousness are of special interest here because of the developing social-psychological literature on the power explanations of blacks. Gurin et al. have pioneered in this area. They developed and analyzed a special set of four race-related items in their elaboration of Rotter's IE (Internal-External) Scale which measures an individual's expectancy for control or self-efficacy. The items were administered to a sample of black college students in the mid-1960s and produced a factor labeled "individual-system blame." As Gurin et al. noted, "Consistently choosing the internal alternative on these four items means resting the burden for failure on Negroes themselves, specifically on their lack of skill, ability, training, effort, or proper behavior. In contrast, choosing the external alternative means attributing the responsibility for failure to the social system because of lack of opportunities and racial discrimination."[14] In a reversal of what one might expect from the usual I-E findings, those who chose the external control alternatives on the race-related items and blamed the system participated more in civil rights activities. When the students perceived systemic constraining forces as opposed to internal frailties as the source of blacks' failures, they reached a level of consciousness which motivated action against the prevailing system.[15]

A number of provocative questions are raised by Gurin et al.'s discussion and findings which can be answered by analyzing the measure of power consciousness developed in this study separately for whites and blacks. Do whites and blacks tend to give different explanations for power deprivation? How strong, in relative terms (i.e., with social status controlled), is the tendency for blacks to blame themselves for inadequate power possession and, for that matter, to express satisfaction with the amount of power achieved? Are there different correlates and consequences of power dissatisfaction for whites and blacks?

(Cambridge, Mass.: MIT Press, 1971), who defines class consciousness, p. 73, as "the sense, become conscious, of the historical role of the class." The reader should examine pp. 46–82, "Class Consciousness," at a minimum.

[13] Explanations of satisfactory power situations were not secured from respondents in the study reported here.

[14] Patricia Gurin, Gerald Gurin, Rosina C. Lao, and Muriel Beattie, "Internal-External Control in the Motivational Dynamics of Negro Youth," *Journal of Social Issues*, 25 (Summer 1969), 45.

[15] Ibid., p. 52. As Gurin et al. say (p. 33): "Instead of depressing motivation, focusing on external forces may be motivationally healthy if it results from assessing one's chances for success against systematic and real external obstacles rather than the exigencies of an overwhelming, unpredictable fate." See also John R. Forward and Jay R. Williams, "Internal-External Control and Black Militancy," *Journal of Social issues*, 26 (Winter 1970), 75–92.

The Data

Data for this analysis were gathered in a survey of people 16 years of age and older who lived in housing units in the areas covered by the *Detroit City Directory.*[16] The survey went into the field in September of 1970 and the last questionnaire was completed in April 1971. A total of 538 people were interviewed, 174 blacks and 394 whites. An additional sample of 225 (104 blacks and 121 whites) was interviewed at the same time as part of a panel of Detroit area respondents started in 1967. Blacks were interviewed by blacks, whites by whites.

Power Consciousness: A Profile

The major dependent variable in this study, the power consciousness indicator, is derived from answers to two very simple questions: (1) "Do you think people like you have *too little* political power, or just *about the right* amount?"; and (2) if the respondent replies "too little political power," "Why do you have too little power?"[17] The phrase "people like you" in the lead-in question is meant to stimulate respondents, in Converse's words, to think of their "location in the capability pecking order."[18] They should be stimulated to think of themselves as having a relative location, along with others, in the power order of the political system. The intention is to minimize any idiosyncratic explanations which focus on individual inadequacies as the source of an unsatisfactory share of political power and thereby increase confidence in the significance of those that are given.[19]

Table 1 presents the answers of respondents to the power consciousness questions. The similarities in the answers of the panel and cross-section respondents are quite striking.[20] Nevertheless, only data for the cross-sections are displayed in Table 1 since these yield the best estimates of the views of the population. Panel data will, however, be added in the remainder of the tables in order to increase the number of cases in the analysis.

The first and most obvious thing to note is that black and white respondents are about equally satisfied with the amount of political power they believe people like them possess.[21] I will examine any differences in the sources of these positive feelings later, but for now I would like to concentrate on the reasons given for dissatisfaction with the amount of power possessed. Since the percentages of satisfied respondents are quite similar for the two racial groups, I will use the percentages in the right hand columns—that is, the dissatisfied respondents only—in describing the results.

Unusually careful coding procedures for ensuring relaible data should be employed when applying a complex code to an open-ended question. My research assistant and I consulted on the coding of each explanation given by a respondent dissatisfied with the amount of power possessed. An additional coder then was brought in who independently scored each answer. Any discrepancies were resolved. However, the number of discrepancies was quite low and I am very confident about the reliability of the variable.[22]

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[16] For a complete description of the sampling procedure, see Joel D. Aberbach and Jack L. Walker, *Race in the City* (Boston: Little, Brown, 1973), pp. 245–52. Aside from the city of Detroit, the areas covered by the directory were Hamtramck, Highland Park, the Grosse Pointes, and Harper Woods.

[17] A follow-up question was asked only of those who felt they had too little power because of my fear that a probe of the answer "the right amount of power" would appear to challenge that response, damage the rapport between interviewer and interviewee, and bias other responses. The spontaneous comments given to our interviewers, however, and a follow-up question asked of those saying "right amount" on a survey administered to a small sample in Detroit, lead me to believe that these dangers are minimal. The question on the small Detroit study, incidentally, yielded answers focusing mainly on the existence of the franchise, other possibilities for participation, and general system satisfaction. My thanks to Professor Monica Blumenthal of the Survey Research Center at the University of Michigan for placing the item on her questionnaire.

[18] Converse, "Change in the American Electorate," p. 335.

[19] While the phrase "people like you" was not used in the follow-up question, most people seemed to keep it as their frame of reference in answering.

[20] Fifty-nine percent of the white panel respondents and 46 percent of the black panel respondents felt they had the right amount of power. More important, the distributions of reasons given for the power dissatisfaction were remarkably congruent between each racial panel and the appropriate cross-section, especially when one takes into account the slight bias to a more highly educated group found in the panel due to the greater attrition of lower as opposed to higher education respondents.

[21] It is likely that these percentages have dropped since 1971 when the data were collected, but in 1966 when the same question was asked on the SRC Election Study, 55 percent of the whites and 60 percent of the blacks living in the central cities of the 12 largest SMSA's said they had the right amount of power.

[22] There was total agreement between the first and second codings for 82 percent of the answers by

Table 1. Power Consciousness for the Cross-Section Sample, by Race

	White Cross-Section		Black Cross-Section	
Power Consciousness[a]	Total	Too Little Only	Total	Too Little Only
Just about the right amount of political power	60%	–	55%	–
Too little, because of				
1. *Personal (individual) failure or inadequacy*				
a. Lack of initiative, interest, personal inadequacies (blames people like self or self)	7	19%	15	33%
b. Personal inadequacies (no blame of people like self or self)	1	3	3	7
Subtotals	8	22	18	40
2. *Isolated individuals*				
a. Isolated individuals (*no* mention of group): ordinary people do not count; one person, little man does not count	3	8	3	7
b. Isolated individuals (emphasis on need for group): individuals alone can't do much, must belong to a group to have influence	4	10	1	1
c. One vote alone doesn't count	1	3	–	–
Subtotals	8	21	4	8
3. *Group weakness*				
a. My group not powerful enough: unorganized, a minority, not cohesive, etc.	1	3	1	1
b. Black people not powerful enough: unorganized, not cohesive, don't hold enough offices, etc.	–	–	8	17
c. Same as 3b, but *specifically* indicates that whites are responsible	–	–	1	3
Subtotals	1	3	10	21
4. *Political system level problems*				
a. Nominating procedures are closed, unfair	3	7	–	–
b. Electoral system (method) problems	2	4	–	–
c. Government leaders don't care about people, don't listen to them, ignore them	7	19	6	13
d. Government leaders corrupt	1	2	–	–
e. Vested interests, cabal, run the government	3	8	5	11
f. Government (as an institution) doesn't function well, is nonresponsive, too large, etc.	6	14	3	7
Subtotals	22	54	14	31
Totals	99%[b] (N = 365)	100% (N = 146)	101%[b] (N = 165)	100% (N = 75)
Other	(N = 3)	(N = 2)	(N = 1)	(N = 1)
Don't Know, No Answer	(N = 26)	(N = 11)	(N = 8)	(N = 2)

[a] Question: Do you think people like you have *too little* political power or *just about the right amount?* (If too little) Why do you have too little power?

[b] Totals do not sum to 100 percent because of rounding.

The data are arrayed along a crude dimension running from dissatisfaction explained in terms of personal failures to explanations focused on problems of the political system. The rationale for this arrangement is that each major category represents an increasing level of power consciousness culminating in a belief that people have too little political power because of faults in the institutions, personnel, or procedures of the government. Answers were coded so that higher level responses received priority. For example, a person citing a political system level problem and a group weakness problem would be coded into one of the political system categories for purposes of this analysis.

1. *Too Little Power Due to Personal Inadequacies.* Turning to the first major category of those who felt they had too little power (people citing personal failures or inadequacies), 33 percent of the dissatisfied black respondents and 19 percent of the dissatisfied whites said they or people like them lacked the initiative or interest to achieve the right amount of political power. The blame for this situation, most clearly believe, rests squarely on their own shoulders since it is only their personal failures which account for this state of affairs. Typical responses follow:

> (black, male, age: 45) Because people like myself do not get out and let the government know what we, the little people, want from them.
>
> (black, female, 25) People like me don't usually take advantage and do what they could. Most people don't speak out or vote when they should.
>
> (black, female, 28) For one thing, I don't take interest in it like I should and never express my beliefs when in a group.
>
> (white, female, 22) Well, a lot of them are like me, they don't keep up with politics. (Interviewer probe: Why?) They're just not interested.
>
> (white, female, 69) I'm dumb. (Laughed) I'd follow it. Every time I did vote, I'd vote for the wrong one. We'd have an argument, my husband and I. I really don't know. So many things that I really don't understand.
>
> (white, female, 37) It's our own fault. We don't go out and do more about it. We just sit back; guess we are the silent majority. We don't make any noise, you know, go out and march and picket. That's not for me. I've other things more important to do; not that the government isn't important, but my family and home are more important to me.
>
> (white, male, 58) If you get involved you can have it. If you abstain then you don't have any political power. I have not extended myself.

One would have to read very deeply into these comments to catch even a glimmer of blame placed on the political system. In fact, it is difficult to discriminate these respondents from many who said they were satisfied with the amount of political power they possessed and then went on to add spontaneous comments like: "When I vote"; "As much as we're entitled to"; "If we make an effort"; "If we use it"; "If I vote, I would have enough. I don't get out anymore, much"; "If I aspired to more political power I could have it. The fact that I have no such aspirations, that I don't get out and beat the bushes, work as hard as those who are active in politics, doesn't give me any excuse to cry about my lack of power."

A small group of whites (three percent of those dissatisfied with the amount of power possessed) and a slightly larger group of blacks (seven percent) also cite personal inadequacies, but they do not blame themselves for the situation or, at least, one could only infer self-blame from their statements through a very loose interpretation:

> (white, female, 78) It's my age. If I were a young lady, my point of view would be different.
>
> (black, male, 56) I don't have the actual education.

2. *Too Little Power Due to Individual Isolation.* The second cluster of reasons given for unsatisfactory amounts of power all involve individual isolation. Virtually equal percentages of dissatisfied whites and blacks (eight and seven percent respectively) see the individual or ordinary person as cut off, without contacts, unable to mobilize support:

> (black, female, 34) I'm just one person. I don't think my word would go too far.
>
> (white, male, 28) The little man cannot express himself and be heard.

---

whites and 79 percent of the answers by blacks. Most of the disagreements (12 percent for whites and 15 percent for blacks) were about the order of the appropriate codes (i.e., what should be coded as the primary response and what as the secondary response), not the meaning itself. In only 6 percent of the cases for each race was there complete disagreement about the meaning of a response, and these disagreements were well spread out among the code categories.

> (white, male, 53) I guess because one person can't do nothing alone.

Another group within the isolated individuals heading also believes that people like them are isolated and unable to do much, but, unlike the respondents described above, they emphasize the need for group affiliation or assistance in order to gain adequate influence. This category is populated almost exclusively by whites[23] (ten percent versus one percent of the dissatisfied blacks):

> (white, male, 22) No one individual has power. It has to be united with others to have any effect.

> (white, male, 42) Because nobody represents us as an individual. There's no organized group who represents the individual vote.

> (white, female, 50) I don't believe there are enough of us. (Probe: What do you mean?) People are not interested in doing anything. If you could get enough people together who were interested they could do something as a strong group.

3. *Too Little Power Due to Group Weaknesses.* Group weaknesses are the focal point of a third major heading used to identify responses. This category is dominated by blacks who are dissatisfied with the amount of political power blacks possess.[24] I have divided the black powerlessness answers into two categories. The first (17 percent) stresses that black people are not sufficiently organized, are not cohesive enough, or don't hold enough offices. It is composed about equally of those who state this as a sad fact of life at this moment and those who imply that blacks could change things if only they were sufficiently motivated. The following are typical responses of those who see this as a sad fact of life:

> (black, male, 37) The trouble lies in the fact of not getting enough people organized. (Probe: How do you mean?) Not just people, but black people.

> (black, female, 22) Everything we want to change isn't getting changed. The reason must be we don't have enough power—pressure—as a group.

> (black, male, 52) They don't have enough black people in political offices.

Respondents who imply that blacks could change things if only they were sufficiently motivated gave answers like the following:

> (black, male, 37) If we were strong enough Austin[25] would have been in, but some of our people wouldn't vote for him and some didn't vote at all. (Probe: Anything else?) The faith in one another.

> (black, female, 36) Because we have quite a few people that can vote and they don't, thus not giving the Negro much voice.

A second (three percent) and much smaller group consists of people who specifically indicate that whites are responsible for the fact that blacks do not have enough power:

> (black, female, 48) [They] don't really let blacks do everything whites do.

4. *Too Little Power Due to Political System Level Problems.* The last major heading along the power consciousness continuum is "political system level problems." Respondents in the categories listed within it mention aspects of the political system as the source of the inadequate power they believe people like them possess. Almost twice as many whites as blacks (54 percent versus 31 percent) give explanations of this type.

Notions that nominating procedures are closed or unfair or that the electoral system does not function properly were prominent among whites, but unmentioned by blacks:[26]

> (white, male, 27) You don't have any say about who's nominated.

> (white, female, 27) Because of the way the political conventions are set up. The delegates basically are people very active in politics. If you are not an active individual you have no say as to who is nominated. Also, I don't care for the electoral college; should be by direct vote of the people.

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[23] Whites also comprise the small percentage of respondents who say, to quote one of our respondents, "my one vote doesn't mean much."

[24] Without going into detail, the small number of respondents in the group weakness category who were not concerned with race mentioned a variety of groups. For example: "people like me—the minority better educated group—[who] have little influence over the majority group."

[25] Richard Austin ran for mayor of Detroit in 1969 and lost by a very slim margin. He ran successfully for Michigan secretary of state in 1970 and is now the highest ranking black state office holder.

[26] The small number of people coded under isolated individuals in the category "one vote alone doesn't count" were not coded here or under 4.f. because their responses did not clearly indicate that the political system itself was at fault. It is possible that they believe this is so, but it seemed a more conservative convention to assume that they see themselves as isolated, symbolize this isolation in terms of the vote, and do not blame the situation on some problem with the political system.

> [This] may not actually give the people more power, but they will feel more involved.

Relatively large percentages of whites and blacks (19 percent and 13 percent respectively) believe that they do not have the right amount of political power because government leaders are simply unresponsive—they don't care about people and don't listen to them:

> (white, male, 60) What are we going to do? We got no choice. (Probe: What do you mean?) Doesn't matter who's in there, they are going to do what they want. We got nothing to say about it. We vote them in, but as soon as they get in they forget their promises; so nothing we can do.
>
> (white, female, 67) Politicians are voted in, then they do what they want rather than what people want. All lobbyists should be eliminated. They are all paid by large interests.
>
> (white, female, 42) We can write our letters, etc., but if our congressmen feel one course is what they want to take, they do as they please.
>
> (black, female, 39) Normally we don't have too much of a say so when it comes to politics. The politicians, once they get elected, they do what they want to anyway.

Unresponsiveness fades into a deeper charge of corruption for some and, for others, a sense that powerful forces are firmly in control of the political system (10 percent of the whites and 11 percent of the blacks):

> (white, female, 51) Because a little man doesn't stand a chance. You have to know someone in office or have a lot of money. (Probe: Why is that?) All everyone ever seems interested in anymore is money. You have to pay somebody off to get a favor.
>
> (white, male, 29) Too many monied people who run everything. There is a lot of *big* money behind politics.
>
> (black, female, 38) Because most of the people with the money seem to control the government.

A final group of respondents who give system level problems as the reason that people like them have too little political power is composed mainly of whites. They tend to see the government as a large, nonresponsive, poorly functioning institution:

> (white, male, 28) I feel when I vote the government turns it around the way they want. It is not a fair system. The Supreme Court changes a lot of votes too. They change the people's opinions.
>
> (white, female, 34) Because our democracy is not a democracy and our government is too complex. It's a representative [government], but it's like being ruled by a group of people . . . how can you possibly know them? There are too many to actually know what each [other are] doing.

Summary. Whites are only slightly more satisfied than blacks with the amount of political power they feel people like them possess. There are, however, substantial differences in the distribution of reasons that the two racial groups give for power dissatisfaction. Blacks are much more likely to believe that individual failures or inadequacies are the source of the unsatisfactory power distribution. The emphasis here is on the lack of initiative or interest of people who could achieve enough power if only they were willing to make the effort. Blacks are also more prone to cite group weaknesses when explaining why people like them have too little power. They pay substantial attention, not surprisingly, to the problems blacks face in organizing adequate pressure, but there is also some disillusionment with the efforts members of their race have put forth on their own behalf. Whites, more often than blacks, stress that people like them are isolated and alone—in need of a group affiliation or an organized effort to gain adequate attention to their interests. Most striking is the fact that 54 percent of the dissatisfied whites, as compared to 31 percent of the blacks, talk about problems with the political system. Particular attention is given to problems with the nominating or electoral systems and to the nonresponsiveness of leaders and institutions.

In the sections which follow I will analyze some of the correlates and consequences of power consciousness. The responses in Table 1 were arrayed along a crude dimension of power consciousness running from satisfaction with the amount of power possessed, to dissatisfaction ascribed to personal failures, to dissatisfaction explained in terms of problems with the institutions, personnel, or procedures of the government. The many categories and subcategories identified are important for descriptive purposes, but now that we have a better understanding of the frequencies and nuances each represent it is necessary to combine categories so that the data are in a more usable form for analysis.

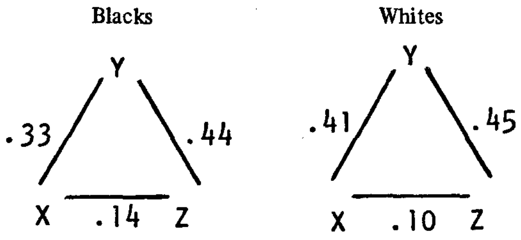
The indicator of power consciousness that I will use is presented below along with the rationale for each category:

1. *Right amount of power:* Self-explanatory.
2. *Too little because of personal failures or inadequacies (self-blame only):* This category contains *only* those who blame themselves or people like themselves for an unsatisfactory power situation. There is no hint in their explanations of blame attached to the political system.
3. *Too little because of isolation or group weakness:* This category is an inclusive one meant to embrace all those whose explanations hold neither themselves nor the political system directly responsible for an unsatisfactory amount of power. The category includes the small number of respondents who mention personal inadequacies, but indicate no self-blame (1.b. in Table 1) and excludes blacks who say people like them do not have enough power and then go on to indicate specifically that whites are to blame. The latter answer seems so clearly to imply blame of "the system" that I felt it would be reasonable to place it in category 4.
4. *Too little because of problems with the political system:* All explanations involving problems with the institutions, personnel, or procedures of government are included here. The one exception is described in the last sentence of paragraph three above.

The Place of Power Consciousness in the Matrix of Power Measures

In the introduction a distinction was drawn between measures of individuals' perceptions of their ability to influence government—mainly a cognitive dimension—and measures more oriented to tapping the evaluative aspects of powerlessness. The primary purpose of this section is to see how the power consciousness measure fits into the matrix of power measures. I will examine the relationships among three measures of perceived political powerlessness: One, a straightforward cognitive (probability) measure, taps the "expectancy that one's own behavior can control the occurrence of [political] rewards."[27] It is a modified version of the Almond-Verba "Political Competence" scale, also called the "Ability to Influence Government" scale by Muller.[28] A second cognitive measure taps respondents' assessments of the amount of political power people in their situation possess.[29] A third measure, clearly evaluative, is the power consciousness indicator which measures discontent with the amount of power held.

X = Perceived Ability to Influence Government*
Y = Amount of Political Power Held by "People Like You"
Z = Power Consciousness



*All variables are scaled so that high scores equal high efficacy or satisfaction.

Figure 1. Relationships (Gamma) Between Power Measures, by Race

Relationships among the three measures are depicted in Figure 1.[30] No assumptions about causality are made in the figure, although a simple causal model of the form X→Y→Z fits these data reasonably well.[31] Whether this simple model is correct or not,[32] it is apparent that the direct link between Variable X (Perceived Ability to Influence Government) and Variable Z (Power Consciousness) is quite weak (.14 or below for both whites and blacks). Many people who are confident that they can influence governments to do the things they want are not satisfied with the amount of political power they believe people like them possess. The link between Y (the Amount of Political Power Held by "People Like You") and Variable Z (Power Consciousness) is much stronger than that between X and Z, although it is possible that the magnitude of the Gamma coefficient is an artifact of the common phrase "people like you" in both measures. The correlations among all three variables, however, are quite modest considering that all deal with aspects of political powerlessness. It is clear, therefore, that discussing the differences between measures of powerlessness that focus on the cognitive (probability) dimension and those which explicitly include an evaluative dimension is more than just a conceptual exercise. This raises the distinct possibility that power consciousness has different correlates than the measures of political efficacy usually employed by political scientists.

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[27] Seeman, "Alienation and Engagement," p. 472.

[28] Muller, pp. 794–801. The following questions were asked in deriving the "Ability to Influence Government" index:

1. Suppose a law were being considered by the Congress in Washington that you considered very unjust or harmful. What do you think you could do about it?
   1a. If you made an effort to change this law, how likely is it that you would succeed: very likely, somewhat likely, or not very likely?
2. Suppose a law were being considered by the *[name of local government unit]* Common Council that you considered very unjust or harmful. What do you think you could do about it?
   2a. If you made an effort to change this law, how likely is it that you would succeed: very likely, somewhat likely, or not very likely?

[29] The item is worded as follows: How much political power do you think people like you have? A great deal, some, not very much, or none?

[30] Gamma is used because it is a widely known and understood ordinal measure of association. It is highly sensitive, however, and can distort when there are extreme marginals. Therefore, the tables used to calculate the Gamma coefficients were checked for each coefficient reported in the article in order to guard against unreliable results.

Correlates of Power Consciousness

Objective Social and Demographic Factors. The strong, positive relationship between high social status and a feeling of political efficacy is one of the most consistent findings in political survey work. "In fact, there is support for the proposition that advanced education is the single most important factor contributing to high efficacy,"[33] and the assumption is readily made that this reflects an accurate reading of the power situation. When it comes to evaluations of the amount of power people have, however, advanced education is, if anything, a predictor of power dissatisfaction.[34] Upper status respondents, rather than lower status respondents as one might expect from the literature on efficacy, believe that people like them do not possess enough political power. Tables 2a and 2b present the relevant data.

The relationships displayed in Table 2a between education and the full power consciousness variable is quite pronounced for blacks (Gamma = .27) and very weak for whites (Gamma = .09). As you will recall, Table 1 indicated that blacks are somewhat less likely than whites to say that people like them have the right amount of political power. Table 2a demonstrates that most of this difference can be accounted for by the unusually high level of power dissatisfaction found among college level blacks.[35]

Table 2b presents the relationship between education and the reasons given for power dissatisfaction only (i.e., respondents believing that people like themselves have the right amount of power have been removed). A factor that was visible in Table 2a is readily apparent here: when lower education respondents, white or black, are dissatisfied with their political power positions, they are very likely to explain the problem in terms of personal inadequacies. College educated respondents, on the other hand, are the most likely to explain power dissatisfaction by citing problems with the political system. Significant differences between whites and blacks are found only among the high school educated respondents. High school educated blacks resemble grade school educated respondents in their reasons given for power dissatisfaction, while high school educated whites bear a closer resemblance to the college educated than to grade school educated respondents in their explanations of unsatisfactory amounts of power possessed. These factors (and the differing levels of formal education in the two racial communities) account for the finding in Table 1 that blacks are more likely to explain power dissatisfaction in terms of personal inadequacies than whites and

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[31] If one specifies that X→Y→Z is correct, then one would predict that $\Gamma_{XY\cdot Z} = 0$. The relevant data for blacks and whites are as follows:

Blacks

$$\begin{tikzcd}
X \arrow[r, "\Gamma = .30"] \arrow[rr, dashed, bend right, "\Gamma = .12"'] & Y \arrow[r, "\Gamma = .33"] & Z
\end{tikzcd}$$

$$\Gamma_{XZ\cdot Y} = .03$$

Whites

$$\begin{tikzcd}
X \arrow[r, "\Gamma = .35"] \arrow[rr, dashed, bend right, "\Gamma = .05"'] & Y \arrow[r, "\Gamma = .31"] & Z
\end{tikzcd}$$

$$\Gamma_{XZ\cdot Y} = -.07$$

[32] That power satisfaction or dissatisfaction is basically the end-product of perceived ability to influence government rather than the other way around seems logical enough, although there are grounds for arguing the other way. See Muller, pp. 801–02.

[33] Prewitt, p. 226.

[34] Education is not atypical of the SES variables in the Detroit study. Occupation and income are similarly related to power consciousness.

[35] This reinforces a picture of discontent among upper status blacks found in other analyses of the data from the Detroit study. See Aberbach and Walker, esp. pp. 118–21, 158–61, and 185–86.

Table 2a. Power Consciousness by Education and Race

Education	Right Amount of Power	Too Little Power Because of: 1. Personal Inadequacies	2. Isolation or Group Weakness	3. Political System Problems	Totals	(N)
Blacks						
Grade school	63%	11%	17%	9%	100%	(64)
High school	50	19	18	13	100	(165)
College	39	8	17	36	100	(36)
			Gamma = .27			
Whites						
Grade school	64	11	14	11	100	(92)
High school	58	9	12	22	101[a]	(247)
College	60	5	8	27	100	(143)
			Gamma = .09			

[a] Percentage does not sum to 100 due to rounding.

Table 2b. Power Dissatisfaction Reasons by Education and Race

Education	1. Personal Inadequacies	2. Isolation or Group Weakness	3. Political System Problems	Totals	(N)
Blacks					
Grade school	29%	46%	25%	100%	(24)
High school	39	35	27	101[a]	(83)
College	14	27	59	100	(22)
		Gamma = .26			
Whites					
Grade school	30	39	30	99[a]	(33)
High school	20	28	52	100	(103)
College	12	19	68	99[a]	(57)
		Gamma = .36			

[a] Percentage does not sum to 100 due to rounding.

whites are more likely to condemn aspects of the political system.

How should we interpret these data? Lane's study of the political ideology of the "common man" is very instructive. He reports fascinating data on the attitudes toward political power of a small sample of whites interviewed in depth. Power, most of his respondents believe, is possessed by those who organize and work for it: "If one has little power at any given time, it is his own fault for failing to organize his interests."[36] The working and lower middle classes have power by virtue of their numbers and what is lacking is a function of their own failure to exercise available opportunities. In a phrase, people get what they deserve. The social and political systems are not seen as very important factors in explaining the inequities which exist in American society.[37]

The consequences of this type of thinking should be debilitating for those at the bottom of the status hierarchy. Ira Katznelson draws on insights flowing from the concept of false consciousness (as well as identification with the aggressor) to describe and explain blacks' interpretations of their relative powerlessness. He gives special emphasis to the notion that the underprivileged powerless will be prone to blame themselves for their predicament in liberal democratic societies with an ethos of equality of opportunity:

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[36] Robert E. Lane, *Political Ideology* (New York: Free Press, 1962), p. 143. See also Richard Sennett and Jonathan Cobb, *The Hidden Injuries of Class* (New York: Knopf, 1972).

[37] This view is also prevalent in whites' notions about the sources of the deprivations blacks experience in American society. See Angus Campbell and Howard Schulman, *Racial Attitudes in Fifteen American Cities: A Preliminary Report Prepared for the National Advisory Commission on Civil Disorders* (Ann Arbor: Survey Research Center, Institute for Social Research, 1968), pp. 30–31.

> Given the onus of choice, the powerless internalize their impossible situation and internalize their guilt. Thus, Elkins has argued, the complete powerlessness of the slave (and of the inmates of German concentration camps) "produced noticeable effects on the slave's very personality." The slave often identified with his master, and accepted society's estimate of himself as being without worth. The less complete, but nonetheless pervasive powerlessness of blacks in America's northern ghettos . . . has had similar effects. . . . The low self-esteem of the powerless may be especially acute in liberal democratic societies with an ethos of equality of opportunity, for then the individual, "free" to choose, may blame himself and/or his group for his predicament.[38]

Recent work on black powerlessness by social psychologists has identified a developing "shift from self to system blame"[39] by ghetto residents. My data on power consciousness indicate that college educated blacks have broken the chains of self-blame which may once have gripped them, but that high school educated blacks are still more likely than comparable whites to blame an unsatisfactory power situation on themselves and not on problems with the political system.[40] Those who have not gone beyond grammar school, regardless of race, are equally prone to self-blame and share the same high level of satisfaction with the amount of power they possess.

The latter fact is explainable by the notion that most of those who are very low in social status, regardless of race, share a belief prevalent in the culture that there can "be no thought that the poor and broken [have] too little power—they [do] not deserve more. . . ."[41] There are, however, clear racial effects at the high school level on the reasons given for power dissatisfaction and these may well reflect the special debilitating effects of the ghetto experience described in Katznelson's statement. High school educated whites are generally satisfied with their level of political power, as Lane indicates was the case for lower middle class and upper working class Eastport residents, but much less likely than comparable blacks to heap the blame on themselves if they are dissatisfied. As noted, when upper status (college educated) people of both races are dissatisfied, they are generally confident that the political system is to blame.

I do not wish to devote too much space to demographic factors, but should note that sex is an inconsequential predictor of power consciousness for both races. (Gamma is –.07 for whites and .02 for blacks.) Age, not surprisingly, is inversely related to power dissatisfaction. (Gamma is –.22 for blacks and –.20 for whites.) Younger respondents are less satisfied with the amount of power people like them possess and are more likely to explain power dissatisfaction in terms of deficiencies in the political system. The higher level of educational achievement of younger respondents, incidentally, explains part, but not all, of this relationship, reinforcing the interpretation cited above of a generational shift from self to system blame. Variables that have been used as indicators of traditionalism for blacks—where respondents were raised and whether or not they are church members[42]—also have a clear impact on power consciousness. Blacks who were raised in Michigan are much more likely to be dissatisfied with the amount of power they have than are blacks raised in the South (Gamma = .31).[43] Blacks who are not church members are more dissatisfied than those who are members (Gamma = .21).[44] In summary, younger blacks with less traditional backgrounds are the most dissatisfied with the amount of political power people like them possess. Traditionalism, in the sense used here, is not a meaningful variable for whites, but younger whites are the most dissatisfied.

Subjective Evaluations of Government. An ideal analysis of the correlates of power consciousness would be based on a longitudinal design which allowed for variation in such factors as economic conditions, income distribution, and the performance of the political system.[45] On the latter point, one would be interested in how power consciousness varied with the provision of public services and benefits, measured both objectively and in terms of perceptions about their quality and adequacy, and with citizens' overall evaluations of the responsiveness and legitimacy of government. Since this analysis is based on data gathered at one point in time I cannot correlate changes in power consciousness with changes in the level of services (measured either objectively or subjectively) or changes in perceptions of government responsiveness and legitimacy. I can, however, look at the relationship between power consciousness and measures of satisfaction with local government services and responsiveness and with a more general measure of basic trust in the authorities and institutions of government. The proposition to be tested is that scores on the power consciousness measure increase with negative perceptions of both the day-to-day workings of government and its overall trustworthiness.

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[38] Ira Katznelson, *Black Men, White Cities* (New York: Oxford University Press for the Institute of Race Relations, 1973), pp. 199–200.

[39] Forward and Williams, p. 88. See also Gurin et al. for the seminal work in this area.

[40] This is not a function of the lower number of terms of high school completed by the average black respondent in the high school group.

[41] Lane, p. 142.

[42] Aberbach and Walker, pp. 124–30.

[43] The Gamma for whites is –.02. Respondents raised in the South and border states are coded 0, those raised in Michigan are coded 1, and those raised in other parts of the North are coded 2. Respondents who lived in more than one place are coded in the category representing the place lived in the longest between ages 6 and 18.

[44] The Gamma for whites is –.09. Church members are coded 0, nonmembers 1.

Table 3 presents the correlations between power consciousness and measures of discontent with local government services and responsiveness as well as with an index of basic political trust which is composed of items focusing on both the federal and local governments. For purposes of this table, all variables are coded so that a positive coefficient indicates that power satisfaction is greatest when the respondent is satisfied with government services or responsiveness or is politically trustful, and power dissatisfaction blamed on problems with the political system is greatest when the respondent is politically distrustful or dissatisfied with services or responsiveness. References for each measure are found in the footnotes to the table.

All of the relationships are in the predicted direction, with the political trust correlations the highest. It is not too surprising that this is the case because the indicators dealing with specific government services and responsiveness are quite concrete and focused on the present, while the political trust measure is more abstract, represents more diffuse and basic evaluations of government, and is a reflection of deeper-seated feelings formed over a longer time span. The power consciousness measure shares these features with the trust measure and, if we wished to use the language commonly employed by alienation theorists, both consciousness and distruct could be labeled as variants of alienation.[46]

Table 3. Correlations (Gamma) Between Measures of Political Discontent and Power Consciousness, by Race[a]

	Blacks	Whites
Evaluations of:		
City services[b]	.19	.20
Police behavior[c]	.15	.24
Likelihood of favorable policies[d]	.21	.19
Political Trust Index[e]	.40	.31

[a] For purposes of this table, all variables are coded in the same direction, i.e., low scores indicate satisfaction or trust and high scores indicate dissatisfaction or distrust. See the text for a more detailed explanation.

[b] The City Services measure is a simple additive index of the number of city services about which the respondents expressed dissatisfaction. The index runs from 0 to 5. The services are garbage collection, parks and playgrounds for children, sports and recreation centers for teenagers, police protection, and public schools.

[c] The Police Behavior measure is a simple additive index of feelings that neighborhood police lack respect or use insulting language, frisk people without good reason, or use unnecessary force in making arrests.

[d] Respondents were first asked, "As you well know, the government in Detroit faces many serious problems. What do you personally feel is the most important problem the government in Detroit should try to take care of?" Then: "You have mentioned [cite problem]. What would you like to see the government do about that?" The question was followed by the item used in the analysis: "How likely do you think it is that the government will do what you want about this problem: very likely, somewhat likely, or not very likely?"

[e] See Aberbach and Walker, pp. 180–84,for a complete description of the political trust measure.

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[45] Marxist notions about the dynamics of class consciousness make one especially interested in a longitudinal examination of the relationship between objective social status and power consciousness which covered periods of social and economic stress as well as equanimity. The expectation would be that significant numbers of lower class individuals would begin to blame the system for their powerlessness only in a time of crisis. See Lipset, p. 299.

[46] See Seeman, "On the Meaning of Alienation," pp. 783–91, and "Alienation and Engagement," pp. 472–74.

The introduction to this essay describes in some detail the debate within the alienation literature about the appropriateness of correlating primarily evaluative measures of powerlessness with measures of resentment, despair, or distrust since all share common elements of discontent. This issue will provoke debate for a long time because it encompasses both a highly complex methodological problem and a fundamental question about the nature of alienation. I am not in a position to offer a definitive resolution to it here, but it is clear that a person's power consciousness, as I measure it, is closely tied to his or her basic evaluation of the trustworthiness of governmental authorities and institutions.[47] If government is seen as worthy of trust, people are likely to be satisfied with the share of political power they and others like them possess; if government is not to be trusted, then power distribution is more likely to be seen as unsatisfactory, and, as the data in the tables used to calculate the trust correlations demonstrate, some problem with the political system is seen as the reason for the unsatisfactory situation. The relationship is obviously reciprocal, with each evaluation (power dissatisfaction and political distrust) feeding the other, although the correlation is certainly *not* strong enough to claim that the two always occur together.[48] The two variables share much in common, but they do not appear to lie on the same psychological dimension and can be distinguished empirically.

Power Consciousness, Racial Militance, and Political Behavior

What attitudinal and behavioral patterns should we expect from those who do not feel that they have the right amount of political power? There are some suggestions in the literature. As noted earlier, Gurin et al. hypothesize that people who blame powerlessness on the social system, and for whom this is a reasonable rationale (oppressed minorities, for example) will cope better than others with adverse circumstances. In their research, Gurin et al. found that black college students who attributed the problems faced by blacks to the social system rather than to individual qualities were more militant on racial matters, more likely to support confrontation tactics in the civil rights struggle, and engaged more in civil rights activities.[49] Portes, in a study of the impact of class consciousness on leftist radicalism in Chile, examined the effects on radicalism of separate measures of frustration with one's position in the social order and of structural versus nonstructural attribution of blame for the frustrations felt. He found that each had an independent effect, but there was no evidence of significant interaction effects.[50]

The data collected in this study are not, unfortunately, ideal for examining the behavioral effects of power consciousness. They provide no detailed information on the civil rights activities described by Gurin et al. nor on the types of radical activities emphasized in the Portes study (participation in collective movements or leftist radicalism). They do, however, enable us to look at the relationship of power consciousness to racial militancy and to measures of participation in a series of conventional political activities as well as to a summary measure of participation in unconventional, nonviolent political activities.[51] With regard to these data, both the class consciousness analogy pursued in this article and the logic of Gurin et al.'s and Portes' findings and speculations lead one to expect power satisfaction to encourage conventional forms of political participation, structurally explained dissatisfaction to be associated with unconventional techniques of political expression and with racial militancy for blacks, and dissatisfaction blamed on the self to lead either to inactivity or to very conventional attitudes and behavior.

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[47] Incidentally, the measure of power consciousness is related to political trust with the measures of "Ability to Influence Government" and "Amount of Political Power Held by People like You" controlled.

[48] For example, it is easy to imagine people who distrust the government, but feel they have a satisfactory amount of influence. Litt describes this for efficacy operationally defined in a cognitive manner, but the logic is the same. See Edgar Litt, "Political Cynicism and Political Futility," *Journal of Politics*, 23 (May 1963), 312–23.

[49] Gurin et al., pp. 31–34, 47–52.

[50] Portes, pp. 232–44.

[51] The study also contained a measure of participation in violent activities, but the number of participants in such activities in the type of community study from which these data were drawn was too small to permit a meaningful analysis. (Three percent of the black respondents questioned and none of the whites reported taking part in a violent protest such as a riot or rebellion.)

I use the terms conventional and unconventional here as defined in *Race in the City*, p. 203: "The term *conventional techniques* means those customarily used in standard American electoral or pressure politics. The term *unconventional techniques* refers to actions, ranging from peaceful protest to rebellion, which implicitly or explicitly threaten to disrupt the normal political process. Unconventional techniques need not be illegal, although they often are."

Turning first to black racial militancy, let us examine the relationship between power consciousness and approval of the black power slogan, a slogan which has been analyzed in depth as a component of militant ideology in the black community.[52] Table 4 shows that approval of the black power slogan is highest among blacks who cited problems with the political system as the reason for their power dissatisfaction and about equal among those who are satisfied with the amount of power they possess and those who believe that personal inadequacies or failures are sufficient to explain an unsatisfactory situation. As expected, there is a distinct relationship between blaming the political system and not the self for failure to secure an adequate amount of political power and adopting more militant views on racial matters.[53]

With regard to the available data on political behavior, rates of participation in both conventional and unconventional activities were calculated for respondents classified according to their positions on the power consciousness measure.[54] The same basic pattern emerged for both whites and blacks, although the percentage differences were generally quite modest.[55] Those who were dissatisfied with the amount of power people like them possessed and who placed the blame for this on the political system tended to be the most active politically, regardless of the type of behavior. Those who blamed themselves for power dissatisfaction tended to be the least active, although the magnitudes of the differences in participation between this group and those who were satisfied with their political power or dissatisfied because of isolation or group weakness were not great. The overall pattern fit what Portes describes as the interaction model.[56] The factor stimulating political activity was dissatisfaction *combined with* blame of the political order, not dissatisfaction by itself. In fact, self-blame inhibited behavior slightly so that those who held themselves responsible for an unsatisfactory amount of power were usually the least active politically.

The data, then, suggest that political system explanations of power dissatisfaction are an impetus to political behavior.[57] However, the trend unexpectedly occurred for both the

Table 4. "Black Power" Approval and Power Consciousness

	Right Amount of Power	*Too Little Power* because of: 1. Personal Inadequacies	2. Isolation or Group Weakness	3. Political System Problems
Percent approving black power[a]	55% (N = 119)	56% (N = 34)	65% (N = 43)	74% (N = 33)

[a]Question: Do you approve or disapprove of "black power"?

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[52] See Aberbach and Walker, Ch. 4.

[53] Since both black power approval and the reasons given for power dissatisfaction are related to education, the relationship between power consciousness and black power approval was examined with education controlled. The number of cases, unfortunately, makes some of the cell sizes much too small for reliable analysis, but the pattern is quite consistent on the crucial point: those who blame the political system for failure to secure an adequate amount of power are more favorable to black power than those who blame themselves.

[54] The conventional activities were voting, wearing a campaign button or going to a political meeting, contributing money to a political campaign, and working actively in a political campaign. The measure of unconventional activities was a composite indicator of participation in nonviolent protests such as boycotts, marches, sit-ins, or picketing. (Only ten percent of the total black sample and six percent of the total white sample reported engaging in any such activities.) See fn. 51 for reasons that reports of violent activities were not analyzed.

[55] A full display of these data in detailed, tabular form can be found in Joel D. Aberbach, "Power and Consciousness: A Comparative Analysis," paper prepared for the annual meeting of the American Political Science Association, New Orleans, La., September 4–8, 1973, pp. 25–26.

[56] Portes, p. 242, describes the expected data display predicted by the interaction model as one where the attitudinal or behavioral dependent variable "will be inhibited among those who, being frustrated, do not blame society for their failures and will be stimulated among those who are frustrated *and* blame the social order for their situation."

[57] The assumption that power consciousness causes behavior is made here in order to simplify the description. It is likely that behavior also influences power consciousness. Incidentally, controls for education (on the relationship between power consciousness and behavior) yield a rather complex pattern which is difficult to interpret with any confidence because of the small Ns in many of the categories.

conventional and unconventional modes of behavior measured, and the impact of political system blame, while fairly consistent, was not strong. With regard to highly unconventional behavior, such as taking part in riots or rebellions as a means of showing dissatisfaction or disagreement with government policy, there are some indications that the predictions derived from the literature hold, at least for blacks.[58] Using a measure of approval of rioting or rebellion as a means of expressing discontent, we find that ten percent of those satisfied with the amount of power possessed approved, only two percent who are dissatisfied but blame personal inadequacies approved, while 21 percent of those giving political system explanations approved. The relationships are not so clear with other types of unconventional activities, such as sit-ins, however, and better data drawn from studies designed to yield a significant number of participants in a range of unconventional activities are necessary to support the point one way or the other.

Summary and Conclusions

Political scientists, long interested in political efficacy, have recently begun to examine the differences between the cognitive and evaluative components of operational measures of that concept. This article, drawing on recent studies of class consciousness and of powerlessness explained in individual versus system level terms, provides a comparative analysis of black and white respondents' satisfaction or dissatisfaction with the amount of political power they possess and of explanations given by them for power dissatisfaction.

The basic variable employed is a measure of power consciousness—defined as a person's evaluation of his or her power position and his or her explanation of the causes of any inadequacies or advantages perceived in this position. The operational measure arrays responses to two items on political power dissatisfaction (one of them open-ended) along a dimension of power consciousness running from satisfaction, to dissatisfaction ascribed to the personal failures of the respondent, to dissatisfaction explained by reference to problems with the political system.

Whites are only slightly more satisfied than blacks with the amount of political power they hold, but the distribution of reasons the two groups give for power dissatisfaction is quite different. Blacks are much more likely to believe that individual failures or inadequacies are the sources of an unsatisfactory power distribution. The emphasis here is on the lack of initiative or interest of people who could achieve power if only they were willing to make the effort. Blacks are also more prone to cite group weaknesses as the reason for power dissatisfaction. Whites, more often than blacks, stress individual isolation as a source of power dissatisfaction, but mainly emphasize problems with the institutions, personnel, and procedures of government.

In spite of the fact that both the power consciousness indicator and the cognitive measures of political efficacy described in the text deal with political powerlessness, the relationships between them are quite modest. Power consciousness also has a different relationship than political efficacy to social status: the more highly educated tend to be the most dissatisfied. College educated blacks are the least satisfied with the amount of political power people like them possess, and both college educated blacks and whites are the most likely to explain power dissatisfaction in terms of difficulties with the political system. Lower status people of both races tend to condemn themselves rather than the political system for their unsatisfactory amounts of power. The majority of the lower status respondents are, in fact, satisfied with the amount of political power they have and the minority who feel they or people like them do not have the right amount of power often accept the blame for this situation. The negative impact of the black experience in the United States on black self-image is much in evidence in the power consciousness of high school educated blacks who are more likely to blame themselves for unsatisfactory amounts of power than are high school educated whites.

Power dissatisfaction is consistently, but modestly, related to measures of discontent with government services and responsiveness. It is also related to racial militancy for blacks. This, incidentally, coincides very nicely with the fact that the more traditional blacks—those who were raised in the South and maintain their ties with the church—are the most likely to say that people like them have the right amount of power. It is the militant, nontraditional blacks who are the most dissatisfied.

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[58] As indicated above, too few respondents reported taking part in riots or rebellions to use this as a variable in the analysis. As a surrogate, I employed a measure of approval of such methods for showing dissatisfaction or disagreement with government policy (approved by ten percent of the total black sample and two percent of the total white sample), but approval, of course, is not the same as behavior. See Aberbach and Walker, pp. 203–06.

Political distrust is an especially potent predictor of power dissatisfaction. People who believe that government authorities and institutions do not deserve their trust are the most likely to believe that they have too little power. They are also the most likely to explain unsatisfactory power situations in terms of problems with the political system. The two feelings (distrust and power dissatisfaction) clearly feed on each other.

The behavioral correlates of power consciousness are worth a fuller exploration than that given here. Political system explanations of power dissatisfaction are associated with higher than average levels of political activity for both blacks and whites. This is true for both conventional and unconventional types of behavior. Blaming oneself for unsatisfactory amounts of power tends to depress activity slightly. Data on approval of rioting or rebellion as a means of expressing discontent indicate that it would be worth exploring, in some detail, the impact of power consciousness on unconventional behavior. To do this well, we would have to study purposefully selected samples which evidence high rates and a variety of disruptive activities.

This brings me to the question of directions for future research if we are to understand power consciousness more completely. A more sophisticated measure of the political power satisfaction dimension of power consciousness would be very helpful. Are people satisfied because they get what they want, or what they feel their own lack of drive and initiative condemns them to accept? How important are the highly visible symbols of democratic politics, like voting, in building satisfaction with the amount of political power people possess?

Longitudinal studies would permit us to trace trends in power consciousness and to understand the dynamic elements of the relationships discussed in this article. What are the impacts of changing economic and political conditions, measured both objectively and as perceived by people, on power consciousness? Does it take a severe crisis which undermines the legitimacy of the political and economic systems before large percentages of the lower status population begin to express power dissatisfaction and to blame the political system for their dissatisfaction? What is the impact of changing government performance and actions (the data reported in this article are pre-Watergate, for example) on the perceptions of government services and responsiveness and, in turn, on power consciousness? How does the relationship between political trust and power consciousness change as situations change and the distributions of scores on the index change? What is the nature of the lags, if any, between changes in objective indicators of economic status and government performance, subjective reactions to these changes, and power consciousness?

A final set of issues concerns the impact of power consciousness on political behavior. The necessity for purposive samples of those who engage in unconventional activities is clear, but the suggestive data on the relationship of power consciousness to conventional participation indicate that more subtle information on political behavior might aid our understanding. We need better and more complete measures of the *meaning* of political behavior to people—that is, the subjective meaning of political expression as well as the type and quantity. Do those who are dissatisfied with the amount of power they have and condemn themselves for this situation tend to participate in a passive manner, doing their citizen duty in a ritual way as a matter of course (and perhaps, in rare cases, striking out in blind anger)? Do those who blame the political system perform their acts with an intensity brought on by anger, often lashing out at the authorities as a catharsis, or do they tend to be unusually purposeful in their mobilization of political resources? In larger terms, under what circumstances can power dissatisfaction be used by leaders for their own ends, when will it be dissipated in self-pity, and when can it be a creative force for political change?

On the Meaning of Political Support*

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The incumbent vs. system affect distinction is basic in the conceptualization of political support. It is based on the premise that system affect is a more important antecedent of aggressive political behavior than incumbent affect. The data reported here show that it is possible to distinguish incumbent from system affect empirically, and also theoretically important to make the incumbent-system distinction. Measures especially sensitive to incumbent affect correlate differently with ideology than does a measure especially sensitive to system affect. Byvariate correlations between measures of incumbent affect and a measure of aggressive political behavior are shown to be either spurious or indirect, due to the fact that incumbent affect is correlated with what appears to be a more powerful and direct antecedent of aggressive political behavior, namely, system affect. The theory behind the incumbent-system distinction is expressed in four propositions. In general, the data conform to it, but each prediction is qualified according to whether ideology and community context are inhibitory or facilitative.

Nearly two decades ago, in "On the Meaning of Alienation," Melvin Seeman addressed himself to clarification of one of the central concepts in social thought.[1] Our concern here parallels his: clarification of an often-invoked yet imprecisely understood concept. In this instance, the concept is not alienation in general, but a specifically political variant of it;[2] and the method by which we shall attempt to clarify its meaning is not theoretical but empirical.

The major work has been done.[3] An abundance of constructs in the area of political support and alienation have been proposed, but empirical validation of their utility is still in its infancy. Perhaps the most basic analytical distinction presented so far is that between affect for an incumbent administration versus affect for the political system. Yet, we do not know if this distinction is important.

What are the ingredients necessary to resolve the issue of the utility of the incumbent versus system distinction? Simply, one must have a set of variables which, on the basis of theory about the difference between incumbent evaluation and system support, relates to one differently than to the other—that is, discriminates between them. But that prescription has proved notoriously difficult to fill. This is because one member of the set of discriminating variables—perhaps the most necessary ingredient—is aggressive political behavior.[4] And, in those soci-

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[1] Melvin Seeman, "On the Meaning of Alienation," *American Sociological Review*, 24 (December 1959), 783–91.

[2] We view the concept of political support and alienation as a continuous variable ranging from support at the positive end of the continuum to alienation at the negative end. Thus, when we use the terms "support" and "alienation" we are referring to the same variable.

[3] The most comprehensive theoretical treatment of political support is David Easton, *A Systems Analysis of Political Life* (New York: Wiley, 1965), pp. 153–140. A seminal, though brief, discussion appears in Seymour Martin Lipset, *Political Man* (Garden City: Doubleday, 1960), Ch. 3. See also Robert E. Lane, *Political Ideology* (New York: Free Press, 1962); Gabriel A. Almond and Sidney Verba, *The Civic Culture* (Princeton: Princeton University Press, 1963); William A. Gamson, *Power and Discontent* (Homewood, Ill.: Dorsey Press, 1968), ch. 3; David Easton and Jack Dennis, *Children in the Political System: Origins of Political Legitimacy* (New York: McGraw-Hill, 1969). Other studies dealing with the conceptual problems of political support and alienation include: Joel D. Aberbach, "Alienation and Political Behavior," *American Political Science Review*, 63 (March 1969), 86–99; Ada W. Finifter, "Dimensions of Political Alienation," *American Political Science Review*, 64 (June 1970), 389–410; Edward N. Muller, "Correlates and Consequences of Beliefs in the Legitimacy of Regime Structures," *Midwest Journal of Political Science*, 14 (August 1970), 392–412; John Fraser, "The Impact of Community and Regime Orientation on Choice of Political System," *Midwest Journal of Political Science*, 14 (August 1970), 413–33.

[4] We define aggressive political behavior as collective action on the part of nonelites that is illegal and has a political focus in that it is intended to disrupt the normal functioning of government. It is

eties best equipped to measure it systematically through highly developed survey research technology, aggressive political behavior has been difficult to find by means of normal cross-sectional sampling.

This article reports the results of a study undertaken, in part, to come to grips empirically with the problem of determining the utility of certain analytical distinctions which have been made about the concept of political support. In particular, we shall focus on the question of the incumbent-system distinction. Also, in that context, we shall attempt to shed some light on the recent controversy over the status of political trust: Is political trust largely a measure of approval-disapproval of an incumbent administration? Or is it bound up closely with feelings about the legitimacy of the system of political authority as a whole?[5]

The question of the empirical meaning of political trust is especially important because of its frequent use in survey research.[6] Though commonly regarded as a measure sensitive to system affect, decisive evidence in favor of this interpretation has not yet been forthcoming. Citrin has called the prevailing interpretation into question, claiming that political trust is, in fact, a more sensitive register of affect for an incumbent administration than for the system of government.[7] Miller has argued just the opposite position.[8] Due to severe measurement constraints in the data at hand, neither Citrin's challenge nor Miller's rebuttal could really settle the issue. Satisfactory resolution of it would require, at a minimum, independent measures of incumbent and system affect, as well as measurement of variables—e.g., aggressive political behavior—which might discriminate between them. Although ample measures of incumbent affect were available, measurement of system affect and aggressive be-

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important to the distinction between incumbent and system affect because the latter is thought to be a far more consequential antecedent of it than the former.

[5] This was the central issue raised in the recent Miller-Citrin debate. See Arthur H. Miller, "Political Issues and Trust in Government: 1964–1970," *American Political Science Review*, 68 (September 1974), 951–72; Jack Citrin, "Comment: The Political Relevance of Trust in Government," *American Political Science Review*, 68 (September 1974), 973–88; Arthur H. Miller, "Rejoinder to 'Comment' by Jack Citrin: Political Discontent or Ritualism?" *American Political Science Review*, 68 (September 1974), 989–1001.

[6] The literature employing the concept political trust (or "cynicism") is extensive, but for examples see Robert E. Agger, Marshall N. Goldstein, and Stanley A. Pearl, "Political Cynicism: Measurement and Meaning," *Journal of Politics*, 23 (August 1961), 477–506; Edgar Litt, "Political Cynicism and Political Futility," *Journal of Politics*, 25 (May 1963), 312–23; Herbert McCloskey, "Consensus and Ideology in American Politics," *American Political Science Review*, 58 (June 1964), 361–83; Frank A. Pinner, "Parental Overprotection and Political Distrust," *The Annals of the American Academy of Political and Social Science*, 361 (September 1965), 58–70; Robert E. Lane, "The Politics of Consensus in an Age of Affluence," *American Political Science Review*, 59 (December 1965), 874–95; M. Kent Jennings and Richard G. Niemi, "The Transmission of Political Values from Parent to Child," *American Political Science Review*, 62 (March 1968), 169–84; Kenneth P. Langton and M. Kent Jennings, "Political Socialization and the High School Civics Curriculum in the United States," *American Political Science Review*, 62 (September 1968), 852–67; Aberbach, "Alienation and Political Behavior"; Paul R. Abramson and Ronald Inglehart, "The Development of Systemic Support in Four Western Democracies," *Comparative Political Studies*, 2 (January 1970), 419–42; Jack Dennis, "Support for the Institution of Elections by the Mass Public," *American Political Science Review*, 64 (September 1970), 819–35; Joel D. Aberbach and Jack L. Walker, "Political Trust and Racial Ideology," *American Political Science Review*, 64 (December 1970), 1199–1219; Edward N. Muller, "The Representation of Citizens by Political Authorities: Consequences for Regime Support," *American Political Science Review*, 64 (December 1970), 1149–66; Jeffery Paige, "Political Orientation and Riot Participation," *American Sociological Review*, 36 (October 1971), 810–20; Edward N. Muller, "A Test of a Partial Theory of Potential for Political Violence," *American Political Science Review*, 66 (September 1972), 928–59; Andre Modigliani, "Hawks and Doves, Isolationism and Political Distrust: An Analysis of Public Opinion on Military Policy," *American Political Science Review*, 66 (September 1972), 960–78; Paul R. Abramson, "Political Efficacy and Political Trust Among Black School Children: Two Explanations," *The Journal of Politics*, 34 (November 1972), 1234–75; Donald D. Searing, Joel J. Schwartz, and Alden E. Lind, "The Structuring Principle: Political Socialization and Belief Systems," *American Political Science Review*, 67 (June 1973), 415–32; Richard L. Cole, "Toward a Model of Political Trust: A Causal Analysis," *American Journal of Political Science*, 17 (November 1973), 809–17; Meredith W. Watts, "Efficacy, Trust, and Commitment to the Political Process," *Social Science Quarterly*, 54 (December 1973), 623–31; Harrell R. Rodgers, Jr., "Toward Explanation of the Political Efficacy and Political Cynicism of Black Adolescents: An Exploratory Study," *American Journal of Political Science*, 18 (May 1974), 257–82; Miller, "Political Issues and Trust."

[7] Citrin, "Comment." See also Jack Citrin, Herbert McClosky, J. Merrill Shanks, and Paul M. Sniderman, "Personal and Political Sources of Political Alienation," *British Journal of Political Science*, 4 (September 1974), 1–31; Merrill Shanks and Jack Citrin, "The Measurement of Political Alienation: Strategic and Methodological Issues," paper prepared for the Conference on Political Alienation, Iowa City, Iowa, January 8–11, 1975.

[8] Miller, "Rejoinder."

havior was deficient.[9] In this study we have sought to correct those deficiencies by including a multi-item measure that we think is sensitive to system affect, by using a measure of actual participation in aggressive political behavior, and by employing a research design intended to enhance variation in aggressive political behavior.

Theoretical Importance of the Incumbent-System Distinction

Why might one want to distinguish between affect for incumbents and affect for the system? And what are the objections which have been raised in regard to the utility of this distinction?

The hypothesis behind the distinction between incumbent affect and system affect is that the latter is more consequential for the stability of a political regime than the former. This hypothesis may be stated in very general form as follows: *If system affect is negative among powerful or sizable segments of a polity, the threat to the stability of the prevailing regime will be great, even if affect for a particular incumbent administration is positive; conversely, if system affect is positive among powerful or sizable segments of a polity, the threat to the stability of the prevailing regime will be small, even if affect for a particular incumbent administration is negative.* An application of this hypothesis to modern Anglo-American and European political history is Lipset's argument (where "effectiveness" refers to incumbent affect and "legitimacy" to system affect) that "when the effectiveness of various governments broke down in the 1930s, those societies which were high on the scale of legitimacy remained democratic, while such countries as Germany, Austria, and Spain lost their freedom, and France narrowly escaped a similar fate."[10] Societies presumed to be low on a scale of system affect ("legitimacy") experienced much higher rates of disruptive political protest and violence during the 1920s than did those presumed to be high on a scale of system affect. And when incumbent affect turned negative in those societies already threatened by low system affect, this was enough to bring about the demise of the prevailing regime.

Lipset's interpretation, and the hypothesis which underlies it, is intuitively plausible. But, it has not yet been cast into operational form and tested empirically. In the interim, serious doubts about the applicability of the incumbent-system distinction to the real world have arisen which center around the question of whether it is possible actually to measure something like "effectiveness" or approval of the performance of an incumbent administration, as opposed to something like "legitimacy" or support for the system of government in general.

In Loewenberg's view, the distinction between what Easton calls "specific" versus "diffuse" support—a distinction roughly comparable to effectiveness versus legitimacy—raises "insuperable problems of measurement."[11] These measurement problems arise from the fact that separate indicators of these analytical constructs generally are found to be highly correlated. The difficulties posed by this state of affairs have been elaborated succinctly by Shanks and Citrin:

> At least in our Bay Area materials, the citizen's evaluations of incumbents and the political system are highly correlated enough that any analysis has to consider the rival hypothesis that indicators of disaffection from the system, on which "high" scores are relatively rare, merely assess a form of generalized dissatisfaction with government per se. But even if the attitude objects are conceptually distinct in the minds of all respondents (which we doubt), the very high correlation between the two theoretical dimensions would make dimensional analysis and item selection a hazardous business with unstable results.... This combination of highly intercorrelated theoretical dimensions and indicator items which are actually caused by more than one of the dimensions will, of course, produce hopelessly indeterminate statistical results, because the underlying causal structure is under-identified.[12]

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[9] Citrin, in "Comment," p. 975, uses two single items (with limited response options) as surrogate measures of system support: one question elicits respondents' pride in "our form of government" and the other inquires whether a "change in our form of government" is needed. For a succinct discussion of the weaknesses inherent in single-item measures, see Jum C. Nunnally, *Psychometric Theory* (New York: McGraw-Hill, 1967), pp. 56–58; Also, as Citrin points out (p. 80; see also Shanks and Citrin, p. 15), cross-sectional surveys simply do not capture sufficient variation in antisystem behaviors. Because of the limitations of his data source, Miller ("Rejoinder," pp. 995–97) is restricted to three items measuring only approval-disapproval of three protest activities: legal protest marches, refusal to obey unjust laws, disruptive sit-ins and demonstrations.

[10] Lipset, p. 69.

[11] Gerhard Loewenberg, "The Influence of Parliamentary Behavior on Regime Stability," *Comparative Politics*, 3 (January 1971), 184.

[12] Shanks and Citrin, p. 14.

And if it turns out that the incumbent-system distinction is impossible to validate empirically, then it has no utility as part of any explanation of the stability or instability of a political regime, despite face plausibility.

In responding to these objections, Easton has raised the question: "Without discriminating in some way between specific and diffuse support could we explain adequately the occurrence of extreme political tensions, conflict, and discontent in some systems, especially democratic ones, without all these giving rise to serious threats to the stability of the regime, or political community?"[13] To be sure, as he points out, "this is a transparent and universal phenomenon the explanation for which is not intuitively known."[14] But if it is not possible to discriminate between specific and diffuse support, then one must face the fact that it may be necessary to look elsewhere than to political support to account for this phenomenon.

Is there a way out of the dilemma posed by high correlation between measures of incumbent and system affect? Only if the correlation is not so close as to preclude the occurrence of enough instances of "inconsistent" combinations—positive incumbent affect together with negative system affect and vice versa—sufficient for reliable statistical analysis. If this condition is met, given that one also has available a measure of participation in political behavior potentially threatening to the stability of a political regime, then the theoretical utility of the incumbent-system distinction could be assessed.[15]

Aggressive political behavior encompasses overt political actions which, if manifested by segments of a population that are powerful or sizable, can threaten the stability of a regime. Taking a person's rate of participation in aggressive political behavior as the dependent variable, the theory behind the incumbent-system distinction may be stated in the form of four propositions:

(P_1): If system affect is positive, participation in aggressive political behavior will show little tendency to vary with the extent to which affect for an incumbent administration is negative.

(P_2): If system affect is negative, participation in aggressive political behavior will show a slight tendency to vary with the extent to which affect for an incumbent administration is negative.

(P_3): If incumbent affect is positive, participation in aggressive political behavior will show a moderate tendency to vary with the extent to which affect for the system of government is negative.

(P_4): If incumbent affect is negative, participation in aggressive political behavior will show a strong tendency to vary with the extent to which affect for the system of government is negative.[16]

What this theory says, in effect, is that if affect for an incumbent administration declines from positive to negative, the stability of a regime will not be threatened by virtue of aggressive political protest and violence, no matter how powerful or sizable the segment of the population registering this decline, as long as their affect for the political system is positive. But if affect for the political system were to decline from positive to negative, the threat to the stability of a regime posed by aggressive political protest and violence would be, at the very least, moderate, if powerful or sizable segments of the population were registering this decline; and the threat would become strong, if affect for the incumbent administration also turned negative.

Data

The data base for this investigation is a survey of 2663 adults carried out in the Federal Republic of Germany during the Fall of 1974 by *Infratest.*[17] Interviews were administered to a sample of persons residing in selected rural, urban, and university milieus throughout the Federal Republic. There were 12 sampling sites, each selected because opposition to the regime had been manifested there during the preceding five years at higher than average levels. In the rural and urban sites opposition had taken the form of voting support for extreme Left or extreme Right political parties. Oppositionist behavior in the universities had taken the form of civil disobedience and political violence.

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[13] David Easton, "A Re-Assessment of the Concept of Political Support," *British Journal of Political Science,* 5 (October 1975), 443–44.

[14] Ibid., p. 444.

[15] For a discussion of this point, see Shanks and Citrin, pp. 15–21.

[16] Any rules regarding the strength of ordinal measures of association are always strictly arbitrary, but, nevertheless, quite necessary for specifying predicted relationships. The following conventions are used in this article: The degree to which participation in aggressive behavior is expected to vary as a function of a predictor variable is defined in terms of the strength of the relationship as measured by an ordinal correlation coefficient. When the correlation (absolute magnitude) does not exceed .10, the strength of the relationship is considered practically zero. A relationship of slight magnitude is defined as a correlation ranging between .10 and .25; moderate magnitude is defined as a correlation ranging from .25 to .50; strong, a correlation ranging from .50 to .75; and very strong, a correlation ranging from .75 to 1.00.

[17] The interview schedule was prepared by the senior author and Jonathan Pool. Helpful advice in preparation of the schedule was received from many

The two major considerations of the research design were (1) to give the political support and opposition variables an opportunity to vary and (2) to investigate the effect of community context on relationships between political attitudes and behavior. Even though most civil disobedience and political violence in the Federal Republic has been manifested in communities with a university present, it was assumed that at least some such behavior also would be picked up in nonuniversity communities which had shown voting support for extremist parties; similarly, it was expected that higher than average support for extremist parties would register in the universities selected for inclusion in the design. A third consideration was to avoid completely sacrificing representativeness at the altar of enhanced variation. While the communities chosen are by no means representative of West Germany as a whole, they do capture basic regional and community-size differences. Moreover, probability samples (supplemented by small samples of community influentials) of the population of each community were drawn so as to achieve representativeness within each research site.

The rural sites are four small villages: Friederichskoog and Neuenkirchen in northern Germany, Erpolzheim and Mauchenheim in southern Germany. From these sites a total of 569 persons were interviewed, of which 479 were drawn randomly from lists of eligible voters, 90 were drawn from lists of community influentials obtained from discussions with the mayor and other community leaders by the chief of the team of interviewers for each site. The urban sites are working-class sections of Bremen in northern Germany and Nürnberg in southern Germany. From these sites a total of 990 persons were interviewed, of which 928 were drawn randomly from lists of eligible voters, 62 were drawn from lists of community influentials compiled from nominations submitted by persons in the eligible voters sample who were active in local organizations. The university sites are six of the major universities in West Germany: Berlin, Bochum, Frankfurt, Heidelberg, Köln, and München. A total of 1104 students and faculty from the arts and sciences at these universities were interviewed, of which 956 were drawn by quota sampling, 148 were drawn from lists of influential persons in various university organizations compiled from nominations submitted by persons in the quota sample.[18]

Measurement of Political Affect

The concepts of effectiveness and specific support refer to evaluations of the actual performance of an incumbent administration. To measure affect for the incumbent administration in West Germany (a coalition government of Social Democrats and Free Democrats headed by Willy Brandt) respondents were first asked to sort a deck of 11 cards, each listing one general area of public policy, into two categories: (1) a task of the government and (2) not a task of the government. The policies were:

1. Guaranteeing justice for all.
2. Providing welfare services for all who need them.
3. Effectively combating pollution.
4. Creating more opportunities for all citizens to participate in making political decisions.
5. Guaranteeing protection and security for individuals.
6. Providing for economic stability.
7. Seeing to it that business corporations, unions, and universities are run in accordance with basic democratic principles.
8. Ensuring a strong national defense.
9. Providing for strong and capable political leadership.
10. Ensuring a free-market economy.
11. Providing for peace and order in society.

For each policy considered to be a task of the government, respondents were asked first to state how important they thought it was, then to grade the present government on its performance in that area. The grading scheme was

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colleagues at the University of Mannheim, especially Rudolf Wildenmann and Uwe Schleth, and from Yola Laupheimer, director of the field work, and Dorothea Reppart, assistant director, both of the Economics Research Bureau, *Infratest* GmbH & Co., Munich.

[18] A quota sample was used to alleviate previous sampling problems. In early studies of students, it was very difficult to acquire a proper sampling frame. For a detailed report of the sampling procedures, see the studies available from Yola Laupheimer or Dorothea Reppart at *Infratest.*

that used in the public schools (a 5-point scale, ranging from 1 through 5, where "1" stands for excellent work and "5" for failure). A mean score was computed across those policy areas considered to be a task of the government. This score was rounded to the nearest integer in the range 1–5. These mean policy evaluation scores constitute one measure of incumbent affect, the Policy Output Evaluation index.

After grading the incumbent government on its performance in particular policy areas, respondents gave the government an overall grade, without regard to specific policies. This single-item indicator, a second measure of incumbent affect, has a range of 1 to 5. We label it Incumbent Evaluation.

The concepts of legitimacy and diffuse support refer to affect for the political system. In Lipset's conceptualization, legitimacy is tied to affect for political institutions. Easton's conceptualization of diffuse support is more inclusive than this, encompassing affect for authorities in general, for the values, norms, and institutions of the regime, and for the political community.[19] But legitimacy is seen as a fundamental component of diffuse support. Defined as belief that the authorities and regime "in some vague or explicit way . . . [conform to a person's] own sense of what is right and proper in the political sphere," the inculcation of legitimacy sentiment is regarded as "the single most effective device for regulating the flow of diffuse support in favor both of the authorities and of the regime."[20]

If one approaches the problem of operationalizing the idea of affect for the political system from the perspective of the broad definition of legitimacy advanced by Easton, system affect can be seen to entail a number of components, each of which may, from slightly different angles, tap into beliefs that authorities and regime conform to a person's sense of what is right and proper. One component would be affect tied to evaluation of how well political institutions conform to a person's sense of what is right. Another component would be affect tied to evaluation of how well the system of government upholds basic political values in which a person believes. A third component would be affect tied to evaluation of how well the authorities conform to a person's sense of what is right and proper behavior.

To measure system affect in the sense of belief in the legitimacy of the system of government respondents were given a deck of cards each containing a statement designed to capture an aspect of legitimacy sentiment. They were asked to report how strongly they agreed or disagreed with each statement, using a 7-point scale which ranged from "+3," labeled "agree completely," to "–3," labeled "disagree completely." English translations of the statements are given in Table 1.[21]

Items S_1, S_3, and S_4 refer to affect tied to evaluation of how well the system upholds basic values: S_1 is intended to capture system affect stemming from a person's belief that the political system does or does not operate in a way that is consonant with his basic values; S_3 and S_4 are intended to capture system affect which stems from belief that the political system does or does not operate in such a way as to uphold presumably consensual political values such as representation of citizens and protection of basic rights due to them.[22] Items S_2 and S_5 are intended to capture system affect stemming from a person's belief that political institutions and the political system in general do or do not conform to his own sense of what is right and proper.[23] Items S_6, S_7, and S_8 refer to affect tied to broad classes of political authorities: politicians, the courts, and the police. The courts and the police are included because, as Easton and Dennis have argued in the context of the United States, such authorities symbolize the law; and in a constitutional, legal society (the Federal Republic is even more of a legalistic society than the United States) feelings about the legitimacy of the courts and police are probably bound up fairly closely with belief in the legitimacy of political authority in general.[24] These items are intended to capture system affect which stems from belief that the authorities in general do or do not conform to a person's sense of what is right and proper behavior: Have political authorities in the past been well-intentioned, regardless of their actual performance? Have the courts and the police wielded their power properly, i.e., in the manner prescribed by their roles in the authority structure?

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[19] In Easton's terms, diffuse support "is not contingent on specific inducements or rewards of any kind, except in the very long run. On a day-to-day basis, if there is a strong inner conviction of the moral validity of the authorities or the regime, support may persist even in the face of repeated deprivations attributed to the outputs of the authorities or their failure to act." Ibid., p. 278.

[20] Ibid.

[21] The German test of the items is: (S_1) Es macht mir Sorge, wenn ich an den Unterschied zwischen dem denke, was Leute, wie ich, im Leben wollen, und was tatsächlich in unserem politischen System geschieht"; (S_2) "Die politischen Einrichtungen der Bundesrepublik sind mir lieb und wert und ich achte sie hoch"; (S_3) "Meine Freunde und ich fühlen uns in unserem politischen System eigentlich doch sehr gut vertreten"; (S_4) "Ich bin immer wieder erschrocken und betroffen darüber, dass die wesentlichen Rechte der Bürger in der deutschen Politik so wenig beachtet werden"; (S_5) "Heutzutage bin ich gegenüber unserem politischen System sehr kritisch eingestellt"; (S_6) "Die Gerichte in der Bundesrepublik gewähren jedermann einen fairen Prozess–es spielt dabei keine Rolle, ob er arm oder reich, gebildet oder ungebildet ist"; (S_7) "Die Grundeinstellung der Leute, die bisher in der Bundesrepublik politisch tonangebend waren, war immer in Ordnung"; (S_8) "Alles in allem genommen, verdient, die Polizei in der Bundesrepublik grossen Respekt."

[22] Two of these items are analogous in tone and content to items used in a measure called Political Alienation from the National Polity, developed in the United States by David C. Schwartz, *Political Alienation and Political Behavior* (Chicago: Aldine, 1973). Item S_1 is similar to: "I generally share the basic values exhibited in the policies and processes of American politics" (p. 265); item S_3 is similar to: "I feel that my friends and I are still really represented in our government" (p. 266).

Thus, affect for the political system, as viewed here, is a wide-ranging, multifaceted concept, for which single-item indicators are inappropriate. Our goal in developing a measure of system affect was to identify a set of indicators of its different analytical components that (1) would show a pattern of at least moderate interitem correlations (no item correlating with a majority of the others at less than .3), and (2) a high overall reliability coefficient. The correlation matrix for those items which conformed to the desired pattern of interitem correlations is shown in Table 1.[25] Their reliability coefficient yields an estimated correlation of test scores with true scores that

Table 1. Intercorrelation Matrix *(r)*: Political Support Items

	Political Support Items	S_1	S_2	S_3	S_4	S_5	S_6	S_7	S_8
S_1:	It makes me concerned when I think about the difference between what people like me value in life and what actually happens in our political system.	1.00	−.26	−.37	.41	.39	−.25	−.34	−.25
S_2:	I have great respect and affection for the political institutions in the Federal Republic.		1.00	.58	−.34	−.31	.40	.49	.55
S_3:	My friends and I feel that we are quite well represented in our political system.			1.00	−.36	−.37	.41	.50	.43
S_4:	I find it very alarming that the basic rights of citizens are so little respected in our political system.				1.00	.36	−.26	−.30	−.22
S_5:	At present, I feel very critical of our political system.					1.00	−.21	−.32	−.22
S_6:	The courts in the Federal Republic guarantee everyone a fair trial regardless of whether they are rich or poor, educated or uneducated.						1.00	.40	.47
S_7:	Looking back, the leading politicians in the Federal Republic have always had good intentions.							1.00	.47
S_8:	Considering everything, the police in the Federal Republic deserve great respect.								1.00

Note: Kuder-Richardson Reliability Coefficient (KR-20), $r_{kk} = .822$; estimated correlation of test with true scores, $\sqrt{r_{kk}} = .907$.

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[23] These items are analogous to two items used by Schwartz in a measure called Conformity-Acceptance of the American National Polity: "If people would only use the laws and institutions of our political system they would find that the system really works quite well"; "I used to accept the American political system more fully than I do now. Today, I feel highly critical of it," Ibid., p. 273.

[24] Easton and Dennis, p. 293.

[25] Two other items in the original political support scale were eliminated. One item ("The real political power in the Federal Republic is held by only a small elite; even citizens who have a great interest in politics have hardly any chance to influence political decisions") correlated less than .25 with all other scale items. The other item ("In general, the police treat everybody in the Federal Republic equally, regardless of whether they are poor or rich, educated or uneducated") was essentially a redundant measure to item S_8 ($r = .63$).

slightly surpasses the desired level of .9.[26] The eight items, scored from 0 to 6, were summed to form an index ranging from 0 to 48, where low scores indicate support for the political system and high scores indicate alienation from the system.[27] This measure of system affect is labeled Political Support.

The fourth type of political affect dealt with in this study is, like Political Support, a putative measure of system affect; known variously as "political trust," "political cynicism," or "trust in government," it was developed by the Survey Research Center at the University of Michigan. Stokes, who first used the SRC "trust in government" items, was relatively unambiguous in specifying the construct which these items were designed to operationalize. According to him, they were included in the SRC biennial American election surveys for the purpose of tapping "evaluative orientations toward the national government."[28] But were these evaluative orientations intended to focus only on an incumbent administration? Or on institutions of the national government as well? Or on even more diffuse system referents? Stokes explicitly cautions the reader against misinterpreting the meaning of the items: "Plainly we ought not to assume that the people expressing negative feeling toward government in this research were calling for fundamental change in the political order."[29]

Despite Stokes' warning, the SRC "trust in government" items often have been interpreted as if they measured affect for the system of government.[30] This may be due in part to the fact that the manifest content of the items appears to tap an aspect of diffuse support, called *personal legitimacy* by Easton, which derives "from the estimate of the personal merit and worth of the authorities rather than only from the validity of their position in the system or their compatibility with the ideological premises of the members."[31] Also, Gamson subsequently defined the concept of political trust as synonomous with diffuse support,[32] and many researchers seeking an operational indicator of Gamson's trust concept—in the sense of diffuse support—seized upon the already available SRC "trust in government" items. Finally, and most importantly, empirical evidence has been forthcoming which suggests that trust in government *is,* in fact, bound up closely with feelings about the legitimacy of the system of government. A number of studies have shown that the "trust in government items" do correlate in the expected direction (negatively) with attitudes toward aggressive political behavior—just as predicted under the assumption that these items, regardless of their manifest content, actually comprise an instrument which captures affect for the political system.[33]

But there is an alternative hypothesis to account for the observed correlation between trust in government and favorable disposition toward aggressive political behavior. It is premised on the assumption that even if trust in government is largely a measure of generalized approval of incumbent politicians, an absence of association or even only a small association between trust in government and an instrument sensitive to system affect would be rather astonishing, since it would be logical to expect that trust in government might be a cause of system affect and/or system affect might condition a person's trust in government. If this assumption held, then an observed correlation between trust and aggressive attitude/behavior could be due to the fact that trust in government was associated with an unmeasured third variable, affect for the political system, which in reality was the true direct cause. In this case, a correlation between trust in government and aggressive attitude/behavior would disappear when system affect was controlled for—because it was either a spurious correlation or only an indirect one. Clearly, the spurious/indirect correlation possibility is a plausible rival hypothesis that must be tested.

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[26] The standard Kuder-Richardson reliability formula was used for computing scale reliabilities. See Allen L. Edwards, *The Measurement of Personality Traits by Scales and Inventories* (New York: Holt, 1970), pp. 20–21; and Nunnally, pp. 191–97. The reliability index indicates the effectiveness of a measuring instrument and is a necessary, though not sufficient, condition for validity. The magnitude of r_{kk} is a function of the homogeneity of the scale and the number of items in the scale. For basic research, reliability coefficients around the order of .80 are considered desirable, because at that level correlations are not attenuated appreciably by measurement error. See Nunnally, p. 226.

[27] Positively worded items were recoded so that all items ranged from positive to negative.

[28] Donald E. Stokes, "Popular Evaluations of Government: An Empirical Assessment," in *Ethics and Bigness: Scientific, Academic, Religious, Political, and Military,* eds. Harlen Cleveland and Harold D. Lasswell (New York: Harper, 1962), p. 64.

[29] Ibid., p. 72.

[30] See, for example, Muller, "Correlates and Consequences"; Muller, "The Representation of Citizens"; Aberbach and Walker; Miller, "Political Issues and Trust"; Miller, "Rejoinder."

[31] Easton, *A System Analysis,* p. 303.

[32] Gamson, p. 45.

[33] See, for example, Aberbach and Walker; Muller, "A Test of a Partial Theory"; Miller, "Rejoinder."

Our measure of political trust is based on four items, English translations of which are given in Table 2, along with the interitem correlation matrix. The reliability coefficient yields an estimated correlation of test scores with true scores which is just slightly under the desired level of .9.

Two of the items in the political trust instrument, T_1 and T_2, are variants of the SRC item which asks respondents directly about their evaluation of the trustworthiness of the national government.[34] T_3 and T_4 were designed to capture beliefs in the integrity of elected representatives and their concern for the public interest in general; they are analogous to SRC items, though developed by Alan Marsh specifically for use in the Nine Nations Project, a cross-national study of values and political behavior in advanced industrial societies.[35] The four trust items, scored from 0 to 4, were summed to form a Political Trust index ranging from 0 (very trusting) to 16 (very distrusting).[36]

For the initial stage of the analysis—investigation of bivariate and first-order multivariate relationships—we divided each variable into five equal "zones" or levels of affect: very positive, positive, intermediate, negative, and very negative.[37] This gives a sufficient number of cases at each zone of affect for reliable statistical analysis, without distorting the full range of the discrete variable.

Table 2. Intercorrelation Matrix *(r)*: Political Trust Items

	Political Trust Items	T_1	T_2	T_3	T_4
T_1:	In general, one can rely on the federal government to do the right thing.	1.00	.45	.29	.34
T_2:	How much do you trust the government in Bonn to act as it really should?		1.00	.55	.54
T_3:	When members of Parliament or cabinet ministers speak on television or in Parliament with journalists, how often in your opinion do they tell the truth?			1.00	.56
T_4:	How much do you trust members of the government to put the interests of the people over the interests of their own parties?				1.00

Note: Kuder-Richardson Reliability Coefficient (KR-20), $r_{kk} = .770$; estimated correlation of test with true scores, $\sqrt{r_{kk}} = .877$.

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[34] One of these items, T_1, was presented to the respondents as part of the cardsort procedure used for the political support items, and scoring was identical to that for the support items. *Cf. supra*, p. 12. The purpose of administering this item with the support indicators was to get at feelings of trust in government as a whole—a different referent from T_2 (the government in Bonn). The modest correlation of the two items indicates they are tapping somewhat different beliefs, although only 14 percent of those scoring in the negative range on the trust-government-in-Bonn item were positive on trust-in-government, while 24 percent of those scoring positive on the former item registered in the negative range on the latter item. Item T was administered identically to T_3 and T_4; see below, fn. 35. The German text of the two items is: (T_1) "Man kann sich im allgemeinen darauf verlassen, dass die Bundesreigierung das Richtige tut"; (T_2) "Wie sehr vertrauen Sie darauf, dass die Regierung in Bonn so handelt, wie sie es eigentlich sollte?"

[35] Alan Marsh is director of the British segment of the project. The German version of these items was prepared by Max Kaase and Hans Klingemann, directors of the German segment of the project. Items T_3 and T_4 were presented with T_2 as part of a series of questions that included personality and social control items. The German text of these two items is: (T_3) "Wenn Bundestagsabgeordnete oder Minister im Fernsehen oder im Parliament mit Journalisten sprechen, wie oft sagen sie Ihrer Meinung nach die Wahrheit?" (T_4) "Wie sehr vertrauen Sie darauf, dass Regierungsmitglieder das Wohl des Volkes über die Interessen ihrer eigenen politischen Partei stellen?"

[36] To attain comparability with the distributions of the other three trust measures, the response scores on either side of the neutral value for T_1 were collapsed (+1 and −1 with 0), providing for five response types in all. Response options for items T_2, T_3, and T_4 were: "about always," "mostly," "sometimes," "only very seldom," "never."

[37] For both the Policy Output Evaluation and the Incumbent Evaluation variables the "very positive" to "very negative" zones correspond to each integer from 1 to 5 in ascending order. For the Political Support index, the "very positive" zone includes scores from 0

Correlates of Political Affect

Our basic hypothesis about the connection between the measures of political affect is that they will show at least a moderate degree of fit to a strict monotonic function: as affect on any one variable becomes more negative, affect on any other variable will become more negative. There are two reasons why this hypothesis might hold. First, political affect could be unidimensional; if so, these variables would be correlated because each was a slightly different indicator of the same thing. Second, political affect could be multidimensional, but there might be causal links between dimensions; if so, these variables would be correlated due to asymmetric or symmetric dependencies. In particular, the two dimensions of affect might be those postulated analytically: evaluation of incumbent performance and support for the system of government. And these might be correlated because how one evaluates incumbent performance has an effect on one's support for the system of government, or because one's support for the system of government conditions how one views incumbent performance, or because both effects occur simultaneously or reciprocally. The upshot is that while the absence of correlation (at least for variables stipulated as incumbent affect versus those stipulated as system affect) would support the validity of the incumbent-system distinction, the presence of correlation would not militate against it.

As Table 3 shows, a rather sizable Tau_b correlation obtains between Political Support and Political Trust. These variables covary according to a strict monotonic function more closely than do Political Support and Incumbent Evaluation or Policy Output Evaluation. By comparison, 41.6 percent of those in the "very positive" zone on Political Trust score at the exact same zone on Political Support, whereas only 22.7 percent of those in the "very positive" zone on Incumbent Evaluation and only a miniscule 3.6 percent of those "very positive" on Policy Output Evaluation also score at "very positive" on Political Support. Reading up the diagonal, on which all persons would be located if the relationship conformed to a strict monotonic function, a majority of those at "positive," "intermediate," and "very negative" Political Trust also manifest the corresponding level on Political Support, but there is no instance of a majority at any level of Incumbent Evaluation manifesting the corresponding level on Political Support, and a majority only of those who score "very negative" on Policy Output Evaluation also manifest the corresponding level on Political Support.

The empirical correlation between Political Support and Political Trust is stronger than that between Political Support and either Incumbent Evaluation or Policy Output Evaluation. Is this evidence that the Political Trust variable is more a measure of system than of incumbent affect? As we have already suggested, one should be wary of such an interpretation because of the possibility that Political Trust is just a measure of incumbent affect related more powerfully to Political Support either as cause or effect than are other measures of incumbent affect. Moreover, the data in Table 4 sound an empirical note of caution. Political Trust and Incumbent Evaluation—the variable tied most directly to straight incumbent affect from the standpoint of manifest content alone—show a healthy fit to a strict monotonic function. On the other hand, the relationship between Political Trust and Policy Output Evaluation is similar to that between Political Support and Policy Output Evaluation.

In sum, the relationships between these measures of political affect are in line with our initial hypothesis that they will be intercorrelated. But as to the question of the incumbent-system distinction, and where to place Political Trust if such a distinction can be made, the evidence is inconclusive.

To generate more conclusive evidence on the incumbent-system distinction we must turn to other variables which should relate to a measure of incumbent affect differently than to a measure of system affect. Ideology is a prime candidate. If one had a polity where an incumbent administration was widely perceived as differing from the overall system of government on a left-right continuum, then it would be logical to hypothesize that a measure of incumbent affect would show a different relationship to self-designated ideological position than would a measure of system affect. For example, let us suppose that a leftist political party is elected to power in a regime long dominated by rightists. Under this condition, what Easton calls the *operating* values and

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to 9; "positive" includes scores from 10 to 19; "intermediate" includes scores from 20 to 29; "negative" includes scores from 30 to 39; "very negative" includes scores from 40 to 48. For the Political Trust index the "very positive" zine includes scores from 0 to 3; "positive" includes scores from 4 to 6; "intermediate" includes scores from 7 to 9; "negative" includes scores from 10 to 12; "very negative" includes scores from 13 to 16.

Table 3. Relationship Between Political Support and Political Trust, Incumbent Evaluation, Policy Output Evaluation

Political Support	Very Positive		Positive		Intermediate		Negative		Very Negative	
	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)
					Political Trust					
Very negative	0	(0)	0.4	(2)	1.9	(19)	14.5	(84)	55.6	(85)
Negative	0	(0)	4.5	(21)	19.1	(192)	34.0	(197)	24.2	(37)
Intermediate	22.5	(20)	37.1	(173)	52.3	(526)	40.3	(234)	17.6	(27)
Positive	36.0	(32)	50.4	(235)	24.6	(247)	10.5	(61)	2.6	(4)
Very positive	41.6	(37)	7.5	(35)	2.2	(22)	0.7	(4)	0	(0)
TOTAL	100	(89)	100	(466)	100	(1006)	100	(580)	100	(153)
					$\text{Tau}_b = .48$					
					Incumbent Evaluation					
Very negative	4.5	(2)	0.9	(5)	5.4	(55)	16.0	(69)	42.0	(58)
Negative	6.8	(3)	13.4	(77)	19.6	(201)	28.1	(121)	23.2	(32)
Intermediate	27.3	(12)	37.5	(216)	47.6	(489)	43.6	(188)	25.4	(35)
Positive	38.6	(17)	38.2	(220)	24.9	(256)	12.1	(52)	8.7	(12)
Very positive	22.7	(10)	10.1	(58)	2.6	(27)	0.2	(1)	0.7	(1)
TOTAL	100	(44)	100	(576)	100	(1028)	100	(431)	100	(138)
					$\text{Tau}_b = .34$					
					Policy Output Evaluation					
Very negative	1.8	(1)	1.1	(6)	4.4	(52)	19.4	(90)	69.5	(41)
Negative	16.1	(9)	10.1	(53)	20.3	(238)	30.8	(143)	13.6	(8)
Intermediate	35.7	(20)	38.6	(203)	48.8	(571)	37.0	(172)	13.6	(8)
Positive	42.9	(24)	38.8	(204)	23.8	(278)	12.7	(59)	3.4	(2)
Very positive	3.6	(2)	11.4	(60)	2.6	(31)	0.2	(1)	0	(0)
TOTAL	100	(56)	100	(526)	100	(1170)	100	(465)	100	(59)
					$\text{Tau}_b = .35$					

Table 4. Relationship Between Political Trust, Incumbent Evaluation, and Policy Output Evaluation

Political Trust	Very Positive		Positive		Intermediate		Negative		Very Negative	
	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)
					Incumbent Evaluation					
Very negative	4.2	(2)	0.5	(3)	3.0	(33)	11.8	(53)	42.0	(60)
Negative	4.2	(2)	13.0	(77)	21.4	(234)	45.6	(205)	42.7	(61)
Intermediate	16.7	(8)	39.5	(234)	53.1	(580)	38.9	(175)	15.4	(22)
Positive	41.7	(20)	37.4	(222)	20.8	(227)	3.3	(15)	0	(0)
Very positive	33.3	(16)	9.6	(57)	1.7	(19)	0.4	(2)	0	(0)
TOTAL	100	(48)	100	(593)	100	(1093)	100	(450)	100	(143)
					$\text{Tau}_b = .44$					
					Policy Output Evaluation					
Very negative	1.4	(1)	1.3	(7)	3.5	(43)	13.4	(64)	66.7	(38)
Negative	20.8	(15)	14.2	(79)	23.2	(281)	44.5	(212)	26.3	(15)
Intermediate	37.5	(27)	40.2	(223)	50.5	(613)	35.9	(171)	7.0	(4)
Positive	30.6	(22)	33.9	(188)	20.8	(252)	5.7	(27)	0	(0)
Very positive	9.7	(7)	10.5	(58)	2.0	(24)	0.4	(2)	0	(0)
TOTAL	100	(72)	100	(555)	100	(1213)	100	(476)	100	(57)
					$\text{Tau}_b = .36$					

norms of the system[38] would be heavily right-of-center in tone, whereas the tenor of the incumbent administration would be left-of-center. Then we would expect persons who designate themselves as leftist to register more positively than rightists on a measure of incumbent affect, while persons who designate themselves as rightist should register more positively than leftists on a measure of system affect.

Fortunately for our purposes, it is probably fair to characterize the Federal Republic of Germany today as a polity with a right-of-center regime and a left-of-center incumbent administration. The SPD, or socialist party, is currently in power.[39] But the regime began under the auspices of an American-dominated military government and for the first decade and a half of its existence was run by Adenauer's conservative party, the CDU/CSU.

Obviously, not everyone in Germany perceives the incumbent administration as leftist and the system of government as rightist; but if it is fair to expect that at least a substantial majority share this view, then our hypotheses with regard to ideology may be formulated as:

(I_1): Measures especially sensitive to affect for an incumbent administration will register more highly in the negative range among rightists than will measures especially sensitive to affect for the system of government.

(I_2): Measures especially sensitive to affect for the system of government will register more highly in the negative range among leftists than will measures especially sensitive to affect for an incumbent administration.

To test these hypotheses we use the left-right scale, previously shown to be a meaningful yardstick in the German context by Hans Klingemann.[40] This variable, which we call Ideological Commitment, is a horizontal, visual scale consisting of ten boxes stretching from "Left" to "Right" (so labeled at the extremes). We have divided it into five equal categories, ranging from "extreme Left" to "extreme Right."

As Table 5 shows, 23.9 percent of those who locate themselves in the "extreme Right" position on Ideological Commitment are negative on Political Support. By contrast, almost a majority of the extreme Right (49.2 percent) is negative on Incumbent Evaluation. Turning to the "Right" category on Ideological Commitment, we see that only 13.4 percent express negative political support, whereas 37.7 percent give the present government a negative grade. As predicted by the first hypothesis, the measure which we think captures system affect registers far less negative sentiment among rightists than does the measure which, on the basis of manifest content, obviously taps incumbent affect. And where does Political Trust fall? A full 52.8 percent of the extreme Right and 31.6 percent of the Right register negative trust. These rates of negative affect are about the same as those registered by rightists on Incumbent Evaluation, and they are more than twice as great as the rates of negative affect registered by rightists on Political Support. At this end of the Left-Right continuum, trust very much resembles the Incumbent Evaluation variable.

Now what about the Left? According to the second hypothesis, leftists should be more negative on Political Support than on Incumbent Evaluation—*if* these variables capture what we think they do. Our alleged system measure does, in fact, register considerably more negative sentiment on the Left than does our alleged incumbent measure: 69.8 percent of the extreme Left register negative political support, but their rate of negative sentiment for the incumbent administration is some 20 percentage points lower; 37.8 percent of the Left register negative support, but less than half this many register negative sentiment for the incumbent administration. And on Political Trust: For the extreme Left, the rate of negative sentiment on Political Trust is 56.6, quite close to their rate of negative sentiment on Incumbent Evaluation. But among the Left, the rate of negative sentiment on Political Trust (27.4 percent) lies almost exactly between their rate of negative sentiment on Political Support and their rate on Incumbent Evaluation. Again, trust looks more like the Incumbent Evaluation variable than the Political Support variable, though the differentiation is not quite as sharp on the Left as on the Right.

The relationship between Policy Output Evaluation and Ideological Commitment is, as would be expected, more similar to that which holds for Incumbent Evaluation and ideology than to that between Political Support and ideology. Among those at the "extreme Left" position, 51.1 percent register negative affect on the Policy Output Evaluation variable—a figure almost identical to the rate of negative sentiment which they register on Incumbent Evaluation (48.6 percent); likewise, those at the "Left" position register a rate of negative affect on Policy Output Evaluation (18.9 percent) which is almost the same as their negative rate on Incumbent Evaluation (16.3 percent). Rightists, however, register rates of negative affect on Policy Output Evaluation which lie between their comparatively high negative rates on the Incumbent Evaluation variable and their comparatively low negative rates on the Political Support variable. Of those at the "Right" position on Ideological Commitment, 23.4 percent express negative affect with regard to policy performance; 31.4 percent of those at the "extreme Right" position on Ideological Commitment are negative on the policy variable. The Right appears to be more negative toward the leftist incumbent administration in an "expressive" sense (the overall grade they give the incumbents, regardless of policies) than in an "instrumental" sense (the grade they give specifically on policy performance).

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[38] Easton, *A System Analysis*, pp. 289–90.

[39] The SPD administration also includes the FDP (Free Democrats), a small centrist party, as a coalition partner.

[40] See his "Testing and the Left-Right Continuum on a Sample of German Voters," *Comparative Political Studies*, 5 (April 1972), 93–106.

We have found that leftists and rightists register consistent and in many instances rather sharp differences on the measures of political affect, differences which coincide with our predictions under the assumption that the Political Support variable is especially sensitive to affect for the system of government while the Policy Output Evaluation and Incumbent Evaluation variables are especially sensitive to affect for an incumbent administration. The Political Trust variable, despite its fairly close correlation with Political Support, relates to ideology in much the same way as the Policy Output Evaluation and Incumbent Evaluation variables. Thus, the analytic incumbent-system distinction does appear to be grounded in reality, and we can sort our measures of political affect according to it. But does it help us in any important way to account for political behavior?

Political Affect and Aggressive Political Behavior

We have formulated a theory about behavioral consequences of the incumbent-system distinction, suggested by the work of Lipset and Easton, in terms of four abstract propositions. The dependent variable is aggressive political behavior: that is, political behavior that is illegal, disruptive of the normal functioning of government, and may entail the use of violence.

Table 5. Relationship Between Political Support, Political Trust, Incumbent Evaluation, and Ideological Commitment

	Ideological Commitment									
	Extreme Left		Left		Center		Right		Extreme Right	
	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)
Political Support										
Very negative	41.4	(92)	9.3	(71)	0.8	(6)	0.9	(3)	8.0	(9)
Negative	28.4	(63)	28.0	(213)	12.3	(89)	12.5	(41)	15.9	(18)
Intermediate	18.9	(42)	38.0	(289)	49.9	(361)	52.9	(173)	45.1	(51)
Positive	6.3	(14)	21.0	(160)	31.2	(226)	31.2	(102)	24.8	(28)
Very positive	5.0	(11)	3.7	(28)	5.8	(42)	2.4	(8)	6.2	(7)
TOTAL	100	(222)	100	(761)	100	(724)	100	(327)	100	(113)
Political Trust										
Very negative	29.7	(65)	4.2	(33)	2.4	(18)	4.0	(14)	8.9	(11)
Negative	26.9	(59)	23.2	(184)	22.4	(171)	27.6	(96)	43.9	(54)
Intermediate	27.4	(60)	46.0	(364)	46.1	(351)	46.8	(163)	35.0	(43)
Positive	7.8	(17)	23.0	(182)	25.1	(191)	18.7	(65)	10.6	(13)
Very positive	8.2	(18)	3.7	(29)	4.1	(31)	2.9	(10)	1.6	(2)
TOTAL	100	(219)	100	(792)	100	(762)	100	(348)	100	(123)
Incumbent Evaluation										
Very negative	19.6	(44)	2.1	(16)	3.9	(29)	7.0	(24)	15.3	(18)
Negative	29.0	(65)	14.2	(111)	15.0	(110)	30.7	(106)	33.9	(40)
Intermediate	33.0	(74)	49.3	(384)	48.7	(358)	49.3	(170)	38.1	(45)
Positive	15.2	(34)	31.8	(248)	30.3	(223)	12.8	(44)	11.0	(13)
Very positive	3.1	(7)	2.6	(20)	2.0	(15)	0.3	(1)	1.7	(2)
TOTAL	100	(224)	100	(779)	100	(735)	100	(345)	100	(118)

Muller has shown that the relationship between political support and aggressive political behavior conforms to a corner correlation pattern: At high and medium levels of political support, aggressive political behavior is very unlikely; only at low political support does aggressive political behavior show a tendency to occur, but even under this condition other responses such as behavioral withdrawal (complete inactivity) or conformative participation are also quite likely to occur.[41] In other words, low political support is a necessary but not sufficient condition of aggressive political behavior.

In our analysis of relationships between the political affect variables and aggressive political behavior, we shall ask these questions of the data. First, is the degree of fit to the "necessary but not sufficient" correlation pattern weakened or strengthened according to (1) whether the political affect variable is an incumbent or a system measure, and according to (2) the different combinations of incumbent and system affect specified in the theory about their joint effect on aggressive political behavior?

Space limitation precludes analysis in this report of the typology of political action developed by Muller.[42] This typology is based on combinations of aggressive and conventional behavior. People who manifest low, medium, or high levels of aggressive action are differentiated according to whether or not they also participate actively in conventional electoral politics. These distinctions are important with regard to a general theory of mass political behavior. But they are not germane to the purpose at hand which is to determine the kinds and combinations of kinds of political affect yielding the sharpest prediction of aggressive political behavior per se.

Measurement of Aggressive Political Behavior. The measure of the dependent variable was obtained by the following procedure. Respondents were given a set of cards. On each card a different kind of behavior was listed.[43] The interviewer explained that these behaviors were actions which one might take in order to influence the government. Respondents were asked to indicate (1) whether or not they approved of each behavior; (2) how large a percentage, in their opinion, of citizens in the Federal Republic would approve of that behavior; (3) whether or not they themselves *would* engage in the behavior; and (4) whether or not they *had* engaged in the behavior. Before each question, the cards were shuffled.

The set of cards contained five behaviors which were not aggressive, according to the definition given above, and five behaviors which were aggressive. The aggressive actions, in English translation, were: (1) refusal to pay rents or taxes; (2) participation in a wildcat strike; (3) takeover of factories, offices, and other buildings; (4) participation in fights (battle with police, battle with other demonstrators); (5) participation in a group which wants to dislodge the government by violent means.[44] Participation in each activity was classified as "negative" if respondents said they would not do it; "transitional" if the respondents said that they might or definitely would do it, but had not yet done it; "positive" if they said that they might or definitely would do it and had already done it.[45] A composite Aggressive Behavior index was constructed by assigning a score of "0" to the "negative" category, a score of "1" to the "transitional" category, a score of "2" to the "positive" category, then summing scores across the five items.

There are two reasons for adopting this particular scoring procedure. First of all, persons who have done an action and would do so again ought to be differentiated from those who would *not* repeat the action. If one desires to predict future participation in aggressive behavior, then high scores on such an index should not be contaminated by persons who have no intention of repeating an action which they once did in the past. Second, and most importantly, it is theoretically desirable to combine intentions and behavior into a composite index. We conceptualize an aggressive political behavior continuum as ranging through four distinct zones. This conceptualization is supported by our data. (For further details see the analysis of components of the Aggressive Behavior index given in the Appendix). The lowest zone is characterized by inactivity and negative intention toward participation in aggressive action. As one moves higher on the continuum, the inactivity zone shades into positive intention for nonviolent aggressive action but little or no participation in such action. Moving still higher, the potential nonviolent civil disobedience zone shades into a zone characterized by actual participation in civil disobedience with progressively greater positive intention to engage in violent aggressive action. Finally, this civil disobedience/potential violence zone shades into the highest zone, characterized by actual participation in political violence.

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[41] Edward N. Muller, "Behavioral Correlates of Political Support," *American Political Science Review*, 72 (June 1977), 454–67.

[42] Ibid.

[43] The phrasing for many of these items comes from a pretest questionnaire developed for the German segment of the Nine Nation Study which is being carried out in West Germany under the direction of Max Kaase and Hans Klingemann of the Zentrum für Umfragen, Methoden und Analysen, Mannheim.

[44] The German test of these behaviors is: (1) "Sich weigern, Mieten, Raten oder Steuern zu bezahlen"; (2) "Beteiligung an einem wilden Streik"; (3) "Besetzung von Fabriken, Ämtern und anderen Gebäuden"; (4) "Beteiligung an Schlägereien (Kampf mit Polizisten, Kampf mit anderen Demostranten"; (5) "Beteiligung an einer Gruppe, die Regierung mit Gewalt stürzen will."

[45] Missing data responses were treated in the following manner: Among those respondents refusing to say whether or not they had done the behavior, those who said they would do it or might do it were classified as "transitional." Respondents who did not say whether they would do it or might do it were treated as having missing data. Individuals with no more than two missing data responses on the activity items were assigned the median score value for each activity in which a missing response occurred. Respondents with missing data on more than two activities were excluded from this analysis.

Relationships Between Political Affect Variables and Aggressive Behavior. We know that the relationship between attitude and behavior in general is influenced by situational variables, and we can expect that the relationship between our measures of political affect and the Aggressive Behavior index will be subject to such contextual influences, captured in large part, perhaps, by the community-type "stratification" factor in the research design for this investigation. Prior to cutting the sample by community type, though, it is useful to look at the overall relationships between the political affect variables and the Aggressive Behavior index; then we can assess community type as a factor which may exert a specification effect on the relationships.

Given that we expect to find association between the political affect variables and the Aggressive Behavior index, what *form* do we expect the relationship to take? Since we are talking about an index of behavior which can be quite costly, especially at the higher levels, we do not expect the relationship to conform to a strict monotonic function according to which the cases would tend to fall on the diagonal of a contingency table. Rather, we expect that it will conform to a pattern of corner correlation where cases would tend to be located along two adjacent edges of a contingency table—the necessary but not sufficient correlation pattern.[46] A summary measure of correlation for ordinal variables which is sensitive to the kind of relationship we have in mind—a monotonic nondecreasing relationship such that as one variable increases another variable either stays the same or increases—is the Gamma coefficient.

The relationships between the political affect variables and the Aggressive Behavior index are given in Table 6. In the case of Political Support, the fit to a corner correlation pattern is quite strong in and of itself, and the strength of the relationship between support and the Aggressive Behavior index is far greater than that which obtains for any of the other measures of political affect. The dropoff in the rate of very low aggressive political behavior as one moves from the "very positive" zone of Political Support to the "very negative" zone is striking, with an especially precipitous decline from the "intermediate" zone—where the rate of inactivity on the Aggressive Behavior index is fully 75 percent—to the "very negative" zone—where the rate of inactivity is only 16 percent or barely one-fifth as large. High Civil Disobedience and Political Violence are virtually nonexistent throughout the "very positive" to "intermediate" zones of Political Support. This is very similar to the relationship found in the pilot study for this project, where cruder measures of support and aggressive political behavior were used.[47] Low or negative support is very close to being a necessary condition for actual participation in actions of civil disobedience and political violence. And with this more comprehensive measure of system affect, when we differentiate between the "negative" and "very negative" zones, the latter begins to tend toward being a necessary *and* sufficient condition of actual participation in aggressive action, as evidenced by the fact that almost two-thirds (62.4 percent) of those in the "very negative" support zone also score in the High Civil Disobedience and Political Violence zones on the Aggressive Behavior index. Thus, the observed relationship between Political Support and the Aggressive Behavior index actually corresponds to an ordinal analogue of a positively accelerated power function, according to which, at the most negative zone of support, the likelihood of actual aggressive action is strong. This pattern of relationships, of course, has considerably more powerful causal implications than the corner correlation pattern. Of the four relationships between aggressive behavior and political affect, that with Political Support as the measure of affect is the only one which corresponds well to the positively accelerated power function pattern.

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[46] A good discussion of corner correlation appears in Johan Galtung, *Theory and Methods of Social Research* (Oslo: Universitets Forlaget, 1967), p. 223.

[47] See Table 4 in Muller, "Behavioral Correlates of Political Support," p. 461.

When Political Support is controlled for, the moderate zero-order correlations between the other political affect variables and the Aggressive Behavior index are simply washed out, as evidenced by the consistently low partial correlations. But when the other political affect variables are controlled for, the correlation between Political Support and the Aggressive Behavior index remains quite strong. Table 7 shows this.[48]

Political Trust does not correlate with the Aggressive Behavior index at any level of Political Support except the most negative. In this it also resembles Incumbent Evaluation and Policy Output Evaluation, thus giving further indication that Political Trust is especially sensitive to incumbent rather than system affect—or whatever it is that differentiates Incumbent Evaluation and Policy Output Evaluation from Political Support. However, the trust and policy variables do appear to have a modest specification effect on the relationship between Political Support and the Aggressive Behavior index, depressing the relationship

Table 6. Aggressive Behavior Index as a Function of Political Affect Variables

Aggressive Behavior Index	Very Positive %	Very Positive (N)	Positive %	Positive (N)	Intermediate %	Intermediate (N)	Negative %	Negative (N)	Very Negative %	Very Negative (N)
					Political Support					
Violence	0	(0)	0	(0)	0.5	(5)	1.6	(7)	7.9	(15)
High C.D.	2.0	(2)	1.9	(11)	6.7	(65)	25.6	(113)	54.5	(103)
Low C.D.	8.0	(8)	8.8	(50)	18.1	(176)	25.9	(114)	21.2	(40)
Inactive	90.0	(90)	89.3	(508)	74.7	(728)	46.9	(207)	16.4	(31)
TOTALS	100	(100)	100	(569)	100	(974)	100	(441)	100	(189)
					Gamma = .66					
					Political Trust					
Violence	0	(0)	0.2	(1)	0.5	(5)	1.2	(7)	8.6	(13)
High C.D.	2.1	(2)	5.4	(27)	9.5	(101)	17.6	(105)	40.4	(61)
Low C.D.	9.4	(9)	11.7	(59)	18.1	(192)	17.3	(103)	13.2	(20)
Inactive	88.5	(85)	82.7	(416)	71.9	(761)	63.9	(381)	37.7	(57)
TOTALS	100	(96)	100	(503)	100	(1059)	100	(596)	100	(151)
					Gamma = .38					
					Incumbent Performance Evaluation					
Violence	0	(0)	0.2	(1)	0.5	(5)	2.8	(12)	6.2	(9)
High C.D.	6.8	(3)	7.9	(47)	10.4	(112)	19.8	(86)	31.0	(45)
Low C.D.	2.3	(1)	13.9	(83)	19.0	(204)	18.4	(80)	9.7	(14)
Inactive	90.9	(40)	78.0	(465)	70.1	(754)	59.1	(257)	53.1	(77)
TOTALS	100	(44)	100	(596)	100	(1075)	100	(435)	100	(145)
					Gamma = .30					
					Policy Output Evaluation					
Violence	0	(0)	0.2	(1)	0.6	(7)	2.6	(12)	12.1	(7)
High C.D.	0	(0)	5.6	(31)	11.0	(131)	22.0	(103)	51.7	(30)
Low C.D.	5.9	(4)	11.0	(61)	20.1	(239)	17.1	(80)	12.1	(7)
Inactive	94.1	(64)	83.3	(463)	68.3	(814)	58.4	(274)	24.1	(14)
TOTALS	100	(68)	100	(556)	100	(1191)	100	(469)	100	(58)
					Gamma = .42					

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[48] When levels on a control variable are actually held constant, the Gamma coefficient is not reported if there is practically zero covariation on the Aggressive Behavior index, i.e., under 10 cases scoring at levels other than Inactivity.

when they are positive, facilitating it when they are negative.

Specification Effect of Ideology. A control variable of potentially crucial importance is ideology.[49] As Miller has pointed out, "Ideology can be expected to condition the predisposition to participate in protest behaviors."[50] Ideologies are behaviorally charged belief systems: They include prescriptions for and against various kinds of political action. In Germany today, though not of course in the past, most civil disobedience and political violence has come from the Left. Most of those who designate themselves as leftist on the Ideological Commitment variable probably share what Miller calls a "social change" orientation, whereas most of those who designate themselves as rightist probably share a "social control" orientation.[51] If aggressive political behavior is more acceptable to those holding a social change orientation than to those holding a social control orientation, then Ideological Commitment should have a specification effect such that the relationship between political affect and the Aggressive Behavior index will be

Table 7. Correlations (γ) Between Aggressive Behavior and Political Affect, Controlling for Each Political Affect Variable

	Correlation Between Aggressive Behavior Index and:					
Controlling for: Political Support	**Political Trust**		**Incumbent Evaluation**		**Policy Output Evaluation**	
Very positive	–.07	(98)	.14	(97)	–.33	(94)
Positive	.03	(562)	.13	(543)	.22	(551)
Intermediate	–.09	(946)	–.12	(909)	.02	(939)
Negative	–.15	(425)	.00	(413)	.18	(428)
Very negative	.48	(186)	.40	(186)	.44	(186)
	Partial γ = –.06		Partial γ = –.04		Partial γ = .10	
Political Trust	**Political Support**		**Incumbent Evaluation**		**Policy Output Evaluation**	
Very positive	.22	(87)	.02	(89)	–.09	(88)
Positive	.54	(455)	.00	(461)	.38	(470)
Intermediate	.60	(969)	.08	(977)	.22	(990)
Negative	.63	(558)	.17	(557)	.29	(575)
Very negative	.89	(148)	.27	(147)	.46	(148)
	Partial γ = .61		Partial γ = .10		Partial γ = .26	
Incumbent Evaluation	**Political Support**		**Political Trust**		**Policy Output Evaluation**	
Very positive	—	(42)	—	(43)	—	(42)
Positive	.57	(563)	.16	(574)	.28	(572)
Intermediate	.61	(1000)	.31	(1051)	.30	(1027)
Negative	.72	(407)	.33	(422)	.41	(413)
Very negative	.79	(136)	.64	(141)	.52	(141)
	Partial γ = .62		Partial γ = .29		Partial γ = .32	
Policy Output Evaluation	**Political Support**		**Political Trust**		**Incumbent Evaluation**	
Very positive	—	(49)	—	(63)	—	(56)
Positive	.45	(513)	.16	(535)	.08	(504)
Intermediate	.57	(1128)	.23	(1157)	.08	(1129)
Negative	.72	(452)	.31	(460)	.18	(452)
Very negative	.85	(56)	.58	(54)	.30	(54)
	Partial γ = .58		Partial γ = .23		Partial γ = .09	

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[49] For a good discussion of ideology and aggressive political behavior, see Ted Gurr, *Why Men Rebel* (Princeton: Princeton University Press, 1970), ch. 7.

[50] Miller, "Rejoinder," p. 995.

[51] Miller, "Political Issues and Trust," p. 962.

strongest among persons who place themselves on the Left of the ideological continuum, becoming progressively weaker among those who place themselves in the center and on the Right.

We have seen that there is no relationship between the Aggressive Behavior index and Political Trust, Incumbent Evaluation, or Policy Output Evaluation when Political Support is controlled for, except when support is very negative. But if ideology has a specification effect, then a relationship should emerge among the Left when ideology is introduced as a second control.[52] However, as the data given in Table 8 show, this does not happen. To be sure, when support is in the negative zone, trust and policy correlate modestly with aggressive behavior only among leftists; but even among persons on the Left, at the other levels of support there is still virtually no relationship between aggressive behavior and either trust or policy. The only other correlations of even modest magnitude occur between policy and aggressive behavior under the condition of positive support and rightist ideology and between trust and aggressive behavior under the condition of intermediate support and rightist ideology. Both are something of a puzzle because the former does not conform to the specification hypothesis, while the latter is in a theoretically implausible direction.

When we turn to the Political Support variable, the expected specification effect due to ideology does appear. At all levels of trust and policy, the strong correlation between support and aggressive behavior holds up on the Left. It is considerably depressed among centrists and rightists, reaching moderate magnitudes only when the incumbent affect variables are intermediate or negative. Interestingly, when trust is negative, a phenomenon appears which is reminiscent of what probably was the case in Germany during the 1920s and early 1930s: The correlation between Political Support and the Aggressive Behavior index is weakest among centrists, strong among leftists, and close to the range which we call strong among rightists.

Specification Effect of Community. Ideology is an important specification variable. But an even more powerful specification effect may come from a contextual factor: the type of community in which one lives and works. What are the most relevant distinctions with respect to community? The simple dichotomy, university versus nonuniversity, probably overshadows all others. As Huntington observed not too long ago, in most modernizing countries:

> The city is the center of opposition within the country; the middle class is the focus of opposition within the city; the intelligentsia is the most active oppositional group within the middle class; and the students are the most coherent and effective revolutionaries within the intelligentsia. . . . It is here, in the university, that the most consistent, extreme, intransigent opposition to the government exists.[53]

And as we know from recent history, Huntington's description aptly characterizes the industrialized nations of the world as well.

Why the university? Because it is likely that the university reflects high or facilitative values on three variables which condition the relationship between attitudes conducive to aggressive political behavior and actual aggressive political behavior. These variables are (1) availability, (2) costs,[54] (3) community norms. At the university, students and faculty are not tied to a daily routine. They have far greater freedom of time and movement than do most people who are not in a university context. Students have especially high availability simply because they are young and likely to be unmarried. The costs of aggressive political behavior are, relatively speaking, lower in the context of the university. Authorities are likely to be more lenient—faculty in positions of administrative authority may sympathize with students and other faculty who take part in aggressive political action. And, of course, there is less to lose: the student is not in mid-career, his career lies ahead; the faculty member, if untenured, stands to lose his job, but this may depend largely on whether or not his behavior is viewed as deviant by his immediate departmental colleagues, or, in the case of the Lehrstuhl system, by a few senior professors. This last point leads into what may be the most important of the facilitating conditions reflected in the university-nonuniversity distinction: community norms.

Consider Table 9: Community context has a very powerful inhibitory-facilitative effect on the Aggressive Behavior index. The likelihood of a person scoring at any level other than inactive on the Aggressive Behavior index is practically zero in the urban and agrarian milieus. In the university milieu, there is a strong likelihood that a person will score other than inactive. (The majority of those who score at other than inactive are students in their twenties working on the German equivalent of an American M.A. degree; the minority are persons fully or partly employed at the university in some capacity, ranging from a graduate assistant working on the Ph.D. to full professor. Almost everyone scoring at other than inactive is under 40 years of age.) Although persons from the university milieu are unlikely to score at the Political Violence level on the Aggressive Behavior index (though almost all of those who *do* score at this level are from the university), civil disobedience is certainly not deviant behavior in this context: 57.5 percent of those in the university sample score at the civil disobedience levels.

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[52] In the SRC data analyzed by Miller, there was no zero-order relationship between trust and the approval of protest items, but when ideology was controlled for, a modest relationship occurred between these variables among those with a leftist ideological orientation. See Miller, "Rejoinder," p. 996.

[53] Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968), pp. 290–91.

[54] For an especially good discussion of costs, see Anthony Oberschall, *Social Conflict and Social Movements* (Englewood Cliffs, N.J.: Prentice-Hall, 1973), ch. 5.

Table 8. Correlations (γ) Between Aggressive Behavior Index and Political Affect Variables, Controlling for Ideological Commitment and Other Political Affect Variables

Controlling for:		Correlation Between Aggressive Behavior Index and:			
		Political Trust		Policy Output Evaluation	
Ideological Commitment	Political Support	γ	(N)	γ	(N)
Left[a]	Positive[c]	.10	(206)	.08	(203)
	Intermediate	.01	(309)	.14	(314)
	Negative[d]	.34	(412)	.43	(417)
Center	Positive[c]	.02	(255)	.17	(248)
	Intermediate	.08	(344)	–.09	(336)
	Negative[d]	–.03	(90)	.15	(87)
Right[b]	Positive[c]	–.12	(142)	.38	(139)
	Intermediate	–.28	(208)	.10	(204)
	Negative[d]	–	(64)	–	(66)

		Political Support	
Ideological Commitment	Political Trust	γ	(N)
Left[a]	Positive[c]	.63	(227)
	Intermediate	.67	(381)
	Negative[d]	.59	(319)
Center	Positive[c]	–.01	(200)
	Intermediate	.26	(313)
	Negative[d]	.35	(176)
Right[b]	Positive[c]	–	(71)
	Intermediate	–.22	(185)
	Negative[d]	.47	(158)

		Political Support	
Ideological Commitment	Policy Output Evaluation	γ	(N)
Left[a]	Positive[c]	.51	(216)
	Intermediate	.64	(468)
	Negative[d]	.65	(250)
Center	Positive[c]	.23	(189)
	Intermediate	.26	(376)
	Negative[d]	.31	(106)
Right[b]	Positive[c]	–	(85)
	Intermediate	–.03	(217)
	Negative[d]	–	(107)

[a] Includes "Extreme Left."
[b] Includes "Extreme Right."
[c] Includes "Very Positive."
[d] Includes "Very Negative."

Or take Ideological Commitment and Political Support, the two most powerful attitudinal predictors of aggressive behavior identified in the analysis to this point: Nearly two-thirds of the university sample is leftist; by contrast, only about one-third of those in the urban milieu have a leftist ideological orientation and only about one-quarter of those in the agrarian milieu designate themselves as leftist. Especially striking differences on the Political Support variable are registered when one controls for community type: Nearly one-sixth of those in the university milieu express very negative support and almost one-third score at the "negative" zone; by contrast, outside of the university milieu, very few register in the "very negative" zone on Political Support, while only 9.1 percent of those in the urban milieu and only 14.8 percent of those in the agrarian milieu register in the "negative" zone.

In sum, availability (by definition), costs (by inference), and community norms (by the evidence in Table 9) are facilitative in the university milieu, inhibitory in the nonuniversity milieu. Therefore, we hypothesize that the relationships among ideology, the political affect variables, and the Aggressive Behavior index will either be enhanced or else little changed among persons in the university milieu and depressed among persons in the nonuniversity milieu.

The first proposition in the theory about behavioral consequences of the incumbent-system distinction is a statement about the

Table 9. Distributions on Political Affect and Behavior Variables by Community Type

	Community Type					
	Rural		Urban		University	
Affect and Behavior Variables	%	(N)	%	(N)	%	(N)
Political Support						
Very negative	2.4	(11)	1.8	(15)	15.6	(167)
Negative	14.8	(69)	9.1	(74)	30.1	(322)
Intermediate	46.9	(219)	44.1	(360)	40.3	(432)
Positive	28.9	(135)	38.6	(315)	12.7	(136)
Very positive	7.1	(33)	6.5	(53)	1.3	(14)
TOTAL	100.0	(467)	100.0	(817)	100.0	(1071)
Political Trust						
Very negative	5.1	(28)	2.5	(23)	10.0	(107)
Negative	24.0	(132)	20.0	(184)	29.0	(310)
Intermediate	40.2	(221)	43.5	(401)	46.5	(498)
Positive	24.0	(132)	27.4	(252)	14.0	(150)
Very positive	6.7	(37)	6.6	(61)	0.5	(5)
TOTAL	100.0	(550)	100.0	(921)	100.0	(1070)
Policy Output Evaluation						
Very negative	0.8	(4)	1.1	(10)	4.4	(47)
Negative	18.2	(86)	14.6	(131)	24.9	(269)
Intermediate	50.3	(238)	45.0	(405)	56.0	(604)
Positive	26.2	(124)	34.3	(309)	13.5	(146)
Very positive	4.4	(21)	5.0	(45)	1.2	(13)
TOTAL	100.0	(473)	100.0	(900)	100.0	(1079)
Ideological Commitment						
Extreme right	15.2	(75)	4.9	(38)	1.2	(13)
Right	23.4	(116)	17.5	(136)	10.2	(107)
Center	33.7	(167)	42.9	(334)	27.8	(290)
Left	21.6	(107)	29.0	(226)	45.7	(477)
Extreme left	6.1	(30)	5.7	(44)	15.0	(157)
TOTAL	100.0	(495)	100.0	(778)	100.0	(1044)
Aggressive Behavior Index						
Violence	–	(0)	–	(0)	2.6	(27)
High C.D.	1.3	(7)	2.0	(18)	26.6	(277)
Low C.D.	4.6	(25)	5.3	(49)	30.9	(322)
Inactive	94.1	(507)	92.7	(853)	39.9	(415)
TOTAL	100.0	(539)	100.0	(920)	100.0	(1041)

relationship between incumbent affect and aggressive political behavior under the condition of positive system affect. Given the form of the relationship expected to obtain generally between political affect and aggressive behavior, the Gamma correlation will be used to test the propositions. From Tables 10 and 11 we see that the Gamma correlations are practically zero, as predicted from P_1, except for the relationship between trust and aggressive behavior in the nonuniversity milieu—but here, the correlation, while barely moderate, is in a theoretically implausible direction.

The second proposition, which predicted a slight, positive correlation between incumbent affect and aggressive behavior under the condition of negative system affect, is not confirmed due to the specification effect of community context. In the nonuniversity milieu, the correlation is either lower than predicted (practically zero) or else its sign (in the case of trust) is in the wrong direction. By contrast, in the university milieu the correlation is moderate—a greater magnitude than predicted.

Tables 12 and 13 show data relevant to the test of the third and fourth propositions. The third proposition predicts a moderate correlation between system affect and aggressive behavior, under the condition of positive incumbent affect. This is disconfirmed in the nonuniversity milieu, where the correlation is only slight. It is confirmed in the university milieu when Policy Output Evaluation is taken as the indicator of incumbent affect. But it is disconfirmed in the university milieu with Political Trust as the indicator of incumbent affect because, in this case, a strong correlation is observed between aggressive behavior and support when trust is positive.

The fourth proposition predicts a strong correlation between system affect and aggressive behavior, under the condition of negative incumbent affect. When Policy Output Evaluation is taken as the indicator of incumbent affect, the fourth proposition is strongly confirmed in the university milieu, and even in the nonuniversity milieu the Gamma correlation of .49 verges on the lower boundary of the magnitude predicted.

When Political Support is controlled for, the measures especially sensitive to affect for an incumbent administration show, at best, only a modest correlation with the Aggressive Behavior index. Generally, aggressive behavior is unrelated to incumbent affect after Political Support is taken into account. But Policy Output Evaluation does appear to have a specification effect on the relationship between Political Support and the Aggressive Behavior index, inhibiting the relationship when Policy Output Evaluation is positive, facilitating it when Policy Output Evaluation is negative, although this occurs more markedly in the nonuniversity than in the university milieu.

What happens when policy and trust are *both* taken into account? Will there be a specification effect due to incumbent affect? And will it hold up independently of ideology? These questions are addressed by the data shown in Table 14. Policy Output Evaluation and Political Trust are combined into three conditions: High, defined by positive scores on both policy and trust; Medium, defined by a positive score on one incumbent affect variable, a nonpositive score on the other; Low, defined by nonpositive scores on both policy and trust.

When evaluation of incumbents is consistently positive or high—they are trusted and their policy performance is satisfactory—the range on Political Support is confined almost completely to "very positive" through "intermediate." Even so, a specification effect due to ideology is apparent. The non-Left is virtually certain to score at the inactive level on the Aggressive Behavior index, regardless of whether their support is at a positive level or at an intermediate level. Among the Left, however, the rate of civil disobedience is higher when support is intermediate than when it is positive, and the relationship is stronger in the university than in the nonuniversity milieu. Even though the number of cases under this condition in the university milieu is small, the relationship is nevertheless sufficiently sharp to warrant confidence in its reliability.

At the low condition of incumbent affect, where incumbents are not trusted and their policy performance is not satisfactory, the specification effect of ideology is quite pronounced. On the Left, as Political Support decreases from "positive" to "very negative," the percentage of persons scoring Inactive on the Aggressive Behavior index consistently declines. The magnitude of the decline is somewhat greater in the university milieu (62.4 percentage points), and this difference is reflected in the size of the Gamma correlations (.55 and .49, respectively); the correlation, however, is strong or very close to strong in both contexts. Community context does not affect the strength of the correlation. But it does have an effect on the rates of inactivity at given levels of support. In the nonuniversity milieu, the inactivity rate on the Aggressive Behavior index begins at 97.6, when Political Support is "positive," then drops to 50, when

Table 10. Aggressive Behavior Index as a Function of Political Trust, Controlling for Political Support and Community Type

			Political Trust										
			Very Positive		Positive		Intermediate		Negative		Very Negative		
Community Type	Political Support	Aggressive Behavior Index	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)	
Rural and Urban	Positive[a]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	$\gamma = -.29$
		High C.D.	1.6	(1)	0.5	(1)	1.6	(3)	0	(0)	–	(0)	
		Low C.D.	7.8	(5)	5.3	(11)	2.7	(5)	2.0	(1)	–	(0)	(N = 511)
		Inactive	90.6	(58)	94.2	(195)	95.7	(180)	98.0	(48)	–	(3)	
	Intermediate	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	$\gamma = -.17$
		High C.D.	0	(0)	3.2	(3)	1.4	(4)	1.4	(2)	–	(0)	
		Low C.D.	16.7	(3)	6.4	(6)	6.4	(18)	5.8	(8)	–	(0)	(N = 549)
		Inactive	83.3	(15)	90.4	(85)	92.2	(260)	92.8	(129)	100.0	(16)	
	Negative[b]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	$\gamma = -.17$
		High C.D.	–	(0)	11.1	(1)	7.5	(3)	3.5	(3)	8.0	(2)	
		Low C.D.	–	(0)	11.1	(1)	7.5	(3)	5.8	(5)	4.0	(1)	(N = 160)
		Inactive	–	(0)	77.8	(7)	85.0	(34)	90.7	(78)	88.0	(22)	
University	Positive[a]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	$\gamma = .10$
		High C.D.	–	(0)	5.3	(3)	5.6	(4)	6.7	(1)	–	(0)	
		Low C.D.	–	(1)	19.3	(11)	27.8	(20)	20.0	(3)	–	(0)	(N = 149)
		Inactive	–	(3)	75.4	(43)	66.7	(48)	73.3	(11)	–	(1)	
	Intermediate	Violence	–	(0)	1.4	(1)	0.9	(2)	2.4	(2)	–	(0)	$\gamma = -.04$
		High C.D.	–	(1)	14.9	(11)	11.8	(27)	16.5	(14)	12.5	(1)	
		Low C.D.	–	(0)	32.4	(24)	37.1	(85)	29.4	(25)	–	(0)	(N = 397)
		Inactive	–	(0)	51.4	(38)	50.2	(115)	51.8	(44)	87.5	(7)	
	Negative[b]	Violence	–	(0)	–	(0)	1.9	(3)	2.7	(5)	13.7	(13)	$\gamma = .36$
		High C.D.	–	(0)	42.9	(6)	35.4	(56)	45.1	(83)	61.1	(58)	
		Low C.D.	–	(0)	35.7	(5)	36.1	(57)	31.5	(58)	20.0	(19)	(N = 451)
		Inactive	–	(0)	21.4	(3)	26.6	(42)	20.7	(38)	5.3	(5)	

[a] Includes "Very Positive."

[b] Includes "Very Negative."

Table 11. Aggressive Behavior Index as a Function of Policy Output Evaluation, Controlling for Political Support and Community Type

Community Type	Political Support	Aggressive Behavior Index	Policy Output Evaluation: Very Positive %	(N)	Positive %	(N)	Intermediate %	(N)	Negative %	(N)	Very Negative %	(N)	
Rural and Urban	Positive[a]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	γ = .09
		High C.D.	–	(0)	0.9	(2)	1.4	(3)	–	(0)	–	(0)	
		Low C.D.	–	(0)	4.6	(10)	5.1	(11)	4.7	(2)	–	(0)	(N = 495)
		Inactive	100.0	(19)	94.5	(206)	93.5	(200)	95.3	(41)	–	(2)	
	Intermediate	High C.D.	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	γ = –.16
		High C.D.	–	(0)	2.3	(3)	2.1	(6)	–	(0)	–	(0)	
		Low C.D.	–	(0)	6.9	(9)	8.5	(24)	2.9	(3)	–	(0)	(N = 538)
		Inactive	100.0	(16)	90.8	(118)	89.4	(254)	97.1	(100)	100.0	(5)	
	Negative[b]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	γ = –.04
		High C.D.	–	(0)	6.9	(2)	2.9	(2)	5.8	(3)	–	(0)	
		Low C.D.	40.0	(2)	3.4	(1)	4.3	(3)	7.7	(4)	–	(0)	(N = 160)
		Inactive	60.0	(3)	89.7	(26)	92.9	(65)	86.5	(45)	–	(4)	
University	Positive[a]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	γ = .02
		High C.D.	–	(0)	4.9	(2)	6.8	(6)	–	(0)	–	(0)	
		Low C.D.	–	(1)	22.0	(9)	23.9	(21)	25.0	(4)	–	(0)	(N = 149)
		Inactive	–	(3)	73.2	(30)	69.3	(61)	75.0	(12)	–	(0)	
	Intermediate	Violence	–	(0)	1.5	(1)	1.1	(3)	1.5	(1)	–	(0)	γ = .04
		High C.D.	–	(0)	13.4	(9)	11.7	(31)	22.7	(15)	–	(0)	
		Low C.D.	–	(0)	32.8	(22)	38.3	(101)	21.2	(14)	–	(1)	(N = 401)
		Inactive	–	(2)	52.2	(35)	48.9	(129)	54.5	(36)	–	(1)	
	Negative[b]	Violence	–	(0)	–	(0)	1.9	(4)	6.4	(11)	16.3	(7)	γ = .41
		High C.D.	–	(0)	35.7	(10)	38.5	(80)	48.8	(84)	69.8	(30)	
		Low C.D.	–	(0)	28.6	(8)	36.5	(76)	29.7	(51)	14.0	(6)	(N = 454)
		Inactive	–	(3)	35.7	(10)	23.1	(48)	15.1	(26)	–	(0)	

[a] Includes "Very Positive."
[b] Includes "Very Negative."

Table 12. Aggressive Behavior Index as a Function of Political Support Controlling for Political Trust and Community Type

Community Type	Political Trust	Aggressive Behavior Index	Political Support										
			Very Positive		Positive		Intermediate		Negative		Very Negative		
			%	(N)	%	(N)	%	(N)	%	(N)	%	(N)	
Rural and Urban	Positive[a]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	$\gamma = .13$
		High C.D.	3.1	(2)	–	(0)	2.7	(3)	11.1	(1)	–	(0)	
		Low C.D.	7.8	(5)	5.3	(11)	8.0	(9)	11.1	(1)	–	(0)	(N = 392)
		Inactive	89.1	(57)	94.7	(196)	89.3	(100)	77.8	(7)	–	(0)	
	Intermediate	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	$\gamma = .37$
		High C.D.	–	(0)	1.7	(3)	1.4	(4)	8.3	(3)	–	(0)	
		Low C.D.	–	(0)	2.9	(5)	6.4	(18)	2.8	(1)	–	(2)	(N = 510)
		Inactive	100.0	(16)	95.3	(164)	92.2	(260)	88.9	(32)	–	(2)	
	Negative[b]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	$\gamma = .37$
		High C.D.	–	(0)	–	(0)	1.3	(2)	2.2	(2)	15.0	(3)	
		Low C.D.	–	(1)	–	(0)	5.2	(8)	5.5	(5)	5.0	(1)	(N = 318)
		Inactive	–	(3)	100.0	(48)	93.5	(145)	92.3	(84)	80.0	(16)	
University	Positive[a]	Violence	–	(0)	–	(0)	1.3	(1)	–	(0)	–	(0)	$\gamma = .54$
		High C.D.	–	(0)	5.7	(3)	16.0	(12)	41.7	(5)	–	(1)	
		Low C.D.	12.5	(1)	20.8	(11)	32.0	(24)	33.3	(4)	–	(1)	(N = 150)
		Inactive	87.5	(7)	73.6	(39)	50.7	(38)	25.0	(3)	–	(0)	
	Intermediate	Violence	–	(0)	–	(0)	0.9	(2)	2.1	(3)	–	(0)	$\gamma = .47$
		High C.D.	–	(0)	6.1	(4)	11.8	(27)	34.0	(49)	50.0	(7)	
		Low C.D.	16.7	(1)	28.8	(19)	37.1	(85)	37.5	(54)	21.4	(3)	(N = 459)
		Inactive	83.3	(5)	65.2	(43)	50.2	(115)	26.4	(38)	28.6	(4)	
	Negative[b]	Violence	–	(0)	–	(0)	2.2	(2)	2.3	(3)	10.3	(15)	$\gamma = .64$
		High C.D.	–	(0)	6.3	(1)	16.1	(15)	37.6	(50)	62.3	(91)	
		Low C.D.	–	(0)	18.8	(3)	26.9	(25)	34.6	(46)	21.2	(31)	(N = 388)
		Inactive	–	(0)	75.0	(12)	54.8	(51)	25.6	(34)	6.2	(9)	

[a] Includes "Very Positive."
[b] Includes "Very Negative."

Table 13. Aggressive Behavior Index as a Function of Political Support Controlling for Policy Output Evaluation and Community Type

Community Type	Policy Output Evaluation	Aggressive Behavior Index	Political Support										
			Very Positive		Positive		Intermediate		Negative		Very Negative		
			%	(N)	%	(N)	%	(N)	%	(N)	%	(N)	
Rural and Urban	Positive[a]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	
		High C.D.	3.5	(2)	–	(0)	2.1	(3)	6.3	(2)	–	(0)	γ = .15
		Low C.D.	8.8	(5)	2.8	(5)	6.2	(9)	9.4	(3)	–	(0)	
		Inactive	87.7	(50)	97.2	(175)	91.8	(134)	84.4	(27)	–	(2)	(N = 417)
	Intermediate	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	
		High C.D.	–	(0)	1.6	(3)	2.1	(6)	1.6	(1)	11.1	(1)	γ = .13
		Low C.D.	4.5	(1)	5.2	(10)	8.5	(24)	4.9	(3)	0	(0)	
		Inactive	95.5	(21)	93.2	(179)	89.4	(254)	93.4	(57)	88.9	(8)	(N = 568)
	Negative[b]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	
		High C.D.	–	(0)	–	(0)	–	(0)	2.3	(1)	15.4	(2)	γ = .49
		Low C.D.	–	(0)	4.5	(2)	2.8	(3)	2.3	(1)	23.1	(3)	
		Inactive	–	(1)	95.5	(42)	97.2	(105)	95.3	(41)	61.5	(8)	(N = 209)
University	Positive[a]	Violence	–	(0)	–	(0)	1.4	(1)	–	(0)	–	(0)	
		High C.D.	–	(0)	5.0	(2)	13.0	(9)	26.9	(7)	60.0	(3)	γ = .40
		Low C.D.	20.0	(1)	22.5	(9)	31.9	(22)	26.9	(7)	20.0	(1)	
		Inactive	80.0	(4)	72.5	(29)	53.6	(37)	46.2	(12)	20.0	(1)	(N = 145)
	Intermediate	Violence	–	(0)	–	(0)	1.1	(3)	1.2	(2)	4.8	(2)	
		High C.D.	–	(0)	7.6	(6)	11.7	(31)	34.3	(57)	54.8	(23)	γ = .52
		Low C.D.	11.1	(1)	25.3	(20)	38.3	(101)	39.8	(66)	23.8	(10)	
		Inactive	88.9	(8)	67.1	(53)	48.9	(129)	24.7	(41)	16.7	(7)	(N = 560)
	Negative[b]	Violence	–	(0)	–	(0)	1.5	(1)	5.0	(5)	11.3	(13)	
		High C.D.	–	(0)	6.3	(1)	22.1	(15)	40.0	(40)	64.3	(74)	γ = .65
		Low C.D.	–	(0)	18.8	(3)	22.1	(15)	33.0	(33)	20.9	(24)	
		Inactive	–	(0)	75.0	(12)	54.4	(37)	22.0	(22)	3.5	(4)	(N = 299)

[a] Includes "Very Positive."
[b] Includes "Very Negative."

Table 14. Aggressive Behavior Index as a Function of Political Support, Controlling for Policy Output Evaluation, Political Trust, Ideological Commitment, and Community Type

Controlling for:				Political Support										
				Very Positive		Positive		Intermediate		Negative		Very Negative		
Community Type	Ideological Commitment	Policy and Trust	Aggressive Behavior Index	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)	
Rural and Urban	Left[a]	High[c]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	γ = .17
			High C.D.	4.0	(1)	–	(0)	–	(0)	–	(1)	–	(0)	
			Low C.D.	12.0	(3)	4.7	(2)	22.2	(4)	–	(0)	–	(0)	
			Inactive	84.0	(21)	95.3	(41)	77.8	(14)	–	(1)	–	(0)	(N = 88)
		Medium[d]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	γ = .36
			High C.D.	–	(0)	–	(0)	7.0	(3)	6.3	(1)	–	(0)	
			Low C.D.	–	(0)	7.1	(3)	2.3	(1)	12.5	(2)	–	(0)	
			Inactive	100.0	(7)	92.9	(39)	90.7	(39)	81.3	(13)	–	(1)	(N = 109)
		Low[e]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	γ = .49
			High C.D.	–	(0)	–	(0)	1.3	(1)	9.5	(2)	20.0	(2)	
			Low C.D.	–	(1)	2.4	(1)	13.3	(10)	4.8	(1)	30.0	(3)	
			Inactive	–	(2)	97.6	(40)	85.3	(64)	85.7	(18)	50.0	(5)	(N = 150)
	Center and Right[b]	High[c]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	––
			High C.D.	5.6	(1)	–	(0)	–	(0)	–	(0)	–	(0)	
			Low C.D.	11.1	(2)	5.9	(3)	4.3	(1)	–	(0)	–	(0)	
			Inactive	83.3	(15)	94.1	(48)	95.7	(22)	–	(0)	–	(0)	(N = 92)
		Medium[d]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	γ = .56
			High C.D.	–	(0)	–	(0)	3.5	(3)	–	(0)	–	(0)	
			Low C.D.	–	(0)	3.0	(3)	7.0	(6)	13.3	(2)	–	(0)	
			Inactive	100.0	(15)	97.0	(98)	89.5	(77)	86.7	(13)	–	(1)	(N = 218)
		Low[e]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	γ = –.39
			High C.D.	–	(0)	3.5	(3)	0.9	(2)	–	(0)	–	(0)	
			Low C.D.	–	(0)	4.7	(4)	5.5	(12)	–	(0)	–	(0)	
			Inactive	100.0	(7)	91.8	(78)	93.6	(205)	100.0	(55)	100.0	(10)	(N = 376)

(continued on next page)

Table 14. *(continued)*

Controlling for:				Political Support										
				Very Positive		Positive		Intermediate		Negative		Very Negative		
Community Type	Ideological Commitment	Policy and Trust	Aggressive Behavior Index	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)	
University	Left[a]	High[c]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	γ = .79
			High C.D.	–	(0)	–	(0)	40.0	(4)	–	(1)	–	(0)	
			Low C.D.	–	(0)	12.5	(1)	40.0	(4)	–	(0)	–	(0)	(N = 22)
			Inactive	–	(2)	87.5	(7)	20.0	(2)	–	(1)	–	(0)	
		Medium[d]	Violence	–	(0)	–	(0)	3.6	(2)	–	(0)	–	(0)	γ = .29
			High C.D.	–	(0)	18.2	(4)	14.3	(8)	25.0	(5)	57.1	(4)	
			Low C.D.	–	(0)	22.7	(5)	46.4	(26)	40.0	(8)	28.6	(2)	(N = 105)
			Inactive	–	(0)	59.1	(13)	35.7	(20)	35.0	(7)	14.3	(1)	
		Low[e]	Violence	–	(0)	–	(0)	2.9	(3)	3.2	(6)	10.9	(15)	γ = .55
			High C.D.	–	(0)	11.1	(1)	23.8	(25)	42.9	(81)	65.2	(90)	
			Low C.D.	–	(0)	22.2	(2)	42.9	(45)	39.7	(75)	19.6	(27)	(N = 441)
			Inactive	–	(0)	66.7	(6)	30.5	(32)	14.3	(27)	4.3	(6)	
	Center and Right[b]	High[c]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	—
			High C.D.	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	
			Low C.D.	–	(1)	9.1	(1)	–	(0)	–	(0)	–	(0)	(N = 18)
			Inactive	–	(0)	90.9	(10)	100.0	(6)	–	(0)	–	(0)	
		Medium[d]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	γ = .16
			High C.D.	–	(0)	3.4	(1)	10.6	(5)	28.6	(2)	–	(0)	
			Low C.D.	–	(0)	34.5	(10)	21.3	(10)	14.3	(1)	–	(0)	(N = 88)
			Inactive	100.0	(5)	62.1	(18)	68.1	(32)	57.1	(4)	–	(0)	
		Low[e]	Violence	–	(0)	–	(0)	–	(0)	–	(0)	–	(0)	γ = .18
			High C.D.	–	(0)	3.9	(2)	4.1	(6)	14.3	(8)	–	(0)	
			Low C.D.	20.0	(1)	25.5	(13)	29.7	(44)	28.6	(16)	33.3	(2)	(N = 266)
			Inactive	80.0	(4)	70.6	(36)	66.2	(98)	57.1	(32)	66.7	(4)	

[a] Includes "Extreme Left."
[b] Includes "Extreme Right."
[c] Includes "Very Positive" and "Positive" on both variables.
[d] Includes "Very Positive" and "Positive" on one variable, "Intermediate," "Negative," and "Very Negative" on the other.
[e] Includes "Intermediate," "Negative," and "Very Negative" on both variables.

Political Support is at its most negative level. By contrast, the inactivity rate among those in the university milieu *begins* at 66.7 percent for those with positive support, then drops all the way to under 5 percent, at the most negative position on the Political Support variable.

Turning to the non-Left we see that the direction of the relationship between Political Support and the Aggressive Behavior Index actually is *reversed* among persons in the nonuniversity milieu due to the fact that everyone on the non-Left who is disaffected from the system of government also is inactive on the Aggressive Behavior index. This phenomenon is quite unexpected from a theoretical standpoint and calls for a separate, "deviant case" analysis. The non-Left in the university milieu is not so deviant. Here the relationship between Political Support and the Aggressive Behavior index, while by no means as strong as that which holds among leftists, goes in the predicted direction at least, except for a small, deviant upturn when Political Support is at the most negative level.

Summary. We can summarize the relationship between the Political affect variables and the Aggressive Behavior index, taking into account ideology and community context, as follows:

1. There is a specification effect due to incumbent affect, as predicted by the theory of the behavioral consequences of the incumbent-system distinction, but this effect is relatively minor compared to that which results from ideology.

   a) Among the Left, regardless of the level of policy and trust, moderate-to-strong correlations are observed except in the instance of High policy and trust levels in the nonuniversity milieu.

   b) Among the non-Left, regardless of the level of policy and trust, either (1) almost all persons are in the Inactive zone on the Aggressive Behavior index (there is too little variation on aggressive behavior to even compute a correlation) or (2) the observed correlation between Political Support and the Aggressive Behavior index is very weak or else has a theoretically implausible sign. The one exception is the instance of Medium policy and trust levels in the nonuniversity milieu, where the Gamma correlation between support and aggressive behavior reaches a strong magnitude.

2. Although ideology appears to have the major specification effect on the relationship between Political Support and the Aggressive Behavior index—inhibiting it (or producing a change in sign) among centrists and rightists, facilitating it among leftists—a slightly stronger strength of relationship is observed among leftists in a facilitative community context than in an inhibitory community context. More importantly, community context affects the likelihood of participation in aggressive action, given varying degrees of political support among those with a leftist ideological commitment.

   a) When support is in the positive range, leftists in a nonuniversity context are practically certain to score in the Inactivity zone on the Aggressive Behavior index. This is much less true of leftists who reside in a university milieu.

   b) When support is in the negative range, leftists in a university context are extremely likely to score at other than Inactive on the Aggressive Behavior index. This is much less true of leftists who reside in a nonuniversity milieu.

3. These generalizations apply to the Aggressive Behavior index as ranging from Inactivity (no participation in aggressive behavior and negative intention toward its performance) to High Civil Disobedience (participation in nonviolent aggressive action and positive intention toward political violence). Political violence occurs only under the following set of circumstances: (1) university context; (2) leftist ideological commitment; (3) low policy and trust; and (4) negative or very negative Political Support. But even under these conditions civil disobedience is a far more likely outcome than violent action.

One puzzle remains to be solved. Granted that aggressive political behavior is more acceptable to the Left than to the Center or Right, we have nonetheless seen that a facilitative community context counteracts the inhibitory effect of ideology to some degree: In the university milieu, under the low policy and trust condition, 26 of the 53 persons who score in the negative range on the Political Support variable also score at the civil disobedience levels on the Aggressive Behavior index. Yet, given the exact same conditions, only with community context inhibitory, all 65 of those who score in the negative range on Political Support also score at the inactive level on the Aggressive Behavior index. Is the Political Support variable for some reason irrelevant under just this latter set of conditions? Table 15 provides an answer.

Centrists do not have much of an alternative if they are in an inhibitory context and are negative toward the political system. But the Right does, at least in Germany. This is the NPD, a party of the extreme Right which for a time displayed clear neo-Nazi coloration. In the nonuniversity milieu, 20.6 percent of those on the Right—and even 16.7 percent of those in the Center—who register in the "negative" zone on Political Support would like to see the NPD either represented or the majority party in parliament. Among those few on the Right who score at the most negative level of support for the political system, one-half would like to see the NPD represented or the majority in parliament. By contrast, the rate of support for the NPD among centrists and rightists who are positive toward the political system is virtually nil. Obviously there is a relationship, and among rightists it looks almost identical to the relationship between support and aggressive political behavior among leftists in an inhibitory context. The Right in an inhibitory context simply manifests its opposition in a different way. Finally, it is also worth noting that those on the Left who score at inactive on the Aggressive Behavior index are considerably more likely to express support for the German Communist Party (DNP) if their support for the political system is negative, than if their support is intermediate or positive.

Conclusion

To return to the questions which began this paper, our data indicate that the analytical distinction between affect for an incumbent administration and affect for the system of government is important empirically because it affords a more precise explanation of aggressive political behavior than would be possible if political affect were treated as a single entity. Affect for the system of government is a more powerful explanatory variable than affect for an incumbent administration. It is generally related to aggressive political behavior regardless of whether incumbent affect is positive or negative (though the relationship is depressed or heightened depending especially on whether ideological commitment is non-Left or Left). Incumbent affect is unrelated to aggressive political behavior when system affect is controlled for—except for modest correlations which appear under one or two special conditions. If we are to make causal inferences, it would appear that bivariate correlations between measures of aggressive political behavior and variables sensitive to incumbent affect actually represent either indirect or spurious correlation. This is due to the fact that incumbent affect also is correlated with what appears to be the more powerful and direct antecedent of aggressive political behavior, namely, affect for the system of government—Political Support, as we label it.

All indications point to the conclusion that trust in government is, as Stokes intimated some time back and as Citrin recently argued, more sensitive to incumbent than to system affect and should be classified in the same category as such variables as evaluation of public policy outputs and general approval-disapproval of the performance of an incumbent administration. Incumbent affect, while probably not of much consequence in any direct causal sense, does appear to condition modestly the relationship between system affect and aggressive political behavior such that when the incumbents are trusted and their policies are regarded in a positive light, the relationship is weaker than it is when the incumbents are not trusted and their policies are not regarded positively. The relationship between system affect and aggressive political behavior is most strongly specified by ideology: the relationship generally holds up well among persons with a leftist ideological orientation; but it is sometimes erratic and considerably weaker among persons with a centrist or rightist ideological orientation. However, among rightists, when one substitutes support of an extreme Right oppositionist party for participation in aggressive political behavior, then in place of a puzzling negative relationship between degree of antisystem sentiment and aggressive action, one finds a not inconsequential positive relationship.

The data indicate that the theory of the behavioral consequences of the incumbent-system distinction suggested by the work of Lipset and Easton must be qualified according to whether ideology and community context are inhibitory or facilitative. The theory should be reformulated to take into account the effect of ideological commitment and community context, for each has a separate role to play in the explanation of aggressive political behavior, and the nature of this role does not appear to be linear additive.

The Federal Republic of Germany today is a stable political system. In part this is because it has managed, in a relatively short space of time, to create legitimacy and also to perform effectively, at least in the eyes of most of its population. This is not so much the view seen by many of those at its universities, but those with an opposing view have not yet managed to convince the populace at large. Out of 1033 persons in a nonuniversity milieu whom we could score on all of our variables, only ten fell in the most volatile condition: leftist, low on policy evaluation and political trust, very negative toward the system of government. But half of these ten scored at the civil disobedience levels on the Aggressive Behavior index; of the other five, three support the German Communist Party. These are large percentages of a small base. But very negative system support is an extremely deviant sentiment in the rural and urban communities from which our samples were drawn. In the rural milieu, only 2.4 percent registered at "very negative" on the Political Support variable. In the urban milieu,

Table 15. Relationship Between Political Support and Attitude Toward Oppositionist Political Parties Controlling for Ideological Commitment

Controlling for:	Party	Political Support										
Ideological Commitment	NPD Represented or Majority in Parliament	Very Positive %	Very Positive (N)	Positive %	Positive (N)	Intermediate %	Intermediate (N)	Negative %	Negative (N)	Very Negative %	Very Negative (N)	
Left	Yes	–	(0)	2.8	(1)	5.6	(3)	6.2	(1)	–	(0)	(N = 113)
	No	–	(2)	97.2	(35)	94.4	(51)	93.8	(15)	100.0	(5)	
Center	Yes	–	(0)	2.4	(1)	6.9	(6)	16.7	(3)	–	(0)	(N = 151)
	No	–	(3)	97.6	(40)	93.1	(81)	83.3	(15)	–	(2)	
Right	Yes	–	(0)	2.9	(1)	8.4	(8)	20.6	(7)	50.0	(4)	(N = 175)
	No	–	(3)	97.1	(34)	91.6	(87)	79.4	(27)	50.0	(4)	
Ideological Commitment	DKP Represented or Majority in Parliament	Very Positive %	Very Positive (N)	Positive %	Positive (N)	Intermediate %	Intermediate (N)	Negative %	Negative (N)	Very Negative %	Very Negative (N)	
Left	Yes	–	(0)	10.8	(4)	8.9	(5)	29.4	(5)	60.0	(3)	(N = 117)
	No	–	(2)	89.2	(33)	91.1	(51)	70.6	(12)	40.0	(2)	
Center	Yes	–	(0)	5.0	(2)	7.6	(7)	10.5	(2)	–	(0)	(N = 157)
	No	–	(4)	95.0	(38)	92.4	(85)	89.5	(17)	–	(2)	
Right	Yes	–	(0)	2.9	(1)	2.1	(2)	6.1	(2)	–	(0)	(N = 176)
	No	–	(4)	97.1	(34)	97.9	(94)	93.9	(31)	100.0	(8)	

Table A1. Relationship Between Composite Aggressive Behavior Index and Index of Actual Participation

Actual Participation	Aggressive Behavior Index 0	1	2	3	4	5	6	7	8	9	10
High (3–5)	5	0	1	0	1	1	3	5	16	6	5
Medium (1–2)	24	9	50	41	67	43	35	13	0	0	0
Low (0)	1431	229	150	146	57	42	0	0	0	0	0
TOTAL	1460	238	201	187	125	86	38	18	16	6	5

Table A2. Relationship Between Composite Aggressive Behavior Index and Index of Intention to Participate

Intention to Participate	Aggressive Behavior Index 0	1	2	3	4	5	6	7	8	9	10
High (5–10)	0	0	0	15	49	84	35	17	13	6	5
Medium (1–4)	0	249	205	176	86	26	4	1	3	0	0
Low (0)	1526	0	0	0	0	0	0	0	0	0	0
TOTAL	1526	249	205	191	135	110	39	18	16	6	5

the "very negative" rate was 1.8 percent. Should "very negative" sentiment for the system of government become less deviant, the incidence of aggressive political behavior would likely increase. Should the incidence of aggressive political behavior increase, aggressive action itself would become less of a deviant form of political behavior. And if negative system support and aggressive political behavior both became less deviant in the nonuniversity milieu, this milieu would begin in all likelihood to lose much of its inhibitory effect on the relationship between system support and aggressive behavior.

Appendix. Analysis of the Components of Aggressive Political Behavior

As mentioned in the text, the aggressive behavior measure is built from responses about behavioral intention and actual participation in the following five behaviors: (1) refusal to pay rents or taxes; (2) participation in a wildcat strike; (3) takeover of factories, offices, and other buildings; (4) participation in fights (battle with police, battle with other demonstrators); (5) participation in a group which wants to dislodge the government by violent means. Refusal to pay rents or taxes and participation in a wildcat strike are examples of nonviolent civil disobedience. Participation in fights and involvement with a group which wants to overthrow the government by violent means are examples of political violence. The item dealing with seizure of buildings represents a behavior which could fall either way. If we include this item within the domain of violence, then participation in three or more behaviors would mean that a person must have participated in at least one violent action. Table A1 gives the relationship between the composite Aggressive Behavior index and an index of actual participation. The High category of participation is defined by involvement in three or more actions, of which one or more entails violence. The Medium category is defined by involvement in one or two actions. Most of these entail civil disobedience. (Of those participating in one or two aggressive actions, 46 percent were involved in rent or tax strikes, 39 percent were involved in a wildcat strike, 17 percent were involved in the seizure of a factory, office, or other type of building, 16 percent were involved in fights with police or other demonstrators, and 4 percent were involved with a revolutionary group.) The Low category is defined by lack of involvement in any aggressive action.

There are four distinct zones or levels on the Aggressive Behavior index when we look at the relationship between it and actual participation. The vast majority of persons who score from 0 to 1 on the composite index fall in the Low category of actual participation (98 and 96 percent, respectively). This is clearly an *Inactivity* zone.

A clear preponderance of persons who score from 2 to 3 on the composite index also fall in the Low category of actual participation (75 and 78 percent, respectively). A minority of these persons have participated in one or two behaviors—mostly nonviolent civil disobedience. This is clearly a *Low Civil Disobedience* zone.

The majority of persons who score from 4 to 7 on the composite index fall in the Medium category of actual participation (54, 50, 95, and 72 percent, respectively). This may be designated as a *High Civil Disobedience* zone.

Finally, all persons who score from 8 to 10 on the composite index fall in the High category of actual participation. This may be designated as a *Political Violence* zone.

The relationship between the composite Aggressive Behavior index and an index of Intention to Participate in aggressive behavior is given in Table A2. The Intention to Participate index is a summation of scores across the five intention items, where negative intention is scored "0;" conditionally positive intention is scored "1" (might do it), and unconditionally positive intention is scored "2" (definitely would do it). To fall in the High category of intention (5 to 10) a person must have expressed at least a conditionally positive intention toward performance of one of the violent actions. The Low category is defined by consistently negative intention toward performance of all the actions. The Medium category represents conditionally or unconditionally positive intention toward performance mainly of civil disobedience actions.

All persons who score at 0 on the composite index have consistently negative intention toward performance of aggressive behavior and, although all of those who score at 1 on the composite index fall in the Medium category of intention, the vast majority of these express only a single conditionally positive intention (85 percent). Thus, the Inactivity zone is characterized by very consistent negative intention toward performance of aggressive behavior.

Nearly all persons who score from 2 to 3 on the composite index fall in the Medium category of intention, and the vast majority express more than one conditionally positive intention (87 and 92 percent, respectively). Thus, the

Table A3. Relationship Between Participation in Discrete Aggressive Activities and Composite Aggressive Behavior Index

Aggressive Behavior Index	Aggressive Activities																	
	Refuse Pay Rent, Taxes						Illegal Strike						Seize Buildings					
	Negative		Transitional		Positive		Negative		Transitional		Positive		Negative		Transitional		Positive	
	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)
Violence (8–10)	0	(0)	1.7	(10)	12.2	(17)	0	(0)	0.7	(4)	18.9	(23)	0	(0)	0.4	(2)	30.0	(24)
High C.D. (4–7)	0.7	(12)	35.0	(200)	63.3	(88)	0.2	(3)	38.4	(235)	50.8	(62)	0.3	(6)	48.0	(243)	65.0	(52)
Low C.D. (2–3)	5.3	(93)	46.7	(267)	24.5	(34)	4.7	(82)	44.1	(270)	30.3	(37)	8.2	(155)	45.5	(230)	5.0	(4)
Inactive (0–1)	94.1	(1663)	16.3	(95)	0	(0)	95.1	(1646)	16.8	(103)	0	(0)	91.4	(1718)	6.1	(31)	0	(0)
TOTAL	100.0	(1768)	100.0	(572)	100.0	(139)	100.0	(1731)	100.0	(612)	100.0	(122)	100.0	(1879)	100.0	(506)	100.0	(80)
			d_{yx} = .81						d_{yx} = .81						d_{yx} = .90			

Aggressive Behavior Index	Aggressive Activities											
	Fight Police, Demonstrators						Use Violence Against Government					
	Negative		Transitional		Positive		Negative		Transitional		Positive	
	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)
Violence (8–10)	0.0	(1)	0.6	(1)	42.4	(25)	0.1	(2)	5.4	(13)	80.0	(12)
High C.D. (4–7)	6.0	(137)	81.2	(125)	54.2	(32)	5.3	(117)	72.1	(173)	20.0	(3)
Low C.D. (2–3)	16.2	(368)	14.9	(23)	3.4	(2)	16.0	(353)	16.3	(39)	0	(0)
Inactive (0–1)	77.7	(1759)	3.2	(5)	0	(0)	78.6	(1736)	6.3	(15)	0	(0)
TOTAL	100.0	(2265)	100.0	(154)	100.0	(59)	100.0	(2114)	100.0	(203)	100.0	(15)
			d_{yx} = .89						d_{yx} = .85			

Low Civil Disobedience zone on the composite index is characterized by consistently positive intention toward participation in civil disobedience actions but generally negative intention toward participation in more violent action.

Those who score from 4 to 7 on the composite index manifest progressively greater positive intention toward participation in violent action, as evidenced by the increasingly larger proportions who fall in the High category of intention (36, 76, 90, and 94 percent, respectively). The High Civil Disobedience zone on the composite index is thus characterized by increasingly greater potential for participation in political violence.

Virtually all of those who score from 8 to 10 on the composite index fall in the High category of intention. The Political Violence zone is characterized by very consistent positive intention toward participation in violent action.

Thus, combining of intention to participate and past participation into a single composite index gives a measure which is not only sensitive to actual past behavior but also is future-oriented and corresponds to a theoretically plausible conceptualization of the aggressive political behavior continuum, according to which, as one goes from low to high on the continuum, one moves from (1) inactivity to (2) potential for civil disobedience, to (3) actual participation in civil disobedience and potential for violent action, and finally to (4) actual participation in political violence. In the analysis reported in the text we collapse the Aggressive Behavior index into the four zones just defined.

The relationship between each aggressive behavior and the collapsed composite index is

Table A4. Zero-Order Correlations (γ) Between Index of Aggressive Behavior Participation and Political Affect Variables

Correlation Between Actual Aggressive Behavior Participation and							
Political Support		Political Trust		Policy Output Evaluation		Incumbent Evaluation	
γ	(N)	γ	(N)	γ	(N)	γ	(N)
.56	(2215)	.37	(2376)	.36	(2301)	.19	(2260)

Table A5. Correlations (γ) Between Aggressive Behavior Participation Index and Political Affect Variables, with Variables Controlled

Controlling for:	Correlation Between Actual Aggressive Behavior Participation and:			
	Political Trust		Policy Output Evaluation	
Political Support	γ	(N)	γ	(N)
Very positive	−.14	(92)	.33	(91)
Positive	.16	(553)	.23	(542)
Intermediate	−.02	(919)	−.03	(915)
Negative	−.07	(414)	.08	(417)
Very negative	.32	(182)	.16	(182)

	Political Support	
Political Trust	γ	(N)
Very positive	−.30	(81)
Positive	.55	(441)
Intermediate	.41	(954)
Negative	.50	(541)
Very negative	.64	(143)

	Political Support	
Policy Output Evaluation	γ	(N)
Very positive	.70	(52)
Positive	.49	(501)
Intermediate	.45	(1104)
Negative	.63	(436)
Very negative	.61	(54)

shown in Table A3. Scale-item correlations are very high.

For reasons mentioned above and in the text, we feel that the Aggressive Behavior index is superior to the index of actual participation. The following is a representative selection of correlations between the political affect variables under various conditions and the index of actual participation in aggressive behavior. This Index's correlations with the affect variables are not markedly different than those with the Aggressive Behavior index reported in the text. On the whole, they tend to be slightly lower, but in no instance do they materially alter the interpretation of the relationships using the Aggressive Behavior index.

Table A6. Correlations (γ) Between Index of Actual Aggressive Behavior Participation and Political Affect Variables and Other Political Affect Variables

Controlling for:		Correlation Between Actual Aggressive Behavior Participation and:			
		Political Trust		Policy Output Evaluation	
Community Type	Political Support	γ	(N)	γ	(N)
Rural and Urban	Positive[a]	.12	(504)	.24	(492)
	Intermediate	–.27	(540)	–.16	(530)
	Negative[b]	–.41	(156)	–.44	(156)
University	Positive[a]	–.02	(141)	.07	(141)
	Intermediate	.10	(379)	–.05	(385)
	Negative[b]	.29	(440)	.23	(443)

		Political Support	
Community Type	Political Trust	γ	(N)
Rural and Urban	Positive[a]	.31	(381)
	Intermediate	.09	(505)
	Negative[b]	.60	(314)
University	Positive[a]	.46	(141)
	Intermediate	.29	(449)
	Negative[b]	.36	(370)

		Political Support	
Community Type	Policy Output Evaluation	γ	(N)
Rural and Urban	Positive[a]	.38	(414)
	Intermediate	.17	(558)
	Negative[b]	.41	(206)
University	Positive[a]	.31	(139)
	Intermediate	.33	(546)
	Negative[b]	.45	(284)

[a] Includes "Very Positive."

[b] Includes "Very Negative."

Table A7. Correlations (γ) Between Aggressive Behavior Participation Index and Political Affect Variables, Controlling for Ideological Commitment

Controlling for:		Correlation Between Actual Aggressive Behavior Participation and:			
		Political Trust		Policy Output Evaluation	
Ideological Commitment	Political Support	γ	(N)	γ	(N)
Left[a]	Positive[c]	.33	(200)	.58	(198)
	Intermediate	.20	(298)	.09	(304)
	Negative[d]	.27	(402)	.20	(406)
Center	Positive[c]	.09	(251)	.10	(245)
	Intermediate	–.18	(338)	.08	(333)
	Negative[d]	–.23	(86)	–.06	(84)
Right[b]	Positive[c]	–.14	(137)	.24	(135)
	Intermediate	–.06	(206)	–.26	(201)
	Negative[d]	.08	(66)	–.26	(68)

		Political Support	
Ideological Commitment	Political Trust	γ	(N)
Left[a]	Positive[c]	.75	(217)
	Intermediate	.43	(377)
	Negative[d]	.33	(306)
Center	Positive[c]	.13	(199)
	Intermediate	–.12	(309)
	Negative[d]	–.05	(171)
Right[b]	Positive[c]	.15	(68)
	Intermediate	–.07	(181)
	Negative[d]	.50	(160)

		Political Support	
Ideological Commitment	Policy Output Evaluation	γ	(N)
Left[a]	Positive[c]	.68	(213)
	Intermediate	.49	(454)
	Negative[d]	.40	(241)
Center	Positive[c]	–.10	(187)
	Intermediate	–.08	(372)
	Negative[d]	.16	(103)
Right[b]	Positive[c]	.59	(84)
	Intermediate	.19	(213)
	Negative[d]	–.06	(107)

[a] Includes “Extreme Left.”
[b] Includes “Extreme Right.”
[c] Includes “Very Positive.”
[d] Includes “Very Negative.”

COMMUNICATIONS

An Apology to Alexander L. George

TO THE EDITOR:

Since Alexander George has been exposed to unpleasantness as the result of his blameless compliance with a request of mine in connection with the *Review,* I believe I owe him an apology in these pages. As a great many busy scholars do each year, Professor George undertook to read and evaluate a manuscript at my request. Through a clerical error in our office, a copy of this manuscript containing annotations in his handwriting, but not identifying him in any other way, was passed on to James David Barber when I invited Professor Barber to prepare a comment on it for publication. Further conversation ensued between Professor George and Professor Barber, to which I was not privy, which establishes to my satisfaction that I failed to protect Professor George's anonymity as I should have.

Professor Barber's comment was published in the March 1977 *APSR.* Faithful readers of the *Review* will no doubt recall that this comment contains many statements that speak unkindly of my management of the editorial process and consequently I felt constrained from subjecting it to editorial modification. However, I welcomed Charles O. Jones' suggestion that he write Professor Barber with editorial suggestions; these were ignored. In the course of this activity I lost sight of my further responsibility to protect Professor George from the publication of untrue comments referring to him. If I had seen his interests clearly and promptly, I think I would have acted differently, either to seek the removal of the offending language, or to provide space for a rebuttal by Professor George in the same issue. After Professor George called the error to my attention, he did receive this space in the June *APSR.* I am very sorry, however, to have contributed to a situation that has caused him personal distress and especially so since this occurred as the result of his willingness to contribute his knowledge to the editorial process.

NELSON W. POLSBY

University of California, Berkeley

From Confusion to Confusion

TO THE EDITOR:

In the March, 1977 issue, Michael Margolis criticizes, among other studies of American voting behavior, my 1972 *APSR* article, "From Confusion to Clarity." While I welcome scholarly controversy, I would like briefly to answer his direct criticisms.

1. Margolis' major point is that my analysis is not based on the total number of respondents, but on a smaller group. The crucial question we analyze is the changing proportion of respondents who believe the Democrats are more liberal than the Republicans on a series of policy issues. Margolis here uses the entire sample; I use only those voters who see a difference of any kind between the parties. Even using his method, there is a trend evident toward greater perceived party differences from 1956 to 1968. The crucial point, however, is that my method is exactly the same as that used in *The American Voter* (p. 182 ff.), a book Margolis still regards as holy writ. Since I was arguing that the conditions described in that volume of the 1950s had changed, it is important to replicate its techniques exactly. By that exact replication—rather than the new techniques Margolis attacks—we find a change in voter awareness.

2. While conceding the trend to 1968, Margolis argues it does not continue through the 1972 election. However, if one continues to use the same methods as I did previously, we find the same voter awareness in 1972 as in other recent years. Thus, in an updated publication available to Margolis, I find that 86 percent of all voters, and more than 80 percent of each party identification group, agree that the Democrats are more liberal on issues of medical care and a federal job guarantee (*Voters' Choice,* Harper & Row, 1975, p. 172). These are the only questions available throughout the 1956–72 period, and they again show a definite increase in the electorate's consciousness. The same trend can be seen in perceptions of the parties' general ideological positions. In 1960, nearly half of the Democrats thought their party was more conservative or as conservative as the Republicans. By 1972, the propor-

tion was reduced to a fourth. Republicans and Independents followed the same pattern, "from confusion to clarity" (*Voters' Choice,* p. 181).

3. Margolis then introduces new issues into his analysis, those of integration in places of public accommodation, strong federal government, and Vietnam. Obviously, these are irrelevant to my analysis, which deals only with issues current from 1956 on.

4. At three different places, Margolis departs from his usual scholarly courtesy to take a "cheap shot" at the absence of the raw Ns on which the table percentages are based. Admittedly, it is good academic practice to include these figures. But Margolis knows these figures, since his printout comes from the same SRC/CPS data sources as mine. Moreover, he knows the Ns are substantial and cannot affect the statistical relationships shown. (To give one typical example: attitudes on aid to education in 1960 are based on Ns of 1704, 1439, and 792, in Tables 1, 2, and 3 respectively.) To stress the omission is to stress the form of tables over the substance of findings.

More generally, I believe Margolis has missed the major point of my work and that of others who have argued that voters are now responsive to issue-oriented politics. That point emphasizes the political context of voter behavior. Given appropriate electoral cues, persuasive leadership, and concerned parties, we believe the citizens of the United States can translate their issue preferences into electoral decisions. The movement to clarity in American politics is evident to virtually every observer. Hopefully the remaining confusion in political science will soon be relieved as well.

GERALD M. POMPER

Rutgers University

TO THE EDITOR

I too believe that, given the appropriate cues, citizens are capable of translating their issue preferences into electoral decisions. Professor Pomper and I disagree, however, about the degree to which citizens have succeeded in making this translation.

My argument concerning the lack of Ns is not a mere quibble over form. At various stages of his analysis, Pomper excludes from consideration those with no party identification, those with no opinion on the issues examined, or those who perceive no differences between the parties. As a result, he comes up with figures which in my judgment substantially exaggerate the extent of issue voting for the years 1956–68. Reporting the sample Ns and the Ns upon which his figures were based would have avoided this exaggeration. (See table 1 of my paper.)

My introduction of new issues serves to emphasize that the major issues of the 1950s and early '60s were not the same as those of the late '60s and early '70s. If issue voting has substantially increased, it would seem that voters ought to be able to relate their preferences on these new issues to their perceptions of the policy positions of the parties. In Table 3 of my paper the figures demonstrate that voters' perceptions of party differences regarding public accommodations and strong government in Washington became increasingly muddled rather than clarified between 1964 and 1972.

The figures Pomper cites from *Voters' Choice* regarding perceptions of party differences on the issues of medical care and job guarantee in 1972 evidently exclude the one-third to one-half of the electorate who have no opinions or no perceptions of the party positions on the issues. Unfortunately, we have no way of knowing for sure just how many voters have been excluded, for once again Professor Pomper fails to report the Ns upon which his percentages are based!

MICHAEL MARGOLIS

University of Pittsburgh

Indian Rights

TO THE EDITOR:

Professor Frances Svensson, in her review (*APSR*, Dec. 1976, 1306–08) of my book, *The States and Their Indian Citizens* (Washington, D.C.: GPO, 1972), says I looked to "the inevitability of the disintegration of community interests in favor of individual ones" rather than considering the "roles of individuals and groups in a democratic society."

In fact, I pointed out that Indian tribal leadership looks to developing self-sufficient Indian communities and individuals. Both are appropriate goals; not all Indians live in Indian communities. For those Indians living in communities with tribal governments there is the possibility of developing community self-sufficiency and integrating such communities into the U.S. political structure. In *The States . . .* (p. 153) the Menominee example of becoming a Wisconsin county is cited, and the possibility of considering the Navajo for our 51st state is mentioned. The genius of our governmental framework and our political system is that it is flexible and can accommodate pluralism: "... the majority of tribal leaders where they now have tribal governments seemed to see no major difficulty in the Indian governments working with local, state, and federal governments to resolve problems as they arise.... There would seem to be little question but that Indian governments will continue to exist for as long as the Indian communities involved want them" (p. 153).

Also Svensson apparently missed my statement that the majority society should not try to change Indians' or others' pluralistic goals "except insofar as they interfere with the rights of others" (p. 128). I cite the Amish as being economically self-sufficient, leading their own lives in their own way, and enjoying maximum freedom as a culturally diverse group (p. 128). The Indians may do the same.

The reviewer raised the question of adequate treatment of Indian relations with the states, in particular, the tension between Indian groups and the states. There is no more tension in the Indian-state relationship than in the Arlington County-Commonwealth of Virginia relationship. *The States* cites examples of Indian desires to cooperate with the state and achieve the benefits of full participation in state programs (pp. 91–92). For example, 68 percent of the Indian children in states with federally recognized Indians are in public schools operating under state law.

The tensions cited by Svensson are rarely discussed in conjunction with all of the related facts. Neither the federal government nor Indian governments are equipped to provide certain services to Indian communities. Foster home care or specialized institutional care for dependent or handicapped children; institutions for the mentally ill, aged, or infirm; judicial procedures to place children in foster homes or state institutions; state correctional institutions; and institutions of higher learning—all these are examples of functions performed by state governments used by all citizens, including Indians in reservation communities (p. 74). Having separate law and order on Indian reservations, in fact, frequently leads to increased tensions.

House Concurrent Resolution 108 (termination) and P.L. 280 (permitting state law and order on Indian reservations) were specifically mentioned by Professor Svensson as tension producing. No termination ever took place without tribal consent, including Menominee (pp. 63–64; pp. 220–223). In most cases P.L. 280 jurisdiction was assumed by the state only if the Indians concerned concurred (pp. 36–37). In Nevada eight counties refused to take jurisdiction even though the Indians therein petitioned them to do so (p. 36). These facts are conveniently lost in the theory and rhetoric of advocates of Indian sovereignty.

Professor Svensson does not address the book's discussion of "Who is an Indian?" There may be 10 million people in this country with some Indian blood (p. 139), but only 800,000 were identified as Indians in the 1970 census, of which 37 percent lived in metropolitan areas. Only about 500,000 have any federal reservation connection (p. 176). Is a person with 1/16 degree of Indian blood an Indian? Which "Indians" are the concern of the reviewer? I tried to discuss all aspects.

I did not "defend the argument that government-sponsored policies of enforced acculturation were ultimately in the best interests of the Indians" as alleged by Professor Svensson. On the contrary I stated my belief that "permissive acculturation" would have facilitated adjustment, but that it was impossible at the time of the Allotment Act of 1887 (p. 15). Svensson asks: "Why and how in 'pluralistic' American society did there come to be no alternative? Why wasn't the environment conducive?" I tried to convey the temper of the times, including Theodore Roosevelt's statement that the tribes had to be broken up (pp. 15–18). Footnote 42, p. 17, refers to Appendix I which treats the environment in more detail. Secretary of Interior Carl Schurz pointed out in 1881 that feeling against the Utes was running high in Colorado and allotment was the only device that saved a portion of their lands. Without allotment the Ute tribe would have been destroyed and all of the land taken (p. 225). Later the national attitude changed (pp. 19–26).

Professor Svensson and I are probably more in agreement than the above might indicate. The concept of culture change (p. 159 ff.) and Indian potential as I see it (p. 145 ff.) bear on the relation of the individual to the group (Svensson's concern) and the response of both to their environment. But I have some trouble with her statement that "the pre-eminence of communal responsibilities and interests over individual rights does not necessarily lead to tyranny, particularly within a tribal rather than a state political structure." Some of the Pueblo government practices are incompatible with the Bill of Rights. This is a sensitive and important issue and relates to the "human rights" pronouncements of President Carter. I agree with Professor Svensson's suggestion that additional study is needed, but, as she correctly says, I favor a state arrangement with its constitutional protection of individuals.

THEODORE W. TAYLOR

Arlington, Virginia

TO THE EDITOR:

I find it difficult to know where to start in replying to Mr. Taylor, since we do not even agree on the "facts" of many of the cases, let alone their meaning. At base, I suspect, we are simply speaking past one another. Mr. Taylor refers to the possibility of Indian communities "integrating . . . into the U.S. political structure." What if they don't want to? His choice of the Menominee example is especially apt, since it is a classic example of a tribe which was coerced in the crudest possible ways by the federal government to accept termination and which, after a struggle of many years, has finally been able to regain some semblance of its former status. And let's face it, the Navajo are never going to be a state.

Mr. Taylor also remarks that he too is for Indian goals "except insofar as they interfere with the rights of others." He notes that: "Some of the Pueblo government practices are incompatible with the Bill of Rights." The issue is: who decides what a "right" is and how it should be recognized and whose conception of "rights" should have priority? Pueblo ways of doing things antedate American ones, and have been found quite satisfactory to the Indians involved. I refer Mr. Taylor to the hearings held in Pueblo country on the "Indian Bill of Rights" in 1968. The "right" of internal self-determination of Indian tribes has been reinforced in numerous treaties and court decisions (including those of Chief Justice Marshall). If Indians choose in some cases to emphasize a code of rights which does not exactly correspond to the U.S. Bill of Rights, they are entitled to do so. Where is all the flexibility and pluralism that Mr. Taylor ascribes to the U.S. political system?

Mr. Taylor's rosy perspective on relations between the tribes and the states, I must confess, boggles the mind. I simply refer him to the record on Indian reaction to H.C. 108 and P.L. 280. Mr. Taylor's gloss on "tribal consent" to termination makes no reference to the enormous pressures put on the tribes to go along; an example is the refusal of the federal government to release money granted to the Menominee by the Indian Claims Commission in compensation for lands illegally seized from the tribe until the tribe agreed to termination. And this was only one of many pressures placed on the Menominee alone! And I must correct Mr. Taylor; a number of states did assume jurisdiction on Indian reservations under P.L. 280 without Indian concurrence. The original bill did not require Indian concurrence! (It was later modified.) While some tribes did desire it, many others did not. Is Mr. Taylor aware of the referendum which the United Sioux Tribes organized in South Dakota to reject P.L. 280? Speaking of tribal-state relations, does Mr. Taylor know what it is like to be an Indian in South Dakota? The "increased tension" that Mr. Taylor attributes to separate jurisdictions arises more from the outraged sensitivities of whites who refuse to submit to Indian jurisdiction than to Indians off the reservation who object to white jurisdiction there. Particularly good examples of the complexities of this jurisdiction question are to be found in Washington State, which assumed jurisdiction originally without concurrence from all the tribes (in violation of its own charter of incorporation) and which has been reaping the consequences in recent Indian-state confrontations on a number of reservations (e.g., Colville, Yakima, Puyallup, etc.).

The crucial issue underlying our divergent perspectives centers, I think, on our approach to history. Mr. Taylor's position comes down to: whatever was, had to be. My position comes down to: why was it that way and not another? I know all about the views of Andrew Jackson and Theodore Roosevelt, and for that matter Cotton Mather and John Wayne, on Indians. What I am interested in is why their views triumphed; why more balanced views of the requirements of justice and equity—and even decency—did not. And I am interested in what this analysis reveals about the American political process, about its "flexibility" and "pluralism," and about the problems of ethnic relations in multiethnic societies.

If anything, our exchange is useful as itself an example of the difficulties of communication across boundaries. Mr. Taylor's views seem generally to correspond to those of many persons involved in the formulation of Indian policy in this country, both in the B.I.A. and in other agencies and offices (including Congress). My views, on the other hand, are those of an Indian who has been involved in Indian affairs and organizations most of my life. Mr. Taylor prefers "a state arrangement with its constitutional protection of individuals" as the final resolution of the problem of Indian status. I prefer the right of Indian tribes and communities to make their own choice, which should include the treaty-guaranteed option of autonomous internal self-determination, whether or not its form corresponds to American political conceptions.

FRANCES SVENSSON

University of Michigan

Financing Elections

TO THE EDITOR:

In his review essay in the March 1977 *APSR*, David Adamany assessed my book, *Financing Elections: The Politics of an American Ruling Class.* Although he was kind enough to give credit where he saw it due, the reviewer unfortunately made a number of errors which demand correction in order to set the record straight.

Adamany said I overstated the degree to which the corporate rich give to both parties; but nowhere did I imply that more than a minority split contributions, and I reported a compilation by the authoritative Citizens' Research Foundation on the amount of split giving.

The reviewer said I gave incorrect data on the amount of big McGovern contributions; but what I really did was to *predict* that when the official figures were in they would show that McGovern raised no more than half his money from givers of $100 or less, and this prediction has since been borne out.

The reviewer said I garbled the text of the 1971 Election Finance Law; on review I find the provisions were accurately summarized.

Adamany said I was unaware of studies of the decline in dues revenue by European parties; in fact I was quite aware of these, but was basing my argument that dues financing is the only really democratic method on the reversal of present trends, not on their continuation.

The reviewer said I contributed no new research; but my study of the economic backgrounds of the 1968 Cleveland metropolitan area big givers, reported in the book, constituted contributory "evidence on the sociology of givers"—something to which Adamany attached considerable importance elsewhere in his review essay.

Finally, Adamany views my argument about the structure of corporate power and its incompatibility with democracy as wrong, and is particularly upset about my analysis of the broadly conservative purposes of the election finance reformers. Now, this was the core argument of the book and can hardly be profitably reargued in the space of a communication. I must point out, however, that Adamany confused my analysis with elitist arguments like Domhoff's. In reality, my book was a Marxian analysis of power, with particular attention to the role of the electoral process.

DAVID A. NICHOLS

State University of New York at Albany

EDITORIAL NOTE

From time to time I will use this space to explain procedures and policies. We try to provide sufficient information in the "Instructions to Contributors" but on occasion it may be helpful to supply additional information.

An issue of current interest is that of critical analyses of and comments on a scholar's work or reputation. As with previous editorial staffs, we see it as the continuing function of the journal to *encourage and promote professional exchange on major scholarly issues.* The precise means by which this policy is implemented may, and should, be expected to vary from one editorial staff to the next. Whatever means are employed, however, we wish to reaffirm the following policy of *The American Political Science Review:* The discourse in scholarly exchange is expected to follow the commonly accepted standards of civility. Should these standards be violated for whatever reason, the offended person will be given an opportunity to respond. Where feasible this response will appear in the same issue.

Articles Accepted for Future Publication

M. L. Balinski and H. P. Young, International Institute for Applied Systems Analysis (Schloss Laxenburg, Austria), "Stability, Coalitions and Schisms in Proportional Representation Systems"

Paul Burstein, Yale University, "Social Cleavages and Party Choice in Israel: 'A Log-Linear Analysis' "

Melissa A. Butler, Wabash College, "Early Liberal Roots of Feminism: John Locke and the Attack on Patriarchy"

George W. Carey, Georgetown University, "Separation of Powers and the Madisonian Model: A Reply to the Critics"

Arthur DiQuattro, University of Washington, "Alienation and Justice in the Market"

Lloyd S. Etheredge, Massachusetts Institute of Technology, "Personality Effects on American Foreign Policy, 1898–1968: A Test of Interpersonal Generalization Theory"

John W. Ferejohn and Roger G. Noll, California Institute of Technology, "Uncertainty and the Formal Theory of Political Campaigns"

Morris P. Fiorina and Charles R. Plott, California Institute of Technology, "Committee Decisions Under Majority Rule: An Experimental Study"

Mark N. Franklin, University of Strathclyde, and Anthony Mughan, University College (Cardiff, Wales), "The Decline of Class Voting in Britain: Problems of Analysis and Interpretation"

Norman Frohlich, Joe A. Oppenheimer, Jeffrey Smith and Oran R. Young, University of Texas at Austin, "A Test of Downsian Voter Rationality: 1964 Presidential Voting"

Fred M. Frohock, Syracuse University, "The Structure of 'Politics' "

Norman Furniss, Indiana University, "The Political Significance of the Public Choice-Property Rights School"

Douglas S. Gatlin and Michael W. Giles, Florida Atlantic University, and Everett F. Cataldo, Cleveland State University, "Policy Support Within a Target Group: The Case of School Desegregation"

Dante Germino, University of Virginia, "Eric Voegelin's Framework for Political Evaluation in His Recently Published Work"

James L. Gibson, University of Wisconsin at Milwaukee, "Judges' Role Orientations, Attitudes and Decisions: An Interactive Model"

John G. Gunnell, State University of New York at Albany, "The Myth of the Tradition"

Barbara Kellerman, Tufts University, "Mentoring in Political Life: The Case of Willy Brandt"

Samuel Kernell, University of California, San Diego, "Explaining Presidential Popularity"

James H. Kuklinski, Wichita State University, "Representativeness and Elections: A Policy Analysis"

Richard D. McKelvey, Peter C. Ordeshook, and Mark D. Winer, Carnegie-Mellon University, "The Competitive Solution for N-Person Games Without Transferable Utility, With an Application to Committee Games"

Manis I. Midlarsky, University of Colorado, "Analyzing Diffusion and Contagion Effects: The Urban Disorders of the 1960s"

David W. Moore and B. Thomas Trout, University of New Hampshire, "Military Advancement: The Visibility Theory of Promotion"

John M. Orbell, University of Oregon, and L. A. Wilson II, University of Nevada, "Institutional Solutions to the N-Prisoners' Dilemma"

Charles W. Ostrom, Jr., Michigan State University, "A Reactive Linkage Model of the U.S. Defense Expenditure Policymaking Process"

John G. Peters and Susan Welch, University of Nebraska at Lincoln, "Political Corruption in America: A Search for Definitions and a Theory, or, If Political Corruption Is in the Mainstream of American Politics Why Is It Not in the Mainstream of American Politics Research?"

David E. Price, Duke University, "Policy-Making in Congressional Committees: The Impact of 'Environmental' Factors"

Arlene W. Saxonhouse, University of Michigan, "Comedy in Callipolis: Animal Imagery in the *Republic*"

Donald D. Searing, University of North Carolina, "Measuring Politicians' Values: Administration and Assessment of a Ranking Technique in the British House of Commons"

Donley T. Studlar, Centre College of Kentucky, "Policy Voting in Britain: The Colored Immigration Issue in the 1964, 1966, and 1970 General Elbctions"

Sidney Verba, Harvard University, and Goldie Shabad, University of Chicago, "Workers' Councils and Political Stratification: The Yugoslav Experience"

Robert Weissberg, University of Illinois at Champaign, "Collective vs. Dyadic Representation in Congress"

On Values and Science:
The Korean Decision Reconsidered

GLENN D. PAIGE
University of Hawaii

All political scientists recognize the importance of values in science, but rarely do we have so clear an example of it in a single case. The Korean Decision, *originally written from a violence-accepting standpoint, is reviewed here from a nonviolent value position. It is argued that such a value shift calls for sharper analytical focus upon violence, for further decontamination of proviolent language, for more vigorous exploration of nonviolent alternatives, and for the creation of comparative actor-observer value profiles to assist awareness and control of biases in research. It is hoped that this reanalysis will assist the transition of political science as a violence-accepting and violence-legitimating social science discipline toward greater emphasis upon the creation and application of nonviolent knowledge.*

> *Science itself is not a liberator. It creates means, not goals. . . . We should remember that the fate of mankind hinges entirely upon man's moral development.* (Einstein)[1]

In *The Korean Decision*[2] I tried to make a contribution to the scientific study of international politics by exploring in a first case study the decison-making approach to analysis that has been suggested by Snyder, Bruck, and Sapin.[3] Since the period of research and writing that resulted in publication of *The Korean Decision* I have changed my personal value position toward violence from acceptance to rejection. The purpose of this essay, therefore, is to explain the principal differences this makes in the original Korean decision analysis.

This should interest both social scientists and critics of social science. We all profess awareness of the importance of values in social science research, but we rarely have so clear an example of it in a single case.

Original Violence-Accepting Approach

The intent of the original study was to describe the series of decisions that led to American engagement in the Korean War; to reconstruct them from the point of view of the decision makers; to analyze them in terms of the interaction of organizational, informational, and motivational variables; to evaluate them; and to seek guidance for coping with future war-prone crisis situations. Thus I devoted two background chapters to explaining pre-decisional domestic and international conditions, seven narrative chapters to describing daily decision-making events from June 24 to June 30, 1950, an empirical analysis chapter to suggest correlations among the decision-making variables, a normative analysis chapter to evaluate the decisions, and a final chapter to suggest guidelines for "crisis management" in Korea-like situations.

Underlying all—reconstruction, analysis, evaluation, and prescription—was my normative acceptance of the employment of violence in politics, both domestic and international. Although generally to be avoided, occasions could arise in which political violence would be inescapable, just, and even heroic. My views on violence coincided exactly with those of the American decision makers whom I studied and were reinforced by my adolescent political socialization during World War II and by a personal sense of just participation in resisting blatant Communist aggression as an antiaircraft artillery communications officer in Korea from 1950 to 1952. Such views on the conditional acceptability of violence were merely the dominant mode of thinking of the mid-twentieth century in which we lived. Almost all political leaders, revolutionaries, counterrevolutionaries, political scientists, and other citizens held essentially the same views. The main political arguments of the age were not about violence per se but rather about the ends of violence and, with the advent of nuclear weapons, increasingly about its scale.

The method of decision-making analysis that I employed did not explicitly require acceptance of a violent or nonviolent value position. Rather it took the form of a value-neutral set of analytical tools. Implicitly, however, in this case it encouraged the acceptance of proviolent value assumptions (a) by stressing that decisions ought to be understood primarily as seen through the eyes of the decision makers, and (b) by not containing methods for explicating researcher values, for comparing them with those of actors, for measuring their effects upon analysis, and for evaluating decisional outcomes. Although actor values were given explicit attention, observer values were not. They were left to vary with the professional conscience of the researcher.

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[1] Otto Nathan and Heinz Norden, eds., *Einstein on Peace* (New York: Simon and Schuster, 1960), p. 312.

[2] Glenn D. Paige, *The Korean Decision: June 24–30, 1950* (New York: The Free Press, 1968).

[3] Richard C. Snyder, H. W. Bruck, and Burton Sapin, *Foreign Policy Decision-Making* (New York: The Free Press, 1962).

Because I believed that social scientists should make explicit their value preferences as indicators of possible factual and interpretive biases in their scientific works, I recorded my personal judgment of the Korean decision: "The writer regretfully cannot accept the pacifist view that the North Korean attack should not have been resisted by the armed forces of the Republic of Korea and such international allies as they could muster."[4] Although not made completely clear in the original text, this judgment rested upon two beliefs: (a) that violent extension of their domains by unjust regimes justified counterviolence, and (b) that the state of civil liberties in South Korea was better than that in the opposing North. I wrote in 1968, "Less than twenty years after the event, the Republic of Korea seems to be traversing a far more open and spontaneous path of development than that of its Northern counterpart. . . . Without the Korean decision, this would not have been possible."[5] In short, American violence had contributed to peace and freedom in Korea. Therefore the decision to fight was good.

Nonviolent Value Change

It is not essential to accept or understand the reasons why I changed to a nonviolent value position in order to appreciate the effects of this change upon re-analysis of *The Korean Decision*, but since colleagues and students have expressed keen interest in them an explanation is necessary.

At the conscious level, I am aware of the converging effects of three principal factors: public commitment to a proviolent value position, realization that Korean conditions were developing contrary to the values taken to justify violence, and discovery that we Americans who were self-righteously committed to threats of violence in Korea were ourselves obstacles to the creation of nonviolent alternatives in international relations.

By 1973 the repressive nature of the Republic of Korea political regime had become globally notorious, mainly through the activities of the Korean CIA at home and abroad. This included the drugging and kidnapping from Japan of opposition presidential candidate Kim Dae Jung, the persecution of the poet Kim Chi Ha and Catholic Bishop Daniel Chi, and the stifling of other voices of legitimate dissent in the press, universities, and the religious community. This has been accompanied by the progressive elaboration of violence-based laws and decrees that make the pacific transfer of power increasingly unlikely.

Growing awareness of increasing repressiveness in Seoul was combined with what was the startling discovery that the main obstacle to the establishment of peaceful cultural relations between Americans and scholars from North Korea was the United States government. Meeting in Paris with scholars from the North Korean Academy of Sciences in the summer of 1973, I invited them on behalf of the University of Hawaii to visit Honolulu. They were eager to come. How shocking it was for me to discover that the American ambassador in Seoul, the Washington Korean desk officer, and the Secretary of State were adamantly opposed to such a visit and refused to give assurances that entry visas would be issued. Although the Department of State was receptive to visits to North Korea by certain Americans such as Professor Jerome Cohen of Harvard Law School and Selig Harrison of the *Washington Post*, it was adamantly opposed to reciprocal American hospitality. This meant no aloha for North Koreans.

Against this background, it was especially disturbing for me, during a visit to the Hiroshima atomic bomb Peace Park in August 1975, to hear on a portable radio a statement by the U.S. Secretary of Defense that the government would not give assurances that nuclear weapons would not be employed in an American response to a renewed outbreak of fighting on the Korean peninsula.

In effect the United States government was threatening nuclear war in defense of a repressive regime, while obstructing the development of peaceful relations between American citizens and those of a potential military adversary. These were definitely not the conditions of freedom and peace to which the wartime killing of 1950 to 1953 had been devoted.

For me, this represented an intolerable situation of cognitive dissonance. Violent means had proved inimicable to peaceful ends. I could attempt to change reality by further commitment to the value of violence; I could elect nonviolence and then seek reality change; or I could deny the conflict and withdraw. In actuality I experienced a profound change in attitude toward violence from acceptance to rejection.

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[4] Paige, p. 352.

[5] Ibid., p. 354.

Furthermore, I experienced this change in a general sense, not just in Korea-specific terms. Perhaps this was because I had always approached the study of Korean history and contemporary society as a social scientist seeking to understand the general from the particular. Partly because of the uniquely intense concentration of American, Chinese, Japanese, and Russian influences upon the Korean people over the past century I have always thought that this convergent experience offered extraordinary possibilities for global insight. Korea thus became for me a broken link in the chain of violence forged by human history, a chain in which the glorification of each preceding link becomes the justification for its successor. But let us examine the implications of such a value change for the scientific single case analysis attempted in *The Korean Decision.*

Implications for Background Reconstruction

Review of the two background chapters from a nonviolent value perspective creates awareness that the reconstruction of pre-decisional "givens" contained therein is biased in at least two ways: proviolent propensities are inadequately stressed, and nonviolent potentials are almost completely ignored.

To illustrate the first point, the chapters make no mention of the American decisions to drop atomic bombs on Hiroshima and Nagasaki (August 6 and 9, 1945) as part of the historical experience that may have preconditioned American decision makers in 1950 toward engaging in violence in Korea. This is especially important for understanding the aggressive aspects of Harry S. Truman's personality and of the presidential role. *The Korean Decision* cites President Truman's letter to his sister of August 12, 1945, to illustrate that "he was learning to live with difficult decisions." "Nearly every crisis seems to be the worst one," wrote Truman, "but after it's over, it isn't so bad. . . ."[6] However the narrative is silent upon the fact that this declaration of growing ability to make difficult decisions without tormenting afterthoughts came less than a week after decisions that had wiped out two urban communities with a horrendous immediate loss of 140,000 lives in Hiroshima and 70,000 in Nagasaki.[7] Japanese violence had legitimated American counterviolence, therefore our consciences were clear.

In accepting counterviolence as justifiable, *The Korean Decision* also underplays the contribution that victims of violence may have made to its initiation. Thus we are told of Truman's disgust with Russia's commitment to power politics (e.g., "Unless Russia is faced with an iron fist and strong language another war is in the making. Only one language do they understand–'how many divisions have you?' "–letter to Secretary of State Byrnes of January 5, 1946),[8] but we are not told of Russian perceptions of American power behavior in this era of American atomic monopoly. Applied to Korea, the background analysis does not ask if American politics from 1945 to 1950 might have contributed to a North Korean decision that only violence could assure the attainment of Communist political objectives there.

On the other hand, the background chapters are silent on the leaders, ideas, and experiences, both domestic and international, that tried to contribute to a nonviolent world in the 1945 to 1950 period. This is an artifact of proviolent values plus method: seeking to explain justifiable American counterviolence to North Korean aggression we tend not to seek evidence that nonviolent alternatives might have been even remotely possible. This means writing violent history that suppresses awareness of human potentials for nonviolent futures. Not all Koreans, for example, both leaders and other citizens, considered it inevitable or necessary that Kim Il Sung send armies south or that Sygnman Rhee invade the north in order to reassemble the tragically divided nation. Who were they? What ideas did they have? How were they suppressed? What can we learn from them for a nonviolent Korean future? Furthermore, what

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[6] Ibid., p. 24.

[7] These figures are taken from the report of an expert commission created by the mayors of Hiroshima and Nagasaki in 1976. The losses are estimated as of December 1945, with a margin of error of ±10,000 persons in each case. The Hiroshima figure includes an estimated 20,000 military deaths. By 1950, total deaths attributable to direct bomb exposure are estimated as "more than 200,000" in Hiroshima and "more than 100,000" in Nagasaki. Takeshi Araki, mayor of the City of Hiroshima, and Yoshitake Morotani, mayor of the City of Nagasaki, *Appeal to the Secretary General of the United Nations,* n.p., October 1976, p. 31.

[8] Paige, p. 54.

American domestic or international resources for nonviolent politics existed in the pre-1950 period? *The Korean Decision* is written as if the American Friends Service Committee and Mohandas K. Gandhi, among others, had never existed.[9]

In short, a nonviolent perspective in decisional background analysis should lead to enhanced awareness of both proviolent and nonviolent potentials in the decision makers and their environments.

Implications for Narrative Reconstruction

The principal methodological feature of the narrative chapters of *The Korean Decision,* aside from the effort to operationalize the variables of the decision-making approach, was the effort to "decontaminate" the description from the normative biases of the author. The intent was to treat normative issues independently of factual description. The narrative might be filled with normative judgments of the decision makers, but those of the reconstructing social scientist ought to be suppressed in that context. Review of these chapters from a nonviolent perspective, however, reveals several outcroppings of proviolent biases and the consequent need for further "decontamination."

For example, describing the Korean military situation just prior to the June 26 American decision to commit air and sea forces to combat, I wrote:

> At this time the Korean Government was withdrawing from Seoul to Suwon, 20 miles to the south across the Han River, as the invaders continued their unrelenting advance. The armored column spearheading their drive in the Uijongbu corridor was voraciously chewing its way through the two full South Korean divisions which hopefully had gone forth to bring it to a halt. *Along the invasion route to Seoul the blood of heroes and cowards together with the blood of those bewildered ones to whom circumstance did not provide a conscious choice between courage or cowardice stained the damp Korean earth the same bright red* [emphasis added].[10]

From a nonviolent position the author's gratuitous judgment of Koreans who killed as "courageous" and those who sought to escape killing as "cowards" is readily apparent. From such a position the judgment, if any were to be made here, could be exactly the opposite. Readers of *The Korean Decision* can further decontaminate the narrative simply by striking out the italicized sentence.

The cited passage contains yet another example of proviolent bias in its reference to the North Korean armored forces as "voraciously chewing" their way through the southern defenders. This imagery, implying in horror film fashion a mechanical beast devouring human victims, sets the stage for human heroes to vanquish inhuman foes. The same mood is conveyed by an earlier reference to "northern legions" that "swarmed" over southern hills.[11] Northern soldiers were neither the ghosts of long-dead Roman phalanxes nor insects; like their southern counterparts, they were mainly farm boys engaged in the task of killing. These passages thus can be decontaminated further by noting that the northern armored forces "murdered" their way through two defending divisions and that large numbers of North Korean soldiers advanced across the southern hills.

In the final paragraph of the narrative section, concluding the chapter on Friday, June 30, the proviolent bias of the author is made unmistakably clear. Referring to the efforts that would have been required to carry out the air, sea, and ground combat decisions that had been taken, the passage begins with the statement: "It would be no picnic."[12] Then the bias emerges clearly in the form of a dramatic quotation in which an actor is found to express the method-suppressed view of the author: "As Republican Representative Charles A. Eaton of New Jersey, an ordained Baptist minister, expressed it: 'We've got a rattlesnake by the tail and the sooner we pound its damn head in the better!' " Note again the employment of inhuman collective imagery—North Koreans are "*a* rattlesnake." Note also the implied religious justification for killing.

The passage concludes with two sentences that complete the effect of bias. First, "Most Americans wholeheartedly agreed." In support of this contention I footnoted a Roper Poll of responses to the statement that "President Truman did the right thing in sending our troops into Korea" which showed 73 percent agreement, 15 percent disagreement, and 12 percent with no opinion. This poll, of course, provides no evidence of the degree of commitment implied by the word "wholeheartedly." In view of the abrupt swing of public opinion against the war, contributing to Eisenhower's victory in the 1952 presidential election, the depth of support is questionable. For the purpose of further decontamination let us simply strike the word "wholeheartedly" from the text.

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[9] Only after experiencing value reversal did I begin to seek out and seriously study the excellent literature on nonviolent political alternatives; e.g., Richard B. Gregg, *The Power of Nonviolence* (London: George Routledge, 1938); Barthélemy de Ligt, *Conquest of Violence: An Essay on War and Revolution* (New York: Garland, 1972), reprint of 1937 edition; Joan V. Bondurant, *Conquest of Violence: The Gandhian Philosophy of Conflict* (Princeton: Princeton University Press, 1958); Staughton Lynd, ed., *Nonviolence in America* (Indianapolis: Bobbs-Merrill, 1966); T. K. Unnithan and Yogendra Singh, *Traditions of Nonviolence* (New Delhi: Arnold Heinemann India, 1973); and Gene Sharp, *The Politics of Nonviolent Action* (Boston: Porter Sargent, 1973).

[10] Paige, p. 157.

[11] Ibid., p. 82.

[12] Ibid., p. 270.

In the final sentence immediately after the assertion that "most Americans wholeheartedly agreed," I wrote: "Not the least of these were those who were committed and were slain on the distant peninsula jutting down between the Yellow Sea and the Sea of Japan." Although the dead are beyond polling, I would now hypothesize that a study of letters written by them to friends and relatives from the combat zone would reveal views more diverse than implied by my gratuitous invocation of their opinion. The value of nonviolence simply raises questions about exaggerated portrayals of human acceptance of violence. In sum, decontamination of the narrative would be better served if we struck out the whole last paragraph of chapter 10.

Implications for Empirical Re-Analysis

Reconsideration of the empirical proposition-building chapter of *The Korean Decision* from a nonviolent value position produces a disquieting sense that the original analysis is somehow truncated, stunted, and cut off from lucid engagement with the central problem of the Korean decision: why violence emerged and why it was responded to in kind. Instead the analysis is first diffusely devoted to the effects of "crisis" as an independent variable upon organizational, informational, and motivational aspects of decision-making processes.

The primary emphasis in the original analysis was to take "crisis" as an independent variable and to treat "organization," "information," "values," "internal setting," and "external setting" as dependent variables.[13] Secondarily, I combined all these variables in a set of propositional statements that described four decisional "stages" that characterized response to crisis in the Korean case.[14] Interestingly, the word "violence" did not appear in this analysis: words such as "positive response" and "costly commitment" were used instead.

Since a violent or nonviolent outcome was not the primary focus of attention, I finally concentrated overall explanatory analysis of the Korean decision upon its most outstanding processual characteristic: it was a "high consensus decision." Thus:

> *The stronger the organizational leadership, the less the variability in decisional unit membership, the more the shared learning of unit members with respect to the issue for decision, and the less tolerable the decision delay–the less the variability of information and values supplied from within the unit, the less the articulation of alternative courses of action, and the greater the probability of single courses of action that are anticipated to win leader approval.*[15]

Combining the initial interest in crisis effects upon decision-making variables with the secondary interest in a high consensus outcome, the overall logic of the original Korean decision analysis can be summarized as: crisis affects decision-making variables that produce high or low consensus outcomes.

If an explicit concern for violence is introduced into the analysis, however, we obtain the following pattern of analysis: crisis affects decision-making variables that produce *violent or nonviolent* outcomes. From a nonviolent perspective we are challenged to focus attention more sharply upon the substantive content of crisis decisions. The Korean case thus needs to be perceived not only as an example of a "high consensus decision," but also as a "violence-accepting decision."

A complex propositional statement to sum up the violence-accepting aspect of the Korean decision may now be added to the text[16] as follows:

> *The more the organizational influence of a violence-accepting leader, the more the decisional participation of members skilled in and accepting of violence, the more the past satisfaction with participation in violence, the greater the availability of instruments of violence, the greater the confidence in overall weapons superiority, the less the anticipated counterviolence, the greater the social acceptance of violence, the less the salience of*

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[13] Ibid., pp. 281–318.

[14] Ibid., pp. 318–21.

[15] Ibid., p. 321; emphasis in original.

[16] Insert after propositions (i.e., after line 22) in ibid., p. 321.

nonviolent alternatives, and the greater the belief that competing decision makers are motivated by a similar logic—the greater the probability of violent decisional responses to crisis in international politics.

At the end of the original chapter devoted to empirical analysis of the Korean decision, I briefly introduced three propositions intended to "link properties of decisions with problems of their execution by large-scale governmental organizations."[17] These predicted a gap between intent and performance if the decision content is ambiguous; a link between the seriousness of expected counteraction and the degree of decisional specificity; and a tendency to delegate command and control functions to field commanders where severe counteraction is not anticipated. Again, the word "violence" did not appear in any of the statements.

Approaching the same problem from a nonviolent perspective, it appears that decisions based upon the assumption of justified violence are apt to be ambiguous; that the acceptance of violence tends to preclude attention to complementary and possibly supplantive nonviolent coping alternatives; and that violence-based decisions are likely to be permissive of initial commander autonomy in a violent direction.

While a proviolent value bias in empirical analysis seems not to have repressed evidence of nonviolent alternatives considered by the decision makers, since they all seemed satisfied with violence, this does not mean that it had no analytical effect. For example, no effort was made (a) to develop nonviolent alternatives with which the decisions could be compared, (b) to question the degree to which each decision maker was committed to violence, and (c) to probe through interviews the existence of latent nonviolent alternatives or to obtain a more detailed understanding of why such alternatives were considered infeasible.

Implications for Normative Re-Analysis

The Korean Decision contains a chapter devoted to normative evaluation of the decision. Four different approaches were taken. First, a normative inventory of the case materials reviewed the judgments of the decision makers, domestic critics, external allies, and external critics. Second, some common criteria of international political evaluation were explored. The Korean example was found to fall in the category of good decisions; i.e., decisions that pursued good ends by just means in a flexible, realistic way with beneficial long-range effects. Third, the conditional approbation of violence by major world religions was recalled and a pacifist perspective was entertained and dismissed. Finally I presented my own judgment.

It is the latter which I wish to revise here. I now believe that the American decision to fight in Korea is not a decision worthy of moral justification by a social scientist, any more than that which produced the North Korean attack, that the American decision vastly increased the loss of life in Korea including later many Chinese, that confirmation of the decision by congressional resolution which I originally recommended would not have made it more just even if politically more tenable, and that the long-range effects of the Korean decision have not been beneficial for Korea as a whole or for international political life. The Korean decision did not realistically make international political violence less likely, as illustrated by the case of Vietnam, to which official American satisfaction with the Korean decision undoubtedly contributed. The international militarization to which the Korean War contributed did not make world peace more secure; witness continuing arms races, increased anxiety over American military security, and nuclear weapons proliferation.

The United States' decision to engage in violence in Korea, not "resist aggression" as in the title of a 1958 article jointly written by Richard C. Snyder and myself,[18] has contributed to the unprecedented militarization of both parts of Korea. In 1950, there were 286,091 men under arms in Korea (151,091, south; 135,000, north),[19] out of a total population of about 29,715,000 (20,167,000, south; 9,548,000, north)—or one soldier for every 104 persons. By 1975 this had risen to 1,092,000 men under arms (625,000, south; 467,000, north)[20] out of a total of 50,350,000—or one soldier for every 46 persons. This increase was combined with vastly more destructive weapons, and with the high likelihood that both contending Korean governments will achieve independent nuclear weapons capabilities in the near future.

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[17] Ibid., pp. 321–23.

[18] The "resist aggression" characterization, rather than "to intervene," was strongly advocated by Secretary of State Dean G. Acheson and accepted by me in Richard C. Snyder and Glenn D. Paige, "The United States Decision to Resist Aggression in Korea: The Application of an Analytical Scheme," *Administrative Science Quarterly*, 3 (December 1958), 341–78. One problem with the "resist aggression" formula is that it implies total evil of the aggressor and that only military measures offer hope of successful resistance.

[19] Roy E. Appleman, *South to the Naktong, North to the Yalu (June-November 1950)* (Washington, D.C.: Office of the Chief of Military History, Department of the Army, 1970), pp. 10–11.

The threat of violence in Korea, based upon the ultimate acceptance of the possibility of a violent "solution" by both sides and their international supporters, has legitimized the suppression of political freedom in both parts of Korea, a value that the original "realistic" commitment to violence was intended to protect and enhance.

Thus my own judgment is that the Korean decision does not merit praise as a contribution to world peace and freedom. It should rather be judged as a stimulus to search for nonviolent alternatives to resolve human conflicts and to realize human aspirations then, now, and in the future.

Reassessment of Action Implications

In the final chapter of *The Korean Decision*, I tried to derive some lessons from the Korean case to guide future American policy makers in crisis situations. All based upon acceptance of violence, these suggestions were: not to underestimate potential enemy military strength; to be receptive to friendly critics so that "force" might be employed less dangerously and with more political support; and to set clear limits on the employment of force so that it might be employed with surgical precision.

From a nonviolent perspective, the best "lesson" to be learned from the Korean decision is that American policy makers should be encouraged to experiment with the assumption that American violence will not be applied in international politics, that American military supplies will not be provided to support the violence of others, and that policy makers should work positively toward nonviolent resolution of the grave domestic and international conflicts that threaten human dignity, economic decency, physical survival, and world peace.

A multinational nonviolent approach to pre-1950 conditions in Korea and to coping with violence if it erupted there would by no means imply that only military measures would be appropriate or effective, either in the short or long run. An extraordinarily versatile combination of political, economic, social, cultural, and communications means might be employed to prevent, resist, limit, and defuse armed aggression including physical resistance to the point of death with intent not to kill but to touch the hearts of the aggressors. A nonviolent policy approach to the Korean decision and its preconditions does not therefore imply passive acceptance of violence but rather more creatively vigorous efforts to end and avoid lethal conflicts than a violence-accepting approach would require.

Analytically we need to add to the repertoire of skills in decision-making analysis the caution that the more the agreement of the scientist with the values of the decision makers, the more limited the likely development of evidence and analysis that would support alternative courses of action. A collegial check upon such biases would be constant encouragement of value diversity among scientists. An individual check would be to prepare comparative actor-analyst value profiles and to seek deliberately to extend the range of congruence-predicted analysis of alternatives.

In conclusion, *The Korean Decision* needs to be re-examined not as a text on how to handle violence better but rather as a challenge to how to avoid it in the first place. If violence does occur, then the best crisis advice is to limit it, compartmentalize it, diminish it, weaken it, calm it, cool it, find alternatives to it, seek rewards to end it[21]—not to increase it, fuel it, supply it, justify it, praise it.

In an age of unprecedented potential for violence, the supreme task of political science becomes the creation and application of nonviolent knowledge. It will benefit us little if our continued "realistic" acceptance and justification of political violence prevents us from creating alternatives to it. *The Korean Decision* thus needs to be reanalyzed as a contribution to this task and not allowed to stand as a scientific apology for the future continuation and possibly irreversible escalation of violence in international political life.

The original dedication of *The Korean Decision* was "To all who died in the Korean War, and to all who make and study political decisions." To this should now be added, "for a nonviolent future."

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[20] International Institute for Strategic Studies, *The Military Balance, 1975–1976* (London: The International Institute for Strategic Studies, 1975), p. 56.

[21] The remarkable experiment by Tsai, in which after 700 trials he got a cat and a rat to cooperate in obtaining food without a coercive security barrier between them, can serve as a stimulus to constructive thought along these lines. See Loh Seng Tsai, "Peace and Cooperation Among Natural Enemies: Educating a Rat-killing Cat to Cooperate with a Hooded Rat," *Acta Psychologia Taiwanica*, 3 (March 1963), 1–5.

BOOK REVIEWS

Political Theory and Methodology

Comparative Social Research: Methodological Problems and Strategies. Edited by Michael Armer and Allen D. Grimshaw. (New York: John Wiley, 1973. Pp. 473. $17.95.)

The book is a collection of papers from an assemblage of social psychologists, methodologists, and other sociology subfield specialists who were invited to think about the methodological implications of their subject. The editors included the salient points recorded from discussions following the paper presentations; though some of the comments yielded further insights, most of the oral comments would have been better left unpreserved. Furthermore, the standard complaint about large edited collections–uneven quality and lack of coherence–is appropriate. However the collection does represent a significant effort on the part of sociologists to address the issues which confront comparative (cross-national) politics as well as social research specialists.

An atmosphere of *déjà vu* about the probability (possibility?) of solutions to the difficulties all comparative research specialists face, from concept equivalence to measurement and data analysis strategies, pervades the book–"All of sociology is a tough business. . . . Let those who cannot tolerate the uncertainties, the ambiguities, and the high probability of failure pursue one of the simpler sciences" (Hill, p. 464). The volume is an example of the burgeoning interest in comparative research and the need to grapple with the research-related issues. Why then the *déjà vu?* Before answering the question, I shall discuss the papers.

The papers may be grouped according to subject emphasis and methodological concern. Topics range from community power, illegitimacy, childrens' acquisition of language, the impact of law on social change to individual level studies of modernity, class consciousness, and national development. Methodological problems addressed include what is distinctive about comparative sociology and the comparative method, the sociology of cross-cultural research (field work interviewing, research acceptance by citizens of the country being studied, the assumptions brought to comparative research by the researcher and the researched), concept formation and measurement issues, unit and level of analysis questions associated with ecological analysis.

If one views the volume as an effort to summarize the lessons a group of able comparative research specialists have learned from experience, it is a worthwhile purchase. Moreover, several papers are worthwhile contributions taken alone. Turk's discussion of the use of historical legal materials for the study of social change is a welcome addition. Armer and Schnaiberg critically evaluate the Inkeles individual modernity scale through a well-designed replication procedure. And Schwirian and Finsterbusch present a useful discussion of comparative ecological analysis strategies. However, one is no clearer about what is distinctively comparative sociology or what comprises the comparative sociological method after than before reading the book. We may address the feeling of pessimism which pervades the collection by drawing two distinctions.

The distinctions are between theory and method in social inquiry and strategy versus method in comparative (cross-national) research. The distinction between method and theory must be insisted upon because much of the confusion surrounding which method, technique, or data base–in combination with which unit and level of analysis) would fade if we had a theory of social change. By theory I mean models or systems of various degrees of completeness, parsimony, capable of generating specific-point predictions about variable states in the sociopolitical process of change. The absence of such a theory in social inquiry leads to the apparently endless debate over method which I take to encompass all steps in the logic of inquiry (the traditional set of research design steps or reconstructed logic). This is especially so in comparative research because of dilemmas suggested by the justifications usually offered for comparative research. Hill states that comparativists desire to examine a problem in a number of societies "to evaluate the importance of cultural or societal heterogeneity as an explanatory variable. . ." because they are either searching for universal laws or checking out the limits of hypotheses developed largely in the American research context. However, underlying this statement is a problem which highlights the issues comparativists face. To assume cultural or societal heterogeneity is to assume a continuum or a process (presumably developmental) within which particular societies, or parts thereof, may be placed. Thus, unfortunately, in the absence of a theory of social change it is difficult to do comparative research because we have no way of knowing whether we have standardized comparative cases (Walton, pp. 173–188) or equivalent units in comparative research, i.e., we do not know where to place the units to be compared on the process or societal heterogeneity continuum. As it is we generate cross-sectionally based correlations in comparative ecological research (Finsterbusch, pp. 417–46) which lead nowhere because, in the absence of hypothetico-deductive theory, such work can only assume the stability among and between the independent and dependent variable systems under study, cross time. It is rare that this assumption is warranted. Cross time analysis (Johnson and Cutright, pp. 377–408) does not solve the issue either because in the absence of parameter estimates provided by a theory, the investigator is still unable to make point predictive estimates about the level and direction of R's cross time; moreover, there are no guides for predicting the amount of time needed for changes in relationships to occur.

The above basic epistemological point leads to the distinction between strategy and method in comparative research. The reason comparative social inquiry "exhibits very little by way of distinctive and rigorous methods" (Walton, p. 174) is because the claim that there is a distinctively comparative (cross-national) method is unwarranted. This collection of papers, like others of its type, comprises a set of more or less useful strategy suggestions about the *way* specific methods might be combined to attack the questions comparativists study. The reasons strategy discussion becomes confusing and contentious (as witnessed in the preserved oral remarks in this book) is because it falls in the "context of discovery" column where tacit level assumptions, images which condition what investigators think are appropriate questions, are dominant.

However, I do not wish to leave the impression that the argument for comparative research is not sound. Several of the authors suggest the limitations of system-specific (within system) studies. For, by inference, generalizations from these (largely American) reports must be regarded at least as cautiously as comparative research-based studies because of the Hill quotation and the point about our inability to place nation-states or sub-units thereof on the societal heterogeneity continuum or process of change. Unless we are satisfied that no further social change will occur, we are not entitled to rest comfortably with system-specific findings—if further change occurs these typically unstable variable systems may change significantly, even reverse direction.

Read with the distinctions made here, the confusion exhibited in the papers over what unit of analysis is appropriate in comparative research, how explanations may be evaluated and compared, what, in fact, is meant by comparative methodology, is not only understandable but probably inevitable. However I know of no single volume in comparative politics which addresses the range of issues discussed in this collection and thus reiterate that it is a useful substitute volume which examines virtually all the problems that anyone dedicated to comparative research faces.

ROGER W. BENJAMIN

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Capitalism and the Permissive Society. By Samuel Brittan. (Atlantic Highlands, N.J.: Humanities Press, 1973. Pp. 397. $12.00.)

It is now obvious, both to the outside world and the British intellectual elite itself, that British society is close to the ultimate throes of social collapse. National adversity, however, has produced a few concerned social thinkers who have begun to re-think the conventional wisdom of the British way of life from fundamentals. One such is Samuel Brittan. Mr. Brittan, not surprisingly in terms of British society, though it would be so in the United States, is not an academic political scientist or economist, but a financial journalist. As a social philosopher, however, he has the advantage of a first-class Cambridge economics degree, during which he was taught by both Joan Robinson and Milton Friedman, and a scholarly bent which has made him deeply versed in the philosophy of Hayek and the "Vienna School" of liberal philosophers.

The present book is for the most part a record of work in progress and minor variations on a major theme, in the form of short pieces on British politics and political personalities. These pieces are illuminating and penetrating; but they stop short of tackling head-on the fundamental problem in political science and sociology that the British case increasingly raises—namely, why political debate in Britain is couched in language and imagery that consistently avoid the rational treatment of policy problems (especially Britain's international relations). There comes a time when the appeal to rationality must be recognized to be pointless by those who pin their faith on belief in the ultimate rationality of the democratic process. Mr. Brittan still has faith—naturally enough, for anyone born in and committed to any country of historical tradition and pride—but one occasionally senses a hint that the author is uneasily aware that no one is willing to listen.

This has, indeed, been the reaction in Britain to the main essay, "A Restatement of Economic Liberalism," which accounts for the first 150 pages of the book. This is a very carefully worked out and balanced essay, notable for its willingness to suffer fools patiently (if not gladly) and its philosophical sweep, as good as can be found in the way of a relevant modern restatement and extension of classical liberal ideas. If anything, Brittan concedes too much to the wilder superficial critics of capitalism for American tastes; but one has to remember that the universities of privilege in the United Kingdom stifle genuine intellectual enquiry and debate to an extent impossible in the political and cultural polycentricity of the United States. Nevertheless, this has not saved Brittan from savage reviewer attacks based on refusal to follow his arguments, and depiction of him as a mere beautician lackey of "the ugly face of capitalism."

There is a growing body of observers who fear that Britain today is a picture of what the United States is likely to become tomorrow. Few, however, if any, have extended their gloomy prediction from the economics into the politics of the welfare state. Brittan's work suggests a number of themes worth exploring in the context of contemporary American democracy.

HARRY G. JOHNSON

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Coalition Theories: A Logical and Empirical Critique. By Eric C. Browne. (Sage Professional Papers in Comparative Politics, Vol. 4. Sage Publications: Beverly Hills, Calif.: 1973. Pp. 95. $3.00, paper.)

Coalition for what? What is the payoff? These are the questions that continue to plague decision theorists. This short, tightly written monograph examines the assumptions and logic of coalition models by William H. Riker, William A. Gamson, and Michael A. Leiserson and finds important weaknesses.

In order to compare the models, Browne tested them against coalitions actually formed in 13 parliamentary democracies over a quarter of a century. The results, although quite a lot better than chance, do not give "us a very good chance of predicting an actual formation" (p. 26). The Gamson and Riker models were especially weak. Analysis by individual nations offered no encouragement for the predictive value of coalition theory.

Why do none of the models allow us to predict? Browne concludes, using some empirical evidence and a tightly reasoned argument, that the model builders depart from incorrect assumptions. In particular, he believes, "the value of belonging to a particular winning coalition is not related in any necessary way to its size alone" (p. 72).

Riker's famous "size principle" is irrelevant in most cases. Why? Because "in most political coalition games there is no payoff waiting to be divided" (p. 73). Browne therefore seeks to answer the question of why, then, coalitions are formed at all. Relying upon some recently published theoretical work (all of it in the 1970s) especially that of the Netherlander, Anthony De Swaan, he argues that players seek to maximize their coalitional benefits by minimizing the policy or ideological differences among partners in the winning coalition. If the coalition is not to be minimal, there is a problem of determining how large it should be, for the larger the coalition the greater are what Charles Press and I have called "dissonance" costs. (*American Political Science Review*, 62 [June 1968], 556–63). Browne offers suggestions for calculating optimal coalition size and membership, but the problems are formidable. In effect the goal is one of finding the winning coalition having the smallest dissonance costs.

Even if the Browne-DeSwann approach can be refined to the extent of providing satisfying predictability, it is still limited in concept to cabinet formations in parliamentary democracies. The ad hoc coalitions that occur in the United States Congress, for example relative to "revenue sharing" (positive) or foreign aid (blocking), could not be predicted by their method, in part precisely because while policy distances are small, ideological distances are great—members vote the same direction for different reasons. Alas, no general coalition theory is yet in sight.

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Ferdinand Toennies: A New Evaluation. Essays and Documents. Edited and with an introduction by Werner J. Cahnman. (Leiden: E. J. Brill, 1973. Pp. viii + 302. 64 guilders.)

This recent volume aims at a new evaluation of the sociology of Ferdinand Toennies through the combination of secondary analytical essays with newly translated excerpts from that author's work. The secondary evaluative essays include the following: (1) two older pieces (Albert Salomon's essay in memoriam and Talcott Parsons' note on the concepts of *Gemeinschaft* and *Gessellschaft*); (2) a series of new essays on various facets of Toennies' work (Albert and Gillian Gollin on the study of public opinion in Toennies, A. Oberschall on his empirical sociology, recent afterthoughts by Parsons on *Gemeinschaft* and *Gesellschaft*, p. Etzkorn on Toennies as a formal sociologist, and three wide ranging systematic treatments of Toennies' sociological system by E. G. Jacoby, R. Heberle, and W. Cahnman); and (3) an introductory statement by the editor locating the various contributions to the volume. Here, the essays by Cahnman on "Toennies and Social Change" and Jacoby on "Three Aspects of the Sociology of Toennies" are especially acute and discerning additions to our understanding of that German pioneer's wider contemporary relevance.

The newly translated material is accompanied by helpful commentary and notes by the editor and focuses on Toennies' relation to the work of Spencer, Marx, Durkheim, and Max Weber. An added bonus for the reader is the inclusion of a new translation of Emile Durkheim's own 1889 review of *Gemeinschaft und Gesellschaft*, along with Toennies' comments on that review and his own reviews of Durkheim's *Division of Labor* and *Rules of Sociological Method*.

Undoubtedly the most rewarding part of this book is the section of new translations. To read Toennies' modifications and criticisms of other pioneering figures in the sociological tradition is to understand more readily key differences between these masters as they saw matters themselves. The sections on Toennies' relations to Marx, Durkheim, and Weber are immensely helpful in this respect. Indeed, Toennies' criticisms and extensions of Marx, carrying many of Marx's assumptions and perspectives into the light of more systematic sociological conceptualization and historical empirical evidence, can be read with profit even today for their clarity, scope, and depth and will doubtless be preferred by many sociologists and social scientists in general to some more recent yet also more elusive treatments of that author. The wide horizons and conceptual penetration of Toennies' remarks will appear even more unusual when it is remembered that they appeared as early as 1908 (and later in more extensive form in 1921), well before more recent interpretations based on a fuller recovery of Marx's unpublished manuscripts. One is allowed to wonder whether the integration of Marx's theoretical and empirical contributions into the mainstreams of sociology might not have proceeded more smoothly, especially in America, had earlier sociologists been aware of Toennies' neglected contributions in this area.

More than a few perplexing questions arise from the comparison of Toennies with Max Weber, but this volume at least begins to provide some indispensable beginning materials for their resolution. Only three examples can be cited here. Toennies' remarks in this volume on Weber's Protestant Ethic thesis highlight the distinctive stresses of these two writers in their approaches to historical sociological matters. Indeed, Weber's more comparative historical differential approach to the use of ideal typical conceptions of the varieties of capitalism contrasts with Toennies' own stress on the "common characteristics of *all* kinds of capitalism" (p. 265; his italics). Also, the attempt by Toennies to see the rise of modern capitalism primarily in the light of his notions of *Gesellschaft* and rational will contrasts with Weber's need to distinguish more carefully among the various historical roots and typical constellations of the notions and processes of rationalism, rationality, and rationalization and to see his own study of the impact of ascetic rational Protestantism as one part of a wider understanding of the series of phases in the overall process of rationalization within the Western European civilizational setting. Finally, Toennies' recourse to the concept of reciprocity (p. 268) to help solve the perplexing problems of the relative degrees of causal efficacy of "spiritual" and "real" factors is frequently combined with an ultimate appeal to "economic factors" (social class, occupational activity, productive technology) as the key causal agencies. This volume sheds a good deal of light on these (and a number of other) differences between Weber and Toennies as well as between Toennies and other classical writers considered in these pages.

Despite a certain degree of thematic repetition, especially in the secondary essays on Toennies, this collection is a helpful addition to the growing literature on a sociological pathfinder whose importance is bound to increase as sociologists today try to map the wider dimensions of a more contemporaneously relevant systematic sociology of a larger historical range. As students of the history of social theory move more rapidly toward a fuller reappropriation of the widest perspectives and strands of analysis available in our "collective memory," volumes of this sort will hopefully appear more rather than less frequently.

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From Rousseau to Lenin: Studies in Ideology and Society. By Lucio Colletti. Translated by John Merrington and Judith White. (New York: Monthly Review Press, 1972. Pp. 240. $8.50.)

Some potential readers of this volume could be misled, as I was, by the title; The title suggested to me a coherent, systematic study of a series of social theorists, beginning with Rousseau and ending with Lenin. In fact, however, the book consists simply of a number of essays by a leading Italian Marxist philosopher, heretofore unavailable in English translation, and these essays vary widely in theme, style, method, length, and quality.

The low point in the book is reached in the first essay, "Marxism as a Sociology," which is an example of Marxology—perhaps "Marxophilia" is a better word—at its worst. We find here all the marks of solemn Marxist exegesis carried to an extreme. The seminal texts, especially *Capital,* are quoted with reverence. The prose is dense and obscure. Innumerable words are italicized for no apparent reason. The author leaps from one line of analysis—"Darwin and Hegel"—to another—"Weber and Aspects of Contemporary Bourgeois Sociology"—without establishing a clear connection between both themes.

If one reads only this initial essay, one might get the impression that Colletti is simply a hack. One might get the same impression from the final two essays in the book, "Lenin's *State and Revolution"* and "Marxism: Science or Revolution?" Both of these brief essays are long on assertion and short on argument.

But Colletti is not a hack. There is plenty of meat between the slices of soggy bread. The essay on "Bernstein and the Marxism of the Second International," the longest in the book, is impressive as a concise account of the pre-World War I controversy over revisionism, illuminating in its analysis of the underlying issues of that controversy, and provocative in its bearing on current disputes among Marxian socialists. Not surprisingly, this essay has a good deal to say about the heresies of Eduard Bernstein. But it is by no means an anti-Bernstein polemic. Indeed, Colletti maintains that Bernstein was in some ways more perceptive than Engels and the "orthodox" Marxists, especially Kautsky. But the interest of the essay does not lie primarily in its judiciousness but in its boldness. For one thing, Colletti maintains that Bernstein and Kautsky *shared* a (mistaken) view that Marx thought it possible to distinguish between (nonideational) economic phenomena and all other phenomena. For another, he argues here, as in other essays in the volume, that "dialectical materialism" is both unfaithful to Marx and intellectually indefensible. And for another, he offers a thoughtful and innovative interpretation of Marx's theory of value, which focuses especially on the notion of "abstract labour."

Two lengthy essays, "Rousseau as a Critic of Civil Society" and "Mandeville, Rousseau, and Smith," focus on the political theory of Rousseau. As Colletti himself acknowledges, he fails to integrate fully the twofold task of constructing a critical bibliography of recent writings on Rousseau and critically examining Rousseau's political theory. Accordingly, a number of his observations about Rousseau's political philosophy are hardly groundbreaking (e.g., that Rousseau did not conceive of the state of nature as an actual historical condition and that he did not advocate a retreat from civilization to savagery). Nevertheless, these essays are exceptionally interesting, at least from the standpoint of intellectual history. For example, Colletti provides some striking evidence and argument—and also a fascinating commentary—concerning Rousseau's influence on Adam Smith. And he advances the bold thesis that "revolutionary 'political' theory, as it has developed since Rousseau, is already foreshadowed and contained in *The Social Contract"* (p. 185). Unfortunately, he does not go on to say whether or not, or why, a satisfactory revolutionary political theory should, or could, advance beyond Rousseau.

By far the most provocative essay in this collection is the one entitled "From Hegel to Marcuse." The thesis of the piece—that Marcuse's writings are not those of a Marxist but of a Young Hegelian—is not new. What is new is the care with which the thesis is developed and the persuasive argument that the originator of "dialectical materialism" was Hegel himself. I find Colletti's critique of Marcuse compelling, as far as it goes. Its shortcoming is that it fails to address—unlike Marcuse, however inadequately—the question of whether or not the industrial working class is a plausible agent of revolutionary social change.

This book is so uneven that I find it impossible to give a general assessment. At its best it is very, very good, and at its worst it is horrid.

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Self-Evident Truths: Being a Discourse on the Origins & Development of the First Principles of American Government—Popular Sovereignth, Natural Rights, and Balance & Separation of Powers. By Paul Conkin. (Bloomington and London: Indiana University Press, 1974. Pp. xi + 196. $7.95, cloth; Midland Paperback, $3.25.)

This is an ambitious book, both in terms of the complexity of the ideas and the range of the writers examined. It is divided into three parts, corresponding to the source, intended purpose, and form of American government. Parts one and two present a separate but parallel treatment of popular sovereignty and natural rights: their origins, their application in the colonial debate with England, and their development in American constitutions. Part three contains a chapter on the relationship between mixed government and separation of powers and another on America's checks and balances, called balanced separation. While these political ideas have a "beguiling personal appeal" to the author, he cannot embrace them fully. "It is difficult for me to believe that any normative theory is universal, that all right-minded people have to agree with it, that it fits all contexts and all societies" (p. x). It might be difficult for others to believe that so democratic and inflexible a test is appropriate for assessing political thought. The ambitious task and the author's posture toward thought may have affected the accuracy and thoroughness of his exposition, especially regarding religion and property.

Conkin draws on two jurists, Johannes Althusius and Enrich de Vattel, for the origins of popular sovereignty. Althusius wrote about a religious polity whose object was a "holy" as well as a "just, comfortable, and happy" life (p. 83), but Conkin is more interested in his distinction between the residual sovereignty of the people and government. The fully developed concept of popular sovereignty, found in Vattel, includes a sovereign people, formed by consent, establishing a form of government, and retaining the right to change the fundamental law (pp. 25–26). Conkin later argues that popular sovereignty requires universal manhood suffrage for constitutional decisions but not for regular elections (p. 60).

Indicative of his legal approach to the subject, Conkin suggests that Althusius' distinction between sovereignty and government, which permits coordinate governments, could have resolved the colonists' conflict with England and would have prevented "a hundred years of foolish drivel about "divided sovereignty' " in America (pp. 38–39). Then, in his discussion of the Federal Constitution's new federalism, Conkin prefers Calhoun's generation-after states rights' explanation to Madison's contemporary nationalism. The question is whether the people of the several states or the people of the United States are the source of constitutional authority. Conkin presents the nationalist interpretation, which he associates with Wilson and Hamilton, but, strangely, he thinks that "moderates such as Madison or Randolph" would have agreed with Calhoun's formulation and "might even have accepted . . . secession" (p. 63). Madison authored the Virginia Plan and argued strenuously for a national negative of the states, popular ratification, and proportional representation in both houses. The cautious Randolph refrained from signing the Constitution but later helped Madison secure Virginia's ratification. The Federal Convention is not Conkin's strong point; three prominent framers, Edmund Randolph, Roger Sherman, and William Paterson, are misnamed John Randolph, John Sherman, and James Patterson (sic) (pp. 63, 66, 131, 151, 178, 209, 210).

Conkin traces the evolution of natural rights from Calvin and the French Huguenots to Locke, who "fitted traditional Christian views of responsible ownership to new economic realities" (p. 95). Christian natural law, based on divine right, apparently evolves into modern natural right. The difficulty with this interpretation is revealed by a consideration of Locke's teaching on property. Locke's natural law conforms to rational self-interest. The only restriction on the accumulation of property in the state of nature is perishability. The invention of money eliminates this restriction by providing for the accumulation of durable wealth. Locke unqualifiedly supports this invention, because it encourages cultivation, and "the increase of lands and the right of employing of them is the great art of government" (*Second Treatise*, chapter v, section 42). For Conkin, Locke's teaching contains an objective standard for individual need, a disinterested concern for others, and does not "confer the sanction of nature ... for any monetary wealth" (pp. 96–97). Conkin argues that the only form of property related to natural rights includes what is "needed for sustenance and used productively," as well as "the resultant products of one's labor" (p. 112). This excludes "speculative holdings" and "vehicles of commerce" as well as slavery (p. 112). Having attributed Christian restrictions to Locke's teaching on property, Conkin now celebrates agrarianism for expressing "in a peculiarly secular setting many of the ethical themes of Christianity" (p. 114).

In part three Conkin notes the difficulty of classifying governmental activities by function and that Montesquieu intended a separation and a balance to maintain political liberty. Conkin claims that complete separation "cannot cohere easily with legislative sovereignty"; but, unless a single executive shares the legislative power and possesses the appointing power, legislative supremacy results. Later Conkin notes that the earlier state constitutions achieved a functional separation of powers with weak executives (p. 173). Conkin's "balanced separation" reflects a victory for the strong executive advocates.

In his epilogue Conkin holds Hamilton responsible for the nonpreferred position of property rights today. Americans voted "against a propertied society, eventually paying the price of almost universal economic servility for the glory of national greatness and the solace of abundance" (p. 191). If the right of property comprehends the exercise of one's faculties in the unlimited accumulation of wealth, then we cannot separate our security and prosperity from that right. Almost against his intention, Conkin's book illustrates the difference between a religious polity and a polity limited to securing individual rights. For that reason, and for Conkin's full account of writers from the Protestant Reformation to the Puritans, students of political thought will wish to consult this book.

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The Political Philosophy of Luis de Molina, S.J. (1535–1600). By Frank Bartholomew Costello, S.J. (Spokane: Gonzaga University Press, 1974. Pp. xxviii + 242. $15.00.)

The Jesuit writer, Luis de Molina, is better known as a theologian than as a political theorist. His position on the relation of free will and divine grace ("Molinism") involved him and his order in a conflict with the Dominicans whose great theologian, Domingo Bañez, had given greater prominence to the infallible action of God's grace than did Molina who wished to emphasize the freedom of the human will and the individual decision to cooperate with God's grace. The controversy lasted until 1608 when it was officially terminated–but not resolved–by papal action.

Molina also wrote a five-volume treatise, *De Justitia,* which developed out of his lectures on Aquinas' *Summa* and what he considered to be the inadequacies of its treatment of important moral questions. Father Costello uses the treatise as his primary source, comparing Molina's political theory with that of his contemporaries, analyzing the content of his thought, and–not altogether successfully–attempting to link his political views to his theological position.

Costello gives particular attention to Molina's view of the role of the community in establishing and limiting political authority. Molina shares with contemporary Spanish Jesuit writers the theory that the community transfers political authority to the ruler and can limit it, but unlike the better-known Jesuit, Francisco Suarez, he denies that the community can exercise political power in itself. Costello notes that in contrast to the position adopted a century later by John Locke, Molina never concedes that the individual has the right of execution of the law of nature, which, for Molina, can only be carried out by those acting on behalf of the political community. Here there would have been an opportunity to analyze the relationship of Molina's emphasis on free will and his denial of individual decision making on natural law, but Costello never develops this point. Nor does he draw the obvious parallels with the thought of Molina's English contemporary, Richard Hooker.

The study examines Molina's theories on church and state–and devotes considerable attention to the distinction between the "direct" and "indirect" theories of the temporal power of the papacy. John of Paris, whom Molina read, provides a useful point of comparison and Molina is described as adhering to a less naturalistic view of the state than does John, even granting the church an emergency power to intervene coercively in temporals for spiritual reasons. Costello describes Molina's position as "vacillating" but that is probably not the correct word for a theory which, despite its rejection of the extreme papalists' theory of the direct power of the papacy in temporals, grants unlimited coercive power to the papacy (and even to bishops) if the good of the church requires it.

The attention which the just war theory has received in recent years makes Molina's analysis of this theme of considerable contemporary interest. It turns out to be a fairly straightforward reworking of the classic Thomistic conditions with some additions to take account of Spanish and Portuguese imperialism. Molina justifies a war which is fought to prevent pagans from impeding the spread of the gospel, a reason that could be and was used for the Spanish conquest. He rejects the conquest of the infidels simply because of their idolatry, barbarity, or vice but allows for enslavement (even of women and noncombatants) as a result of defeat or capture in a just war, and explores the extent to which this justifies or limits the Portuguese slave trade.

In general, Molina emerges from this study as a rather conventional synthesizer of the scholastic moral tradition. He is less original than Vitoria on whom he depends for much of his thinking, and less relevant and influential than Suarez and Bellarmine who followed him. For an introduction to the thought of the Spaniards, Bernice Hamilton's *Political Thought in Sixteenth Century Spain* (Oxford: Clarendon Press, 1963) is both more comprehensive and more readable than this rather specialized and pedestrian study of a thinker whose real contribution was to theology rather than to political theory.

PAUL E. SIGMUND

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Locational Approaches to Power and Conflict. Edited by Kevin R. Cox, David R. Reynolds, and Stein Rokkan. (New York: Sage Publications, Distributed by Halsted Press, 1974. Pp. 345. $17.50.)

Most political scientists by now realize that they should keep themselves aware of what is going on in other social sciences—aware of new developments in theory, in methods, and in data resources. Attention has been focused by substantial numbers of major scholars on work in sociology and psychology, as reflected in branches of our discipline now devoted to political sociology and political psychology, and on many aspects of economics, as in the field now devoted to problems of collective choice as well as the new political economy. Even if political scientists are unable to keep up with all these new developments, presumably they recognize the importance and potential contribution of these developments to their own direct concerns.

Relatively little attention, however, has been paid to work in geography. Perhaps this is because we still think of geography as it probably was taught to us in school: heavily concerned with physical characteristics of climate and terrain, making extensive use of maps and, where concerned with societal phenomena, overwhelmingly descriptive rather than theoretical or systematically explanatory. In other words, it does not seem to be a social science.

'Tis pity if 'tis true, because in fact the image is no more correct than an image (still not uncommonly held, however) that the study of politics is primarily descriptive and atheoretic. Not only can geographers learn from us, but there is a lot we can learn from their now very analytical and mathematical discipline, and we can discover a lot through joint enterprises. A number of efforts in recent years, epitomized by several readers in political geography, have attempted to let scientifically oriented geographers know some of the potential contributions lying in wait among political science works. In this volume the editors—two geographers and a sociologist-political scientist—do a little of that, but, more importantly, they try to promote a reverse flow from geography into political science. This is a book that should prove useful to people working in a variety of subfields in our discipline, such as electoral behavior, public opinion, comparative politics, international relations, and of course methodology. As Editor Rokkan notes at the outset, territory, space, and location "have been key concerns in the study of politics since Aristotle" (p. 9)—but they have rarely been addressed with the range of scientific tools on display here.

The papers by Cox and by Cox and Reynolds will hold special interest because of their application to problems of spatial organization of concerns from the public choice literature: externalities and jointness and distribution efficiencies. Although several chapters present useful literature reviews, I found it disappointing that, of 11 chapters, only four represented systematic empirical studies. Of these, to me the most stimulating was Reynolds' application of models of spatial contagion to patterns of election returns. I strongly suspect that various contagion models will prove enlightening for the study of such other phenomena as opinion change and, in comparative and international politics, sequential changes of governments across borders ("coup contagion," as some work has labelled it). Some of the most promising models have been derived from work in epidemiology and public health, where spatial phenomena are surely relevant but often secondary. It seems doubtful to me whether in highly developed societies, where so much communication is by advanced technology rather than face-to-face, spatially oriented contagion models will be the most useful ones. But Reynolds' work, with other variants of contagion models, deserves careful attention.

The remaining papers manage to cover phenomena ranging from urban to international politics. In total, the contributors number nine geographers and four political scientists; there is no point in trying to summarize all the papers, but the reader may consult the book's good introduction for an overview. The volume stems in part from a session at the 1969 Annual Meeting of the American Political Science Association. Although several contributions were written after that, the combination of publication lag and reviewing lag means that this is no longer the place to go to find out what is newest and most exciting at the juncture of political science and geography. But it is a good place to begin.

BRUCE M. RUSSETT

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Crime, Rape and Gin: Reflections on Contemporary Attitudes to Violence, Pornography and Addiction. By Bernard Crick. (Buffalo, N.Y.: Prometheus Books, 1974. Pp. 96. $7.95.)

If the success of a book is to be measured by how well the author achieves his stated purpose, then Professor Crick's small volume is quite successful. The aim set forth at the outset is to clarify the problems connected with violence, pornography, and drugs. The central issue in the author's mind is tolerance, and that issue binds together the three "sermons" (the author's own playful designation) that make up the book. Professor Crick's manner is unpretentious and discursive; his observations are often amusing and are informed with thought and common sense.

The views developed might be briefly summarized (although the author carefully refrains from stating grand conclusions) in terms of a twofold doubt—that social order is threatened by violence, pornography, or drugs, and that liberty is threatened by every governmental regulation designed to deal with these problems. Professor Crick is skeptical of the cries of alarm raised both by conservatives who see contemporary society as disintegrating and by libertarians who discern the onset of totalitarianism in every limit imposed on individuals. This two-edged doubt makes an effective weapon for defending toleration. It serves to hold off both these enemies of toleration who believe that it imperils order and those dangerous friends who would burden it by turning it into an absolute.

It must be asked, however, whether a reviewer is bound to judge a book strictly in terms of the author's stated purpose. That question arises in the present instance because the philosophical principles underlying the social commentary do not seem quite so strong as the commentary itself. The author seems often to make do with common sense in place of theoretical rigor. In his behalf if must be emphasized that he is not trying to formulate a theory of toleration. He does, however, more than once remark that he is a political philosopher and that the clarification he is seeking is of the kind a political philosopher is presumably in a position to provide. Hence it may be appropriate to accord at least brief attention to philosophical underpinnings.

One of the major principles the author brings into play is a strong one, namely, that toleration does not imply approval; on the contrary, it carries with it an obligation to view social behavior with discrimination and with a willingness on occasion to condemn. He insists on distinguishing between toleration and permissiveness. The efficacy of this distinction is suggested in his statement that "those who wish to be their brother's keeper had better really try to do so, to do so themselves with all the trouble, time and care involved, not to agitate for the police and the courts to do it for them" (p. 89).

The author has no doubt that we occasionally have a right to call on the police and the courts, however, and it is in trying to answer the old question "When?" that his basic principles—so far as they are manifest in this book—begin to seem not quite as clear and consistent as those of a political philosopher should be. Mill's distinction between self-regarding and other-regarding actions is explicitly rejected and the legitimacy of limiting freedom for an individual's own good is affirmed. But when it comes to defining the criterion of intervention the author does little more than appeal casually to the likely effect of an action on "sociability." Censorship, for example, can be defended only in cases involving a "reasonable, well-founded likelihood of danger and damage to sociability" (p. 57). But what is sociability? This is not made clear. Yet the term is a very broad one and it might readily be defined in a way that would render Professor Crick's position, contrary to his own intentions, practically the same as Mill's.

Not only does Professor Crick seem to have in mind no clear principle for deciding when toleration may be limited, he also provides no clear reason why it should be preferred where regulation is not imperative. He seems to favor toleration merely on the commonsense grounds that it is better not to become involved in telling people what to do unless you have to. The reader gains little sense that toleration is a positive good.

These lacunae are particularly noteworthy in view of the fact that the author identifies himself as a socialist. Is toleration fully compatible with social justice? If not, which is to be preferred? There is no sign that the arguments of Wolff, Moore, and Marcuse, in *A Critique of Pure Tolerance*, have been seriously considered.

In sum, *Crime, Rape and Gin* is not in all ways a book one would have expected from a political philosopher; it is, nevertheless, a wise and pleasant book.

GLENN TINDER

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Machiavelli and the Nature of Political Thought. Edited by Martin Fleisher. (New York: Atheneum, 1972. Pp. 307. $10.00, cloth; $4.95, paper.)

This book came to me very late to review and I have been sitting on it for a long time, and have needed two reminders to get it out; and probably the person before me just did not do it. Alas, one sees why. Nothing academically has cheered me more in recent years than the formation and fluorishing of the Conference for the Study of Political Thought. But that is not to condone the terrible itch to publish conference proceedings, regardless of coherence and quality. I want to use this review as an excuse to say something about this bad intellectual habit; but first to the book itself.

All but one of the papers printed here were presented at various meetings sponsored by the Conference for Political Thought. Most of them were presented at York University, Toronto, in 1969 to celebrate the five hundredth anniversary of the birth of Machiavelli. The first two essays in the book have nothing to do with Machiavelli at all, Sheldon Wolin's by now famous and much reprinted essay, "Political Theory as a Vocation," and Alan Ryan's "Two Concepts of Politics and Democracy: James and John Stuart Mill." The "and" in the title of the book turns out to be the disjunctive, not the conjunctive "and." "*And* if our fellow citizen about to be hung doesn't wish to address all those here assembled in a dying speech, I'd like to say a few words on the tariff question."

Wolin's few words are very good words, about the necessary involvement of political theory in politics, about the fundamental politics of perceiving what we are, what we could and should be doing: not that narrow "realism" which cannot see beyond the end of its own nose, except when it lifts its head in rhetoric to universalize its own local values. It is concerned with questions about "the quality, direction, or fate of public life." What is interesting is that most of the debate that followed the publication of Wolin's essay, in this journal and elsewhere, has taken the form of the old empiricist and behavioral battle against both the importance of ideas as explanatory factors and as rational moral critique. But the more interesting thing about Wolin's argument is now it touches the New Left. They are so right to demand relevance to human purposes rather than to the interest groups of the Fifteenth Precinct (or the real attempt, as Nixon tried in America and Wilson tried in Great Britain, to treat a nation as if it is a precinct or a constituency), but are so wrong, he shows, to think that any one theory is always relevant. The interplay between theory and practice is much more complicated than that. Political theory is not specific to a single social context: the range of theory is the whole range of memories and possibilities contained in our historical human culture. Putting things in a Marxist, a Christian, or a positivist context is all very well, but the contexts are only self-contained by the assumptions of the theory. What Wolin is really saying, it seems to me, is that political theory is a philosophical and historical critique of political doctrine. As in Oakeshott's thought, the process is continual and without end; but unlike Oakeshott, Wolin has no contempt for doctrines, only a humane awareness that all are attempted, at some time or other, but that none prove final.

Alan Ryan's essay on the two Mills is interesting and important (but what is it doing here?). It shows how far John Stuart was able to break from the strict utilitarian and market economy philosophy of his father, J. S. Mill, the libertarian, the "free spirit," the putative social democrat and communitarian. Within liberalism, there is not simply (as Louis Hartz used to argue so eloquently) one tradition, but rather two—"not obviously compatible" as Ryan modestly concludes.

The five essays on Machiavelli, with some snippets of conference commentary, range widely. They are oddly overly-specialized for such an anniversary assessment; none raises quite those central issues of political theory that are found in Wolin and Ryan's essays, although it would have readily possible to do so, even without committing oneself, simply to show how and why Machiavelli has appeared central to so many people and stirred up so many diverse reactions—as Isaiah Berlin did so cleverly in his "The Originality of Machievalli" in the celebratory studies edited by Myron P. Gilmore, *Studies on Machiavelli* (Sansoni, Florence, 1972).

Fleisher argues in "A Passion for Politics" that Machiavelli sees reason as solely concerned with technical means to pursue political life and struggle, not with ends. Perhaps, but if so, then he could be dismissed more easily. His implicit values are, however, much more interesting. Why does he say that evil actions may be needed to preserve the state? What sort of state was he thinking of? Certainly not something that justified itself simply and circularly in terms of power to retain power. Everything we know about the man and his life points in a humanist direction. To unmask hidden assumptions is not always to discredit, often the contrary.

If Sheldon Wolin can be thought to have played the broadening game, then J. G. A. Pococ, plays the narrowing game of political thought. And in any case the same chase appears more profoundly and at greater length in his recently published *The Machiavellian Movement.* Machiavelli is set firmly in his context, and no more than a rather eccentric late medieval thinker emerges. Pocock applies to politics, quite explicitly, the Kuhnian notion of a scientific paradigm, and then explores what paradigmic structures of political thought were available to Machiavelli—and reaches his narrowing conclusions by walking that reverse spiral. But if one doesn't accept that there are such complete and exclusive paradigms in political thought, if such "paradigms" are more likely to be symbolic than structural, then this is a wild goose chase.

Robert Orr, somewhat an Oakeshott disciple, would also wish to see ideas set firmly in their context, but has no such rigid ideas about context as Pocock. His essay on "The Time Motif in Machiavelli" (also published elsewhere) is excellent. Machiavellian politics emerges from this as the politics of adaptability and flexibility, spanning the whole range of time from the instant reaction to attack, to the hope for personal immortality through fame. Harvey Mansfield writes on Machiavelli's *Florentine Histories,* a very stimulating, original but unsound conference paper: obviously a stage in thinking about a wider problem, just the thing for a conference but not ready for publication. Finally, there is an essay by Neal Wood on "The Value of Asocial Sociability" which traces the theme of the positive value of social conflict from Machiavelli to Sidney, Montesquieu, and finally (if arguably) to Kant.

What I really want to say, however, is that perhaps rising printing costs and the state of the economy can save us from ourselves, that conferences can return to their pristine innocence as places for the discussion of ideas and, if for the presentation of long papers at all, then only of work in progress, not papers prepared for publication of which, perhaps, a few lines will be changed before publication. I even attended a conference a few years ago on a special theme of political thought that really *needed thinking about* in which it was simply *assumed,* not even said in the invitations or agenda, that papers would be published; and therefore the knowing ones brought papers ready prepared, in a technical sense, for publication, and defended them—which was the end of the free play of critical thought at the highest level about important matters, the end of the very thing for which Sheldon Wolin argues. Political thought is essentially an oral, critical, dialectic, discussing tradition and activity. Publications should be fewer and far more considered. Conference organizers should not seek for monuments, and conference sponsors should always—I suggest—make it a condition "that the grantees will *think* and, if they must have conference papers as a trigger to discussion, will *not* publish them together; and if they publish them at all then not in a form recognizably the same as when first delivered, otherwise we will judge the funded activity to have been a waste of time and will sue for our money back."

BERNARD CRICK

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Can Social Science Help Resolve National Problems: Welfare, a Case in Point. By Leonard Goodwin. (New York: The Free Press, 1975. Pp. x + 214. $8.95.)

This book begins with the premise that, despite the lip service paid to the notion of making social science more relevant to national problem solving, this has not resulted in major new research or problem solving efforts. Goodwin attributes this deficiency largely to the Galilean-Newtonian approach to science where there is no place for what he calls "the reflexive quality of human beings." This reflexive quality is essentially the ability of individuals to become aware of knowledge about themselves and to make self-consciousness an integral part of their approach to science.

Central to Goodwin's critique of contemporary social science methods and the assumptions undergirding these methods is his argument that there is an absence of social science interest in becoming involved in the problem-solving process. Goodwin contends the philosophy of science traditions on which social scientists are reared encourage concern with research techniques and not with understanding social problems which would thereby advance social problem solving. Goodwin devotes a large portion of his argument to the development of a philosophical approach to social research which he calls "experimental social research." Goodwin is not arguing for the application of experimental design principles and methodology to social problem solving. What Goodwin means by "experimental social research" is the involvement of individuals in the design of research efforts directed at social problem solving, a critical examination of their own basic beliefs about the problem, and the formulation of policies and implementation of social programs for the resolution of these problems. This arrangement has already been made, of course, by numerous psychologists, sociologists, and even a few enlightened political scientists.

Goodwin's earlier research on work orientations of the welfare poor and middle class demonstrates that policy makers maintain certain assumptions about the nature and cause of the welfare problem. In the current text, he further analyzes these data and contends that the beliefs held by policy makers about the welfare problem lead to their formulating solutions that are congruent with these beliefs but ill-suited to dealing with this social problem. Goodwin's experimental social research approach would overcome this deficiency because it would include the following individuals in the problem solving effort: policy makers (the donors), recipients of services (the clients), and social scientists (the researchers). The interaction of these three groups, Goodwin contends, would lead to greater understanding about the nature and cause of the social problem and therefore result in social policy that was more adequate to the task of addressing the particular problem.

The crux of Goodwin's argument centers on his recommendation that a Public Research Corporation be chartered by the U.S. Congress. This research corporation, using the experimental social research approach, would constitute appropriate client, policy-making, and research groups to address specific social problem areas. Only by employing this strategy does Goodwin feel that social science can help resolve national problems.

Goodwin's book is a thoughtful, if tedious, polemic and suggests this man's genuine concern for making social scientists significant actors in the problem-solving and policy-formulating process. Those who disagree with his arguments and recommendations should give evidence of their concern not by attacking Goodwin, but instead by developing their own thoughts and explicating them for students, scholars, and policy makers.

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Handbook of Political Science, Volume 1. Political Science: Scope and Theory. Edited by Fred I. Greenstein and Nelson W. Polsby. (Reading, Mass.: Addison-Wesley, 1975. Pp. 414. $13.95.)

This is a first-rate book, instructive to those who seek a philosophy of political inquiry or who want to rethink the one that they already have. The book includes five chapters by different authors. Dwight Waldo puts the discipline in historical perspective. J. Donald Moon writes on explanation and interpretation. Dante Germino argues the relevance of the classics of political philosophy. Felix E. Oppenheim discusses central concepts and their explication. And Brian Barry and Douglas W. Rae together analyze the problem of political evaluation.

The book is well conceived. Given the number of contributors, it is surprisingly coherent. As a "handbook," it is better described as reflecting the state of the discipline than as exposing weaknesses or calling attention to needs or breaking new ground; but some new ground gets broken nevertheless, especially in the chapters by Moon and by Barry and Rae. Since the book reflects the state of the discipline, it gives reason for both pride and dismay.

Waldo's "primary method," as he himself says, is "historical-interpretative . . . used as a means of understanding contemporary political science in the United States" (pp. 2–3). He sketches relevant developments in Europe from Plato on, and then focuses on American political science in successive periods. The account is essentially reportorial and rather uncritical, and it cannot help but cover much of the same ground already so well covered in Somit and Tanenhaus, *The Development of American Political Science.* The account is no doubt an accurate reflection of the state of the discipline in that it takes the state system for granted and raises no question about the tacit assumption of most American political scientists that the discipline is essentially concerned with domestic politics, and that comparisons are to be at the domestic level rather than between the domestic and international levels. This is indicated on the very first page, where the definition of political science that is offered is Easton's—referring to the authoritative allocation of values for a society; it is obvious that Easton in formulating the definition and Waldo in quoting it did not have a world society in mind. International politics gets its first mention, a brief one, in the section on political science since World War II. Neither the International Studies Association nor the *International Studies Quarterly* gets any mention at all. It is interesting and paradoxical—though I suspect a faithful reflection of the tacit assumptions of most political scientists—that Waldo uses World War I and World War II in marking the periods of the study of political science, yet pays little attention to the question whether the reasons for or causes of such wars are or should be studied. To be sure, everyone knows that international politics is there—but it is politics with a difference. *Real* politics is domestic, and international politics, when remembered, has to be treated on the side.

Moon focuses on explanation, deliberately leaving to others the treatment of the normative aspects of political science. He analyzes two models of explanation: the naturalistic or scientific model, with its emphasis on generalization based on laws and empirical theories, and the interpretative model, with its emphasis on particular events and on the intentions and beliefs that guide individual actors. He is respectful of both models, each in its place. Moreover, he seeks to synthesize them. He does this by pointing out that the actions of many individuals sometimes combine in such a way as to produce consequences that none of them intend, and that explanations of the unintended consequences may be quasi-causal. The quasi-causal explanations rest not on knowledge of specific, individual actors but on knowledge of man-in-general—on a "model of man." The dominant if not the only model now used is the rational choice model, which obviously assumes that people choose rationally and which depends on knowledge of the values and beliefs that guide them in the choices that they make; it depends on knowledge concerning attitudes about ends and means.

Germino describes himself as making a case for the importance of political philosophy. To him this means substantially a case for the classics and a focus on essences: What is man? What is society? What is history? These questions are abstruse. Germino sketches answers given by what he calls theocentric, anthropocentric, and metastatic humanists, but the sketches are necessarily brief, and those relating to the essential nature of society and history are either obscure or indistinct from each other. Moreover, the obscurity of the answer to the question, "What is society?" has a bearing on Germino's later statement that "the primary purpose of a political philosopher is to explore the individual's relationship to society. . ." (p. 259). He treats the question parochially, despite the contrast he makes between a philosophical and a parochial political science. His "society" is evidently the population of a state, and the population is evidently homogeneous. He ignores the fact that "societies" are sometimes segmented and that politics goes on both among the segments and among the "societies" themselves. Surely the human relationships that count in political philosophy occur not only between the individual and society but occur also within and among groups of a number of kinds—national, racial, linguistic, religious, etc.—and the philosopher should seek out rules concerning the right and the good pertaining to these relationships the world around.

Oppenheim's declared concern is with "the explication of basic concepts." He seeks "to illustrate the more general problems of conceptual analysis." Moreover, he wants to acquaint political scientists with "the relevant trends in analytic philosophy" (p. 284). Starting with a focus on concepts, he speaks in effect of real and nominal definitions, divides the nominal into the stipulative, the explicative, and the reportative, and then limits himself to the explicative—that is, to the clarification of concepts taken over from the language of everyday life. The analysis that follows is insightful and stimulating, but the effort to do several things at once blurs the focus. Questions about the definition of words get mixed up with questions of methodology and epistemology. Are empiricism and behaviorism really methods of explicating concepts? If one emphasizes *Verstehen,* is the concern primarily with the explication of individual concepts or with the formulation of correct statements? Should one announce in a heading (p. 309) that the focus is on "categorical and comparative *concepts*" and then in the first sentence under the heading speak as if the adjectives really refer to *methods?* What is one to make of an exposition that (1) assumes and asserts (e.g., p. 320) that concepts—as distinct from the statements in which they appear—can be classified as either descriptive or normative; (2) says that *legitimacy,* for example, is a normative or moral concept, but that it can nevertheless also be used in a descriptive-legal way (pp. 320–21); and (3) says that "normative concepts had best be used only to express the *advocacy* of some political action or goal. . ." (p. 327)? The components of the chapter are all significant, and much about them is perceptive and instructive; but the various purposes are pursued in such a way that the clarity and the potential forcefulness of the analysis are impaired.

The chapter by Barry and Rae is notable. Its very inclusion marks a rejection of the view that *the* purpose in political science is explanation. More affirmatively, it marks the acceptance of the view that political scientists should concern themselves not simply with structures and processes but also with policy outcomes; or, as Barry and Rae say, it marks the acceptance of the view that, along with the study of how decisions are made, political scientists should also take up the question *which* decision ought to be made. ". . . [A] withdrawal from engagement with the problems of evaluation is untenable" (p. 337). The question, "What shall we do, and how shall we live?" was not initially directed to political scientists; but political scientists, in their teaching and research, should aim to contribute to the answer nevertheless. The problem is that the chapter leaves one somewhat uncertain about the policy issues that are appropriate to political science and about the criteria for selecting them.

Whatever the issue, "the ultimate ground of evaluation lies in the well-being of individual human beings . . ." (p. 390). The consequence for well-being is the touchstone for all political decisions. Barry and Rae find that predicting consequences is often beyond human capacity, and they suggest three simplifying mechanisms: "focal descriptions, duties and obligations, and partial goods." Descriptions are focal when their characterizations are evaluative—e.g., when relationships are indicated between what is described and the public interest, justice, equality, freedom, or democracy. Duties and obligations are indicated, for example, by the French Declaration of the Rights of Man and by declarations of human rights promulgated since World War II. "Partial goods" are goods that are less than general or universal; actors pursuing them accept a division of labor and concern themselves primarily with the good of those with whom they have a special relationship rather than with all human beings.

Barry and Rae do a little more than the other authors to recognize the existence of international politics. They acknowledge that groups as well as individuals have claims that need to be evaluated—claims to freedom, for example, and in some cases even claims to sovereignty. They bewail the fact that "only a tiny fraction of the energy that has gone into the improvement of domestic conditions has been applied to the prevention of war" (p. 375). Moreover, some of their prescriptions for evaluation can be applied to politics at every level. Nevertheless, the pervading orientation is toward domestic politics.

Reasons for both pride and dismay are fairly clear. The volume reflects substantial achievements in human thought and provides a philosophy of inquiry that, on the whole, is commendable. It is distorted, just as American political science is distorted, in the undue orientation to the domestic and in the corresponding widespread failure to think of individuals and groups in relationships with each other and with a world society.

VERNON VAN DYKE

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Handbook of Political Science, Volume 2: Micropolitical Theory. Edited by Fred I. Greenstein and Nelson W. Polsby. (Reading, Mass.: Addison-Wesley, 1975. Pp. vii + 389. $13.95.)

This volume contains five essays (on personality and politics, socialization, recruitment, group theories, and organization theory) included on the basis of their relevance to micropolitical theoretical concerns. However, parceling out the discipline into categories admits of no single solution, and the reader should be aware that other volumes in this series (especially volume 4) address related topics. While each contribution has value, the chapters lack a consistent framework which renders the volume as a whole neither an overall review of the current state of knowledge nor a well developed blueprint for future research.

Fred I. Greenstein's chapter on personality, which begins this volume, draws heavily on his earlier *Personality and Politics: Problems of Evidence, Inference, and Conceptualization* (Chicago: Markham, 1969). While this reduces its utility, nonetheless this essay is an excellent example of what one hopes to find in a handbook—a concise review of the critical theoretical issues, and a literature review which integrates a large and diverse body of material so as to elucidate the underlying theoretical concerns and also provide a useful substantive introduction to the field. Greenstein's discussion of the criticisms of personality theory succeeds admirably in clarifying the nature of the debate, but only partially in resolving the problems he so lucidly discusses. Perhaps most valuable is his insistence that personal and situational qualities influence political behavior in a joint, interactive, contingent manner.

David O. Sears, writing on political socialization, and Moshe M. Czudnowski, on political recruitment, each adopt an approach focused more heavily on the substantive content of the field. Sears organizes his essay so as to identify the replicated findings on the central dependent variables and to address what is perhaps the most pressing question in political socialization, the persistence of preadult learning. He stresses the danger of confusing "partisan" responses (i.e., attitudes toward the authorities, groups, ideologies, and the like) with system attachment, an invaluable point.

What is not included is extensive discussion of socialization agencies or processes, the comparative utility of various theoretical perspectives (e.g., social learning theory), or more than a brief sketch of common criticisms of the field. He concludes with a well-taken but generally stated argument that socialization research needs greater analytic depth and a cogent case for clarifying the measurement of independent variables to move beyond descriptions of children's attitudes to causes.

While hardly a comprehensive review of the field, the above approach is more useful in Sears' case than in Czudnowski's because of the "sprawling" (p. 93) character of socialization research and its recent proliferation. Czudnowski deals chiefly and most successfully with the major variables which tend to dominate recruitment research. The graduate student can find here a useful summary and discussion of the major works in this area. However, this is the least ambitious of the chapters, perhaps because of the more limited nature of the field it surveys.

Czudnowski advocates a "microtheoretical" approach (p. 232) focused on identifying the "sociopsychological profile" (p. 230) of social positions likely immediately to precede initial recruitment, an interesting idea for avoiding the problems of studying only the successful recruits. However, neither the discussion in this chapter nor the field of political recruitment in general has enough to say about the relevance of recruitment to elite behavior in office or system characteristics.

The last two chapters, J. David Greenstone's article on group theories and Dennis J. Palumbo's on organization theory, place much greater weight on articulating a point of view than on surveying the field in a comprehensive manner. Greenstone, for example, neglects any systematic discussion of the long pluralist-elitist debate in discussing group theories. While his essay cannot serve as a general introduction to the field, he elucidates the theoretical relationships among five major contributors to the field, concentrating on the concepts of group and interest.

Greenstone's central conclusion which he argues cogently and convincingly is that, because of the way in which interest is conceptualized, group theory cannot adequately deal with the sudden emergence of novel political demands such as those of the civil rights movement. However, he is less convincing in arguing that the solution to this problem is to incorporate "objective" measures of interest.

Palumbo's essay, like Greenstone's, is organized around a particular theoretical argument rather than designed to present a general overview of the field of organization theory. It stands along in this volume, however, in arguing *against* the utility of its topic's approach. Palumbo concludes, after sketching the elements of the political model and extensively and skillfully discussing selected theory and literature of rational choice, "that a political model . . . seems to be a better way to explain the empirical world of organizational behavior than the pure theory of rational choice" (p. 361).

He argues that most decision making in political organizations (which typically have multiple goals and malevolent opponents) takes place under conditions of uncertainty; further, that this uncertainty cannot be practicably reduced to decision making under conditions of risk; and hence rational choice theory must be so modified as to amount to a rejection of the rational model. Palumbo presents a clear introduction to the debates concerning the applicability of organization theory to political science which should usefully orient readers whether or not they accept Palumbo's conclusion.

Overall, this volume is a valuable addition to political science reference works. While not providing as comprehensive an overview of both the theoretical issues and major substantive findings as it might, it is nonetheless a useful introduction to several important areas of political science for the advanced student and a convenient way for the professional to update her/his knowledge of fields not commonly monitored.

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Handbook of Political Science, Volume 3: Macropolitical Theory. Edited by Fred I. Greenstein and Nelson W. Polsby. (Reading, Mass.: Addison-Wesley, 1975. Pp. xiii + 620. $16.95.)

Volume 3 of the *Handbook of Political Science* is entitled *Macropolitical Theory.* It includes six essays contributed by recognized authorities in their fields—Huntington and Dominguez on "Political Development," Dahl on "Governments and Political Oppositions," Linz on "Totalitarian and Authoritarian Regimes," Michael Taylor on the "Theory of Collective Choice," Tilly on "Revolutions and Collective Violence," and Stinchcombe on "Social Structure and Politics." These constitute a reasonably comprehensive set of system-level themes. Two of the essays—Dahl's and Linz's—deal with varieties of political systems; another two—by Huntington and Dominguez and Tilly—deal with political change. One by Stinchcombe deals with social structure and politics; and the one by Taylor reports the results of formal-logical analysis of the collective political outcomes of postulated individual choices. Notable thematic omissions in a volume entitled *Macropolitical Theory* are the relations of regime types to policy outcomes, and the relationships of the international system to varieties of political systems and developmental patterns.

An evaluation of the volume has to concern itself with how well these essays cover their topics, assuming an audience of preprofessionals—undergraduate majors and first-year graduate students. Three of the essays, those by Huntington and Dominguez, Juan Linz, and Michael Taylor, were written especially for the *Handbook* and are good surveys of the literature and evaluations of the "state of the art." Dahl's contribution is a condensation of his *Polyarchy* and his contributions to his two edited volumes on oppositions. The contributions of Tilly and Stinchcombe, though written especially for the *Handbook,* represent rather inconclusive work in progress and are hardly appropriate as introductions to the theory of revolutions or the relations of social structure to politics.

Of the six essays, the two by Huntington and Dominguez and Linz are important new contributions, and by themselves would justify owning a copy of volume 3 of the *Handbook.* The chapter on political development begins with an historical account of the political development literature, and a summary of the hypotheses as to the relations among political, economic, and social modernization. It then proceeds to a discussion of the conditions and factors which may affect the processes and outcomes of political development, treating such questions as the impact of the domestic and international environments, the historical timing of developmental processes, the sequences of challenges or problems confronting political systems, and the rates of various aspects of social change. It then examines political development as a set of dependent variables admirably summarizing the research literature on such aspects of political development as political culture, political participation, political and governmental institutions, and national integration and cohesion. A final section discusses emerging theoretical trends and research foci.

In a contribution that occupies more than a third of the volume, Juan Linz presents what is easily the most comprehensive treatment of nondemocratic regimes to be found in the literature. It is in the tradition of Weberian historical sociology in which the specifics of individual cases and ideal-typical classification of varieties of authoritarian regimes are in acute tension. Linz resolves the tension in favor of spelling out the specifics of subvarieties and individual cases. This makes it difficult to come away from his more than 250 pages of fact-packed discriminations with a clean and parsimonious typology of nondemocratic regimes. He seems to impute an almost sacred quality to the richness and complexity of historical reality, and errs on the side of including too much.

His contribution begins with a long essay on totalitarianism, easily the best analysis in the literature. He deals with the origins of the notion, the pros and cons of its use as a typological concept, and an examination of regimes which have been characterized as totalitarian and of its varieties. While he acknowledges that no contemporary regimes can be characterized as totalitarian, he points out that the experiences of having been totalitarian, or seeking to become totalitarian, are sufficiently formative to warrant two subcategories of authoritarianism.

He then turns to surviving traditional and semitraditional regimes including some societies in North Africa, Southeast Asia, and Sub-

Saharan Africa, and the *Candillist* and *Caciquist* regimes of Latin America in this category of surviving traditionalism. Under the heading of authoritarian regimes properly speaking, he differentiates some six varieties: Bureaucratic-Military Authoritarian regimes, Organic-Statist Regimes, Mobilizational Authoritarian Regimes in post-democratic societies, Postindependence Mobilizational Authoritarian Regimes, Pretotalitarian and Posttotalitarian Authoritarian Regimes. The categories reflect the diachronic-synchronic aspiration of his typology. The prior history helps explain the principal operating features of the regime.

Having said all of these favorable things about Linz's contribution, it would be dishonest not to admit to exasperation over his prolixity, his refusal to summarize, his failure to draw things together. Historical sociology, to be sure, invites elaboration, differentiation, careful qualification, and acknowledgment of exceptions, but the greatest of historical sociologists, Max Weber, has shown that it is possible to combine this with sharp definition, useful abstraction, and lucid formulation of hypotheses. Perhaps the second edition of this *Handbook* will contain a leaner and more muscular version of this contribution.

It is more difficult to comment on Dahl's chapter. His theories of polyarchy and of oppositions are contributions of great importance to empirical political theory, but they do not constitute a handbook-like treatment of empirical democratic theory. Dahl's *Polyarchy* is concerned with the conditions which are favorable or unfavorable to democracy. His polyarchy scale enables one to locate regimes on the two dimensions of participation and competition, with "polyarchies" being those regimes that are high on both dimensions. Thus we can distinguish among real countries according to whether they are more or less polyarchic, and whether their shortfall is on the participation dimension or the "contestation" dimension, but he does not discriminate among relatively persistent historical types of polyarchy. For the purposes of Dahl's own work there was perhaps no need to do this; for the purposes of a handbook contribution on the empirical theory of democracy this is a serious omission. The work of Lijphart and others on varieties of democratic systems deserved to be treated.

Perhaps more serious is the omission of any treatment of *democratization,* or references to the literature dealing with it. Dahl's own discussion of historical sequences is a formal one and surely the weakest part of his polyarchy theory. It is surprising not to find any discussion of Moore's *Social Origins of Dictatorship and Democracy,* Rustow's *Transitions to Democracy,* the Binder et al. volume on *Crises and Sequences in Political Development,* as well as a number of other serious contributions to the topic of democratization.

Michael Taylor's review of the literature on the theory of collective choice is an excellent summary of this new genre of macropolitical theory, in which collective political outcomes are generated from the logical manipulation of the postulated utilities and resources of political actors under assumed sets of decision rules. As he himself acknowledges, this is very far indeed from being empirical macrotheory. He points out that this formal deductive work has not as yet come to grips with the problem of time. In these studies, he acknowledges, no provision is made for dealing with "changes in preferences, in perceptions of the alternatives, in voting strategies, or in expectations about other individual's preferences and behavior: no changes take place as a result of persuasion; there is no accumulation and reinterpretation of experience, no learning, no adaptation" (p. 469). One misses in this thoroughly honest review of the collective choice literature some estimate of the value of this approach to hypothesis generation as compared with inductive approaches, or mixed inductive-deductive strategies. One also misses some appraisal of the limits of mathematical modeling of political reality.

Charles Tilly, in "Revolutions and Collective Violence," reviews and criticizes some of the literature on revolution and violence and then presents his own theory. He is not kind to the competition, as his metaphor of the old-time doctor sniffing chamber pots suggests. His two principal straw men are Chalmers Johnson and Ted Gurr. He disposes of Chalmers Johnsons' sociological system-dyssynchronization theory of revolution as too general to be of value in research; and as failing to separate out the political realm in which revolutions occur from the society or social system as a whole. He disposes of Gurr's essentially psychological explanation of collective violence as the consequence of "relative deprivation" under specified conditions, on the grounds of the poor quality of its data and some questioning of index construction. He is kinder to Barrington Moore and Eric Wolf because of their emphasis on social class and class coalitions, though surely their work would warrant some criticism as well.

Tilly concludes that the theory of collective violence and revolution is now a new ball game, and that powerful theory will emerge from the collective history movement in which he is playing a prominent role. Rewriting history "from the bottom up" using modern research methods makes eminent sense, but Tilly's own contribution to the theory of revolution seems to rest only in small part on such a research program.

Tilly deals with collective violence and revolution separately and never quite succeeds in relating the two in theoretical terms. One is at a loss to explain why he sought to offer a theory of both simultaneously. It would have made more sense to deal with the one or the other as a major theme, bringing in the minor theme in some appropriate way. His one contribution to the theory of revolution is a specifically political one, the positing of a set of proximate conditions of revolution which are four in number:

> 1. The appearance of contenders or coalitions of contenders, advancing exclusive alternative claims to the control over the government currently exerted by the members of the polity.
>
> 2. Commitment to those claims by a significant segment of the subject population (especially when those commitments are not simply acknowledged in principle, but activated in the face of prohibitions or contrary directives from the government).
>
> 3. Formation of coalitions between members of the polity and the contenders advancing the alternative claims.
>
> 4. Incapacity or unwillingness of the agents of the government to suppress the alternative coalition or the commitment to its claims (p. 521).

Conditions 1, 2, and 4, he argues, are necessary to revolution, The third is neither logically nor empirically necessary but facilitates the fourth condition. Now, if one reflects on these three necessary conditions of revolution for a moment, they appear to come perilously close to the primitive paradigm that all things have a beginning, a middle, and an end, or to the proposition that revolutions are revolutions. In fairness, he elaborates these conditions in a section in which he draws on historical examples, and then looks at the French Revolution from these perspectives in greater depth.

In view of the relatively undramatic dénouement of this contribution, one is led to ask why it was necessary to demolish Chalmers Johnson and Ted Gurr at the beginning, particularly since it would seem that a good theory of revolution would have to have sociological, psychological, and political components. Finally one is led to ask why Charles Tilly, with his rich fund of historical and contemporary knowledge, fails to provide his readers with some straightforward discussion of varieties of revolution, and their conditions and consequences.

Stinchcombe's "Social Structure and Politics" is hardly the introduction to political sociology one would be led to expect under such a title. He provides us with no intellectual history of political sociology within which his contribution can be located. He fails to define either social structure or politics. And although he tells us that he has "put on blinders" and presented an oversimplified theory, he doesn't specify how he has gone about his oversimplification, what he has left out, and what the costs and benefits are of his particular strategy of explanation. Finally, in his conclusion he doesn't tell us what he has accomplished and what research programs might follow from his analysis.

The method that he employs is a combination of rational choice and exchange theory, but unlike the better exemplars of these approaches, he does not present his propositions as heuristics, but as empirical explanatory propositions, checking them illustratively by occasional real examples. One might best describe this effort as work in progress and as inaccessible to most readers who would consult this *Handbook*.

These various criticisms notwithstanding, the editors of this *Handbook* have performed a useful service in publishing this volume on *Macropolitical Theory*. This reviewer has found the volume eminently usable for teaching purposes at the incoming graduate level. Perhaps a second edition will remedy some of the shortcomings suggested here.

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Handbook of Political Science, Volume 4: Nongovernmental Politics. Edited by Fred I. Greenstein and Nelson W. Polsby. (Reading, Mass.: Addison-Wesley, 1975. Pp. xviii + 285. $12.95.)

The title of volume 4, *Nongovernmental Politics,* is a bit strange, but the contents are familiar enough. The intended coverage, especially viewed in relation to volumes 5 and 6 (on governmental and policy-output aspects, respectively) is of the more informal, extragovernmental activities often lumped together as "political inputs." Chapters on political parties (by Leon Epstein) and interest groups (by

Robert Salisbury) predictably appear. Beyond this point jurisdictional claims, especially between volumes 2 and 4, are not so clear. Individual voting behavior might be fitted into such a volume, but might equally fit into volume 2 as one of the more successful efforts at "micropolitical theory." And the same would apply to the topic of individual political participation. Both in fact appear in *this* volume, the former sharing a chapter with the topic of public opinion. This rounds out the volume, which is slimmer and, at least in three of the four chapters, less empirical than one might have expected.

The generation schooled in the Gabriel Almond input functions might inquire for the location of chapters on political recruitment and political socialization; however, they are in volume 2. More mystifying is the absence, both in volume 4 and the entire series, of a chapter dealing explicitly with political communication, press, and other media. Since at least one of the series editors has a long-standing interest in this area, this omission suggests that at least one potential contributor may have missed the final deadline. Fortunately, there is a recent alternative publication with extensive coverage of this area (Ithiel de Sola Pool et al., *Handbook of Communication*, 1973).

For the chapter on "Political Participation" the editors turned, not surprisingly, to Norman Nie and Sidney Verba, for whom the assignment must have come with almost perfect timing. Nie and Verba have wrestled with the topic for most of a decade, had recently published their book on *Participation in America*, and were in the midst of further analysis of extensive cross-national materials. The Nie-Verba emphasis on four distinctive modes of participation, rather than a single continuum, seems well on the way to acceptance as a dominant research paradigm. But it is recent enough—in contrast, for example, to the pioneering work of the Survey Research Center on individual voting—that it is not yet encrusted with layers of criticism, extension, or modification.

Given this propitious timing, Nie and Verba launch into their topic with verve and enthusiasm. And with substantial data. Their chapter presents some 11 tables and 15 figures; the balance of volume 4 includes only 3 additional tables. The organization of the material follows lines worked out in their earlier book on the United States, but with a considerable extension of cross-national comparisons of the various modes of participation. Of course the richness of primary data and coherence of presentation are achieved only at a certain cost: chiefly an acceptance of the overall Nie-Verba view of the topic. This is a broad but not unlimited view, since it excludes nonlegal activities, subjective orientations, participation mobilized *by* government (rather than autonomously directed toward the government), and participation in other aspects of society. The survey data range widely across the United States and Western Europe (including Yugoslavia), but also extend to Japan, India, and Nigeria. The time-frame, however, is strictly contemporary.

If sales of volume 4 outrun those of some other volumes, much of the explanation will lie in accumulated professional interest in what a Phil Converse (or Donald Stokes if he had not gravitated into administration) would have to say about recent changes in American political behavior and various "revisionist" interpretations of such behavior. Converse undertakes this task in a long chapter (94 pages) dealing with the related topics of public opinion and individual voting behavior. In contrast to the data-oriented presentation of Nie and Verba, Converse faces the task of organizing and evaluating a vast and varied body of literature. The result is a closely reasoned and heavily footnoted summary in which the author only rarely permits himself the luxury of presenting new data (there are three tables, presenting descriptive material of a comparative sort). For converse—the data analyst nonpareil—work on the chapter must have represented a sacrifice as well as an opportunity.

Converse analyzes public opinion largely in terms of information level, motivation, and degree of attitude-structure of coherence. These are of vital concern for students of electoral behavior, but they tend to narrow the topic to a marked degree. A full chapter on the topic might have explored how public opinion contributed to the resignation of a president from office, the effects of recessions or limited war on popular approval or disapproval of administrations, or how legislators seek to mobilize specific "issue publics" (to borrow a felicitous Converse term). On the topics treated we have Converse's measured judgment that low information levels remain highly important (acquiring new information is most likely for the minority with a substantial body of previous information, or for those with unusually high motivation). He sees increases in the consistency of mass "belief systems" as sweeping, but probably not indicative of nearly as marked a shift in actual ideological sophistication. He presents some cross-national evidence which hints that the level of attitude-constraint for the U.S. in the 1964 and later period may be above the norm for modern industrialized countries, rather than the 1950s reflecting a period well below the norm.

On the central puzzle of the determinants of voting choice, Converse concentrates his attention on the difficulty of apportioning the explanatory influence due to issues and to party when—as is usual—the two are substantially colinear. At this point one faces a choice of tactics. One can formulate a multiequation model, incorporating effects of party on issue-orientation as well as on the vote and possibly extend this to estimate reciprocal effects of issues on party. Converse opts for the other route of turning to various bits of "side information" which are suggestive of causal and temporal ordering. Predictably enough the "side information" that he values most suggests that party identification is, for all practical purposes, remarkably stable in the short-run (though subject to gradual strengthening over the life-cycle). By contrast, he sees issues as quite unstable. Hence the judgment that most of the "overlap" in explained variance should go to party; hence also the enthusiasm for the table-oriented "normal vote" concept.

He continues the long and fascinating discussion with briefer consideration of macrolevel changes in the electorate. Cohort analysis of the 1930s suggests the absence of any single "critical election," but rather a gradual realignment resting mostly on heavy Democratic gains among new voters. This is congenial with Converse's view of the stability of party identification. The final section considers the design of "representation" studies involving both elite and mass interviews (a procedure, incidentally, pioneered by the late Samuel Stouffer).

The third chapter, "Interest Groups," takes up a topic on which conceptual schemes and typologies rather outrun verified generalizations. Salisbury thoughtfully reviews the Mancur Olson arguments that "selective benefits" (and not the collective policy goal) are crucial. If, however, significant numbers of people take the "golden rule" or Kant's "categorical imperative" seriously, the Olson problem dissolves. It is sad to note the almost total lack of hard evidence on this issue to date. Salisbury emphasizes the importance of group organizers, or entrepreneurs, a factor rather neglected in David Truman's classic account. And mention should be made of William Gamson's (*The Strategy of Social Protest*, 1975) intriguing effort at a systematic analysis of a random sample of "challenging groups," published since the Handbook went to press.

The final chapter, on "Political Parties," is the briefest. The subject served as intellectual midwife to many of the topics dealt with at chapter length elsewhere. Presumably there does not remain that much to be said about contemporary parties per se. Leon Epstein skillfully spells out a modest comparative view of parties and their functions. Once viewed as threatening and seditious, parties tend now to be ignored or perhaps criticized for contributing to a false harmony. Even in Europe the trend seems to be to the "catchall" party. Politics can indeed be pursued by other means. One hopes it is not just an act of faith to conclude that the presence of contested elections between organized parties is, in part, important also for the indirect effect it has on the efficacy of other means of influencing governments.

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Handbook of Political Science, Volume 5: Governmental Institutions and Processes. Edited by Fred I. Greenstein and Nelson W. Polsby. (Reading, Mass.: Addison-Wesley, 1975. Pp. xx + 459. $15.95.)

It is almost impossible to do justice to this volume of the *Handbook of Political Science* in the limited space available. Each of the six essays is a comprehensive analytical survey of a broad topic that falls under the general rubric of "institutions and processes." The contributions include: "Constitutionalism," by Harvey Wheeler; "Federalism," by William Riker; "Executives," by Anthony King; "Legislatures," by Nelson Polsby; "Courts," by Martin Shapiro; and "Bureaucracies," by Mark Nadel and Francis Rourke.

Harvey Wheeler's discussion of constitutionalism is a *tour de force* that covers much of the history of Western political thought from Plato to modern social science as viewed through the development of one of its central conceptual and institutional features. Wheeler will draw fire for his claim that American constitutionalism is outmoded because of its mechanistic nature and for his assertion that the United States needs a more "systemic" approach built on the rule of law and the institutionalization of civil disobedience. He does not make this argument with sufficient clarity or on the basis of empirical evidence. Wheeler also underplays the influence of Locke and Montesquieu on the development of American constitutionalism, and he pays insufficient attention to the idea of constitutional balance. The concept of the balanced constitution has been a persistent and recurring theme in American constitutional politics and is more than mere political Newtonianism. But these are minor quibbles; Wheeler's essay is a sheer delight to read and it sets a tone for the volume that the subsequent contributions manage at times to match but never to exceed.

William Riker's consideration of federalism is a refinement, updating, and restatement of his 1964 critique. Although less polemical in tone than the earlier work, his argument is no less devastating: as an institution federalism does not make much difference in the substance of a nation's policies but it can alter the style of its politics. Federalism's effects are at worst to give advantages to minorities at majority expense and at best to be so trivial (a "minor nuisance") that it is not a significant issue.

Riker grounds his attack in a scientific theory of federalism based on empirical examples. The essence of federalism lies in the *political,* not economic or social, bargain that creates it and in the distribution of power in political parties which shapes the mature federal structure (p. 141). There are two necessary conditions to every federal bargain, (1) the existence of a threat or the opportunity for aggrandizement, and (2) strong provincial loyalties. The structure of a federal system is correlated with its party system; centralized federalism is accompanied by centralized parties. All else relating to the operation of federal systems, e.g., constitutional and administrative arrangements involving the areal distribution of power, are "accidents."

Riker's essay will probably draw more critics than the rest of the volume as he is highly critical of a cherished American institution. Federalism does not agitate him as much now as it did in the mid-1960s only because the race issue has been nationalized and federalism's current beneficiaries are no longer southern racists but economic conservatives seeking to evade centralized regulation. Those who challenge Riker will confront a carefully constructed argument. It seems to me that he is vulnerable on at least two points: failure to account satisfactorily for the sustained vitality of successful federations in which regional and national loyalties coexist over time and a too abrupt dismissal of constitutional and administrative questions, such as those recently raised in *National League of Cities* v. *Usery,* 49 L.Ed 2d 245 (1976).

Anthony King is the only non-American contributor to this volume, and his examination of executives contains a distinctively British flavor. Nevertheless, he draws heavily on studies of the American presidency to flesh out his essay. King's focus is research, or the paucity of it, on executives. He limits his treatment to heads of government and their top subordinates and concentrates on four topics: recruitment, the dynamics of executive institutions, executive-legislative relations, and psychological approaches.

King's survey of research on executives reveals that it is heavily concentrated on the United States and, to a lesser degree, on Britain. He attributes the general paucity of executive-related research to the difficulties of obtaining access to information and of quantifying that which is obtained. Although these are serious obstacles, King argues vigorously that they should not be allowed to deter future inquiry. Specific research needs include cross-national recruitment studies that establish the extent of the relationship between selection processes and subsequent behavior in office and a psychological approach that reveals the interplay between individuals and institutions. King calls for a research agenda on executives that is detailed, comparative (as between similar systems), and which tries to answer specific, answerable questions (pp. 243–44).

Nelson Polsby defines legislatures in terms of their distinguishing characteristics which include official status, legitimacy, multiple membership, formal equality of members, collective decision making, deliberateness, and subsequent accountability to the people. Polsby bases his analysis of legislatures on a typology that employs two variables: access to the political system (open/closed) and specialization of governmental activity. He devotes most of his attention to legislatures in open systems having specialized decision-making institutions, i.e., parliamentary democracies. These he places on a "continuum of legislative power which expresses variations in the legislature's independence from outside influences" (p. 277). At the end of the continuum are arenas for conflict between power centers located elsewhere, as typified by the British Parliament. At the other extreme are transformative legislatures with an independent capacity to make laws, for which the American Congress is the prototype.

In studying arenas, external institutions are of primary importance while internal structures are the central focus in the examination of transformative institutions. Polsby argues that the degree to which a legislature is transformative depends primarily on the character of its parliamentary parties. Thus, American legislative parties are "extremely coalitional, decentralized, and flexible," while their British counterparts are "somewhat less coalitional, hierarchical, and fixed" (p. 292).

Polsby is at his best when he recounts the development of systematic empirical study of Congress by American political scientists and when he examines the modernization of Congress as an institution. His conceptual framework has substantial value for taxonomic purposes and it provides considerable explanatory leverage.

Martin Shapiro's essay on courts rejects as "fruitless" and without approximation in the real world the conventional prototype involving an independent judge applying pre-existing legal rules following adversary proceedings and resulting in a decision in which one party is found to be right and the other wrong (p. 321). Instead, Shapiro offers as a more appropriate model a triad involving two disputants and a third party. The "triad for purposes of conflict resolution is the basic [and universal] social logic of courts" (p. 322). It is maintained in all societies by consent and, as societies become more complex, by law and office.

Tension arises in courts when they function as conflict resolvers because of the need to convince the disputants of the neutrality of the judges and of the laws they have chosen to apply and because of the introduction of a third interest, that of the regime. This tension increases when courts go beyond the functions of conflict resolution and social control and engage in law making. In fact courts are not independent institutions, but an integral part of the regime.

Shapiro sees the courts as major political institutions that perform multiple functions "ranging under varying circumstances from bolstering the legitimacy of the political regime to allocating scarce economic resources or setting major social policies" (p. 365). That this is not widely recognized by the public is due, in part, to the damage that such recognition would do to the basic logic of the courts (the triad) and the consequent development of guises, myths, and conventions that surround the judicial process. Political scientists have long been aware of the policy-making role of appellate tribunals, especially through the exercise of judicial review, but they have for the most part ignored the essential political nature of the entire judicial process. Shapiro performs a valuable service by staking the discipline's claim on the courts. This opens challenging areas for inquiry that we have hardly begun to explore.

The final contribution, by Mark Nadel and Francis Rourke, examines the political role of bureaucracies "as instruments of power or as sources of policy" (p. 373). It is a synthesis of the major themes found in the scholarly writings of the authors. Bureaucratic influence derives from technical expertise and is manifested through advice bureaucrats give to officials, the capacity of bureaucratic organizations to carry out essential and complex governmental tasks, and the exercise of discretionary authority by bureaucrats.

While public bureaucracies are essential policy-making institutions in all political systems, their role is especially crucial in the United States. Extensive political entanglement of government agencies occurs primarily because American political parties do not function effectively as linkage mechanisms between public opinion and public policy. Agencies cannot look to parties for aid in developing policies and as sources of support for programs. Instead they turn to organized private interests. The result is an aggravated problem of insuring administrative responsibility.

Nadel and Rourke analyze the controls for achieving bureaucratic responsibility through a typology which varies along two dimensions, location and formality. The controls are either external or internal to the agencies and bureaucrats, and they are exercised either formally or informally. All four types of controls are essential, no single set of controls can guarantee responsible bureaucratic performance, and the full range of controls cannot eliminate the tension between accountability and effectiveness.

To sum up, this is a most useful reference volume. The essays are comprehensive, thoughtful and often provocative, and theoretically well grounded. All are of high scholarly quality but they vary considerably with respect to their controversiality, imaginativeness, and their conceptual frameworks. Graduate students will find them invaluable in preparing for prelims, and scholars will do well to take them as starting points in preparing new courses and when undertaking research projects.

If there is one aspect of the essays that is disturbing it lies in the great extent to which they depend on the United States for illustrations and thus reflect the culture-bound character of American political science. (Anthony King's is a partial exception.) This may not appear to be a defect to persons interested mainly in American politics, but it is a severe limitation for those who seek to achieve scientific rigor in their analyses of political institutions. However, as long as an overwhelming proportion of the world's political scientists are Americans this condition is likely to continue. It does seem that minimally, more conscious comparative studies of institutions and processes are required if research is to advance much beyond single case analysis and a cumulative body of knowledge is to be developed.

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Handbook of Political Science, Volume 6: Policies and Policymaking. Edited by Fred I. Greenstein and Nelson W. Polsby. (Reading, Mass.: Addison-Wesley, 1975. Pp. xviii + 494. $15.95.)

Handbooks in any scholarly field present special problems in finding the relevant standard of evaluation. The highest criteria of scope, skill, and theoretical subtlety would doom reviewers to perpetual disappointment. Between the useful and the useless, however, this volume tends toward the satisfying.

The reason for such caution is partly that the weaker articles in this collection are quite poor. One expects diversity of viewpoint, style, and achievement; one finds all of that here, with the upsetting inclusion of inferior materials. All of the articles focus on governmental actions and their impacts, taking us rightly away from the older case studies that stressed the origins and enactment of policy. The subject is handled with largely American materials—except for Fried's comparative urban materials—and the discussion of substantive cases of American, foreign, and domestic policy is accomplished with a minimum of quantitative material and statistical reasoning. Within those constraints, our knowledge of many fields is enhanced. Sapolsky brings the reader the themes of science policy in a readable, interesting way, the very model of what a handbook should do. Bernard Cohen and Scott Harris on foreign policy, Robert Fried on urban policy, John Grumm on policy impacts—all bring together the political science research on these topics in an informed, scholarly, usefully summative way. Duane Lockard's article on race policy and Charles Gilbert's on welfare policy are also comprehensive.

The problems with the volume are twofold, only the first of which the editors could have easily remedied. That would have been to jettison the essay by Lassell (on the intellectual and appraisal 'functions' in policy research) and Pechman's account of the role of the economist in the making of economic policy. Not only are these the weakest chapters; they are also the first two.

Lassell's effort is simply out of place, a windy repetition of overarching generalizations written elsewhere about why sensible people should think hard about policy problems. It is practically unreadable, a claim supported by the frustrations of two graduate students assigned to review it. Pechman's essay is readable, but inappropriate in focus. Neither about the politics of economic policy making—a very important topic—nor the conflict of roles for the professional economist, the essay is a simplified account of Samuelson-level public finance and a memoir of the Council of Economic Advisors since the war. A number of political scientists could have treated the politics of economic policy—Robert Eyestone, James Andersen, Douglas Hibbs, for instance. And a number of economists write well about the politics of economic advice—Charles Schultze, Alice Rivlin, Sam Peltzman, to name just three.

But criticism of some of its parts would not do justice to the chief problem with this volume. It is in the aggregation of these articles as a scholarly handbook that one realizes how far we are from a paradigmatic science or even an accepted body of knowledge. The individual articles at their best provide good introductions to their subjects and excellent bibliographies of available works, but they do not constitute a coherent approach to "policies and policy making." Indeed, the subareas are defined by the "industry" studied, not by the political science approach to it. It would be as if an economics handbook brought together studies of trucking and income maintenance policies and failed to mention the distinctions among cash and in-kind programs, capital and human investment government efforts. Despite theoretical articles to open and close the volume, we are no closer to a paradigm for policy studies at the end. The articles do not add up to anything more than the sum of the parts. And that raises the question of why some areas were covered and others not: why were the military, business, tax, health, and environmental fields—which account for the bulk of any modern government's activity—sacrificed to the politics of science, urban life, and foreign policy?

One answer is suggested by the editors at the outset. Political scientists, they say, are early socialized into the view that their field is ill-defined, heterogeneous, and amorphous (p. iii). In this volume, art is imitation of reality, as amorphous heterogeneity triumphs over order to reflect ill-defined subjects. Were the standard of scientific advance held, the volume would be disappointing. As a collection of articles about political science and American public policy, the book contributes some helpful, substantial overviews.

THEODORE R. MARMOR

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Handbook of Political Science, Volume 7: Strategies of Inquiry. Edited by Fred I. Greenstein and Nelson W. Polsby. (Reading, Mass.: Addison-Wesley, 1975. Pp. 458. $13.95.)

The first two chapters of this edited volume cover, respectively, library sources and sources of quantitative data, the first by Clement Vose and the second by Jerome Clubb. Vose's objective is "to discuss some of the areas concerning the use of sources that political scientists are likely to find valuable," and to view in depth "a number of systems or sets of materials that are important for their own sake," in order "to develop principal points to be applied to other sets" (p. 2). He does this by reviewing for us various examples of errors in standard sources, and by enumerating various catalogs, indexes, encyclopedias, yearbooks, directories, and handbooks. He sensitizes the novice to factors such as the political and financial motives of authors and publishers and guides us through the maze of overabundant sources. I cannot resist noting, however, that Professor Vose provides three political science examples of how the Library of Congress Card Division often fails to list books under appropriate subject headings; his three examples include books by Greenstein, Polsby, and Vose. Whether intentional or not, this reflects the somewhat in-house flavor of the entire Handbook series.

Clubb's purpose is to provide a "minimal starting point" for the student interested in computer technology and supporting resources for social science research. He provides a discussion of all major data archives, followed by discussion of the operation and use of archives, archival development, sources of machine readable data, and the addresses of 39 social science data archives. Like Vose, he provides a useful if not startling chapter.

In chapter three, Harry Eckstein makes a strong argument for case studies as means of validating theories, rejecting the prevalent position in our discipline that case studies are useful to discover interesting questions but that theory can only be tested by using the comparative method. His case is presented clearly and persuasively, and his chapter is probably the best organized and readily intelligible of the eight in this volume. Professor Eckstein presents a careful introduction which defines his terms, a brief overview of the possible arguments regarding the relative role of case studies and comparative study, a somewhat longer discussion of various types of case studies, and then rebuts potential objections to his argument. His argument is a sophisticated one, as he distinguishes between ordinary case studies, which he does not necessarily encourage, and crucial case studies, which he sees as efficient alternatives to comparative studies, differing only in expense but no in logical status. Although examples of crucial case studies are few, Professor Eckstein delineates them well enough that careful readers will "know one when they see it." I highly recommend this chapter for use in all introductory graduate seminars in scope and methods; it is clearly written and provocative.

Chapter five is a plea for more experimentation in political research by Richard Brody and Charles Brownstein, while chapter six is an overview of the uses of survey research by Richard Boyd and Herbert Hyman. I find both chapters to be good overviews and cogently presented reviews of current work in these fields. I believe both are useful for introductory graduate seminars in research design, and invaluable for courses in experimental design or survey research, respectively.

Brody and Brownstein are advocates of simulation and experimentation. They provide a brief discussion of the place of experimental method in scientific research–its advantages in terms of controlling extraneous variables and of manipulating the independent variables–as well as presenting some of the details of experimental designs and of several applications in political science, particularly in international relations and political behavior. Their chapter is cogent and well argued, with just the right balance of overview and detail, of review and advocacy. Like all good advocates, Brody and Brownstein present the weaknesses of experimental methods–focusing mostly on external validity–and unlike most advocates, even good ones, they take these weaknesses seriously. The result is an excellent addition to the literature.

Boyd and Hyman address two general questions: "What types of belief and behavior can be measured with surveys?" and "What purposes do surveys serve?" Their intended audience includes those "examining the potential of survey methods for the first time." Their purpose is a general introduction to the uses of surveys, and a review of political science applications. Unlike Brody and Brownstein, they do not address carefully the objections to survey methods, such as the control of confounding variables and the problems of determining cause and effect, although they do review briefly problems such as the impact of question wording and the nonattitudes controversy. Given their purpose, however, they succeed for 40 pages and fail for 30 pages. The first half of the paper is a nice overview-introduction to the major studies that can—and have—been conducted using survey methods. Their unabashed enthusiasm for survey research is evident and ought to whet the appetite of even the most skeptical graduate student. Unfortunately the chapter ends with a whimper, because the last 30 pages contain a too-detailed history of survey research. Their purpose in this history is to emphasize the growth of survey research "in an environment that inextricably mixed policy and scholarly interests," but the detail is unnecessary to the point.

There are two technical chapters, chapter four on polimetrics by Hayward R. Alker, and chapter seven on formal theory by Gerald H. Kramer and Joseph E. Hertzberg. Alker has as his purpose "to help political statisticians to be more critically and creatively aware of the possibilities and limitations of measurement procedures," and to "enrich the analytical tools we can use to make qualitative and quantitative political descriptions." He assumes only one semester of social statistics, although he notes that for most people with that background, the material covered will be novel. Alker presents a brief overview of polimetrics, followed by four sections on representational measurement procedures, aggregative approaches to intentionalist assessment practices, situational and structural measures in intentional assessment, and the individual case in intentionalist assessment. The latter four sections consist mainly of a few general points and arguments followed by examples of recent works which happen to impress Hayward Alker.

My own reaction to Alker's effort is mixed, though more negative than positive. I for one would not assign this chapter to students (with one semester of statistics) in a graduate course in measurement. His examples are not detailed or clear enough to provide a good introduction to any of the topics he reviews. For example, two of his illustrations of aggregative approaches are the Carroll-Chang individual differences scaling and the Baas-Brown application of intensive factor analysis. Alker is unable to devote sufficient space to this discussion to provide comprehensible introductions to these topics. Unless one has read the original articles, one is unlikely to learn much from Alker's discussion, and if one has read them carefully, his discussion adds little. It is my own judgment that the chapter would have been more useful if Alker had written the entire article on his first topic, an overview of polimetrics. A general discussion of statistical theory and practice would be precisely the kind of overview most useful for the first year graduate student who has recently struggled through the incrementalism of introductory statistics. Alker's effort, I fear, is much too idiosyncratic to be useful for most of us.

The Kramer and Hertzberg chapter on formal theory is a competent nontechnical overview and introduction to political economy and game theory. Most of the discussion is cogent, even for the uninitiated, and the use of several extended examples helps clarify abstract points. My only comment is editorial: This chapter is about 45 pages long and introduces far too many complex concepts. The volume as a whole would have been better served by cutting Alker's 70-odd pages and Boyd and Hyman's 80-odd pages and by expanding Kramer and Hertzberg's discussion to allow elaboration of what is at present a dense presentation. The last chapter is by Herman Kahn on futurology.

JOHN SULLIVAN

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Handbook of Political Science, Volume 8: International Politics. Edited by Fred I. Greenstein and Nelson W. Polsby. (Reading, Mass.: Addison-Wesley, 1975. Pp. 450. $14.95.)

If volume 8 of the *Handbook of Political Science* is a faithful reflection and adequate analysis of the state of international relations within political science, its students have no reason to rejoice. For this collection of six essays by seven distinguished authors displays everything that is wrong about the field—along with some of its history and accomplishments.

A reader wandering in from outside would be bewildered by the fragmentation. One of the essays, the fine survey of national security affairs by Richard Smoke, deals largely with the evolution of American thinking and policies in the realms of strategy and arms control; the other essays place themselves at the systemic or global level of analysis. There is little coherence between, on the one hand, the overview of the world political system by George Quester, who focuses on power configurations and on threats to peace, and, on the other hand, the discrete studies of strategy (Smoke), of the dynamics of international economic interdependence and integration (Keohane and Nye), and of the difficulties faced by international law in a world as troubled as the present one (Leon Lipson). Moreover, large chunks remain outside: there is little on the historical sociology of, and political thought about, international politics, little on the relations between domestic politics and foreign policy, or on studies of the foreign policies of specific states or groups of states, little on international organization; there is almost nothing on foreign contributions to the field (in this respect, the volume is intensely parochial; it could at least have asked why the discipline has "taken off" so much more in the United States than elsewhere). There is a fragmentation of approaches as well, in two ways. One concerns the writing styles of the authors. Quester is discursive; others are incisively analytical (and, like Keohane and Nye, perform a most useful service in clarifying concepts and critically reviewing the literature in the field); Lipson's piece is really a think piece on the different intellectual perspectives about contemporary international law. More serious is the opposition between methodologies. Four of the essays are resolutely "literate" and traditional. One—the longest—by Dina Zinnes is a thorough survey of quantitative international politics (she prefers to call it the scientific study of international politics, thus showing once again that the champions of one particular set of methods not merely confuse science with it, but believe that they are the only ones willing "to move from the unique to the general" and committed "to search for patterns of association" between classes of events or types of entities: a rather arrogant annexation, unjustified by the meagre intellectual results of the effort, if not by the material and human resources devoted to such labors). The remaining essay (actually the first, by Kenneth Waltz) dismisses in a few sentences all the research so painstakingly reviewed by Professor Zinnes: for, says Waltz, it merely amasses "coefficients of correlation . . . without asking which theories lead one to expect what kind of a connection among which variables." Waltz, whose assignment was the theory of international politics, and whose criteria for theory are as rigorous and "scientific" as those set up by the self-styled "scientists" of quantitative methods, does not even consider their work! Obviously, all the fireworks of the 1960s about contending approaches have led to no rapprochement among the contenders.

An innocent reader would also, in all likelihood, be scared away from the field by the conspicuous absence of any broadly accepted conceptual framework, into which all these fragments could fit. This is true at two levels. At what might be called a constitutive level, there is no paradigm for international relations. The paradigm which prevails in the history of political thought, and in the works of most of the authors discussed by Waltz as well as by Waltz himself—the "realist" image of the state of war, or troubled peace, among sovereign units in a state of nature—is now under sharp attack, as the Keohane and Nye essay demonstrates. Quester's chapter does not even consider the issue, a central one for students of world politics. At the level of explanation and prediction, there is no satisfactory theory—this is the point made by Waltz in his ambitious essay with his customary mix of brilliance and perversity.

No student of international politics can afford to ignore Waltz's analysis. He shows why a theory cannot be derived inductively from the data which it is supposed to explain; he defines the steps which testing a theory requires; he states that "most theories of international relations are so imprecise that expectations of outcomes cannot be stated in ways that would make falsification possible"; he concludes that the only theory without which international politics cannot be understood is balance-of-power theory—but it does not tell us nearly enough. And yet, contemporary theorists have done no better; he proceeds to tell us why by skewering several of them (including me).

I have no quarrel with his skeptical findings, but I have two chief criticisms of his position (I will not dwell here on some trivial disagreements with his readings, or misreadings, of my own writings). First, concerning his definition of system, the rigorous distinction he makes between structure (defined as a "purely positional picture" of international society) and interacting units is far less helpful than he believes. He defines system change as the result of a major change in the distribution of capabilities among major actors; but this assumes a far more homogeneous and calculable conception of state power than is justified, and it leaves out important changes (such as the shift from the Old Regime to revolutionary France) which have their origin not in a shift in the distribution of capabilities but in the internal make-up of the units. Recent work by Nye and Keohane has shown the degree to which the outcomes of international processes cannot be explained by structure, as well as the complexity of "structure" once one ceases equating it with the distribution of military might. Waltz's relentless rejection of what he had earlier called "second image" explanations (what he now calls reductionist theories that search for causes within the units)—his desire to separate completely unit level and system level changes and to reduce the study of the international system to that of the relations between the structure and the interactions of the units—leads him, in the name of theoretical rigor, to leave out many of the most important aspects of the reality he wants theory to explain (he criticizes Morton Kaplan for conceiving of "subsystem dominance": it is the negation of a systems approach, says Waltz; but is it any more absurd than, in "domestic" political science, talking about a political system dominated by one of its components, such as a single party or a strong presidency?). Waltz may be right that definitions of the international system which are less exclusive than his own result in a loss of theoretical rigor. But his own approach makes him incapable of appreciating fully some of the highly original features of the present international system (the coexistence of military bipolarity and moderation, the effects of transnational relations).

Secondly, I suspect that Waltz demands of theory in international relations more than the field allows. He does not address himself to Raymond Aron's argument, in *Peace and War* (and several essays), about the impossibility of achieving a "scientific" theory of indeterminate behavior along the lines of economic theory: the concept of power differs from that of money; goal-oriented behavior differs from instrumental action; and while the permanence of a contest among units introduces elements of calculation and order, they do not suffice to provide the field with a rigorous theory. We can describe, in skeletal ways, the logic of state behavior, the main patterns of interaction and regulatory mechanisms, but making the field intelligible is not the same as disposing of an explanatory and predictive theory.

Leon Lipson ends his essay with advice to international lawyers to "curb their zeal, contract their horizons, and Think Small." The advice one might want to give to students of international politics, after one has read this volume, is more complex: on the one hand, Think Small, and try to find out more and to be more rigorously analytical about all the areas about which we still know, in fact, very little (this includes much of the past). But on the other hand, Think Large—not Big: try to understand the interrelations between functional or geographic areas, try to ask important questions about the field as a whole (those which traditional philosophers have asked, and for which endless collections of data and correlations are no substitutes); but forget about a master key or about the mirage of grand theory.

STANLEY HOFFMAN

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Community: A Critical Response. By Joseph R. Gusfield. (New York: Barnes & Noble, 1975. Pp. xvii + 120. $11.50.)

This book offers a comprehensive critical interpretation of the way in which the concepts of community and society are used as contrasting ideal types in sociological thought. It covers such central sociological themes as the relations between traditionalizing and modernizing processes in societies, the progressive and regressive aspects of urban-industrial societies, tribalism and nationalism as social processes, and finally communes and communitarianism as responses to mass society. These are difficult themes to discuss in a short book and Gusfield deserves congratulations for developing coherent and often original positions on these and other related issues. He is especially skillful in pinpointing ideological positions, including those disguised as value-neutral social science. While noting how ideologies sometime distort objective and subjective realities, he also pays sharp attention to their role in providing necessary dramatic and critical perspectives on social issues.

An important original contribution of the book lies in the way in which it delineates a model for thinking about communal and societal structures that views them as time and space bound resultants of human choices rather than as the fixed outcome of irreversable social trends. Using materials drawn from India, Gusfield shows how entire castes accomplish collective social mobility and thereby deploy communal resources to accomplish effectively what are usually conceived of as societal purposes. He analyzes the extensive literature on communal organization, stratification, and national development contrasting "systematic" theories about traditional resistance to modernization with a more complex and more accurate version of the multiple possible relations between these variables in diverse settings. American ethnic patterns are briefly considered and a remarkably flexible framework for thinking about associational life, class, and urban-industrial trends is set forth.

It appears ungrateful to ask more from a book which does so much to such good effect as does this one. My only wish would be to hear more from Gusfield about every topic he takes up. His thinking is broad and deep; all students of community at any level have much to learn from him. This book provides a model of concise, original, and fair discussion of a key concept in social science. Would that every concept we use could receive such excellent treatment.

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Public Goods and Public Welfare. By John G. Head. (Durham, N.C.: Duke University Press, 1974. Pp. ix + 291. $12.50.)

This monograph consists of 12 essays. Eight of them examine one or more aspects of the concept(s) of public goods, two discuss welfare economics, and two discuss Musgrave's concept of merit goods. The essays were written over the period 1962–1971. Eleven of the 12 essays originally appeared in European journals.

The chapters, with a few exceptions, are surveys of the literature Professor Head deems relevant to the topics examined. Footnotes are used generously. This feature makes the book useful for those who wish an introduction to the topics examined. Almost all contributions are patted on the head; the highest plaudits are reserved for James Buchanan, Richard Musgrave, and Paul Samuelson. That Buchanan happens to espouse a variety of political anarchy whereas the latter two economists are enthusiastic supporters of government intervention does not seem to bother the author.

Only after checking the original published papers did I learn that two chapters are reproductions of replies, written in response to criticisms (one by Harold Hochman and one by Charles McLure). Unless one reads the criticisms, the replies are difficult to appreciate. Either the comments of the two critics should have been reproduced or the replies should not have been included in the volume.

Of the three main topics, the welfare chapters are rather good commentaries. The two chapters on merit goods are not very useful. I agree with McLure that Head has misinterpreted Musgrave. The major portion of the work is devoted to the examination of concepts of public goods.

Head is one of many enthusiasts for the self-styled "modern" approach to public finance–a position that among other features places public goods as the main actor in the new scenario. The vast literature that has grown around this topic has a large payoff in promotions for academic economists; its payoff in terms of selecting and scaling public programs may be only slightly greater than zero. Head, like many others, has no trouble finding numerous illustrations of public goods. The ploy here is to list supposed illustrations, with little or no justification. But he has almost nothing to say about how actual public programs should be selected and scaled. There are promising applications of the public-good approach to local government as illustrated by the works of Trebout, McGuire, and Oates, among others. Head did not choose, however, in the present essays to push very deeply into that literature.

Viewed as surveys, Professor Head's study has considerable merit especially but not only for novices in the fields covered. Many of the deficiencies of the book could have been remedied by rewriting the papers and by eliminating duplication. The excess verbiage seriously handicaps the reader in trying to understand Head's message. Nevertheless if one perseveres, there are many worthwhile observations to be found.

EARL R. ROLPH

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On Liberty and Liberalism: The Case of John Stuart Mill. By Gertrude Himmelfarb. (New York: Alfred A. Knopf, 1974. Pp. xxiii + 345. $8.95.)

Among the great number of recent writings on Mill, this book is distinguished by its originality, and even where there is disagreement, one is refreshed by an encounter with an informed and provocative and politically relevant argument.

Professor Himmelfarb argues that there are two incompatible sides to John Stuart Mill's thought, one found in the essay *On Liberty* (and in a few related writings on the women question) and the other, reflecting intellectual traditions that are incompatible with the doctrine of the essay *On Liberty*, which is found in most of Mill's other writings, whether they were early or post-1859, whether primarily reflecting Benthamite teachings or written during the period of rebellion against Benthamism. Thus we have the more widely known Mill of *On Liberty* and the "other Mill." The doctrine of *On Liberty*, it is argued, is fundamentally radical. Despite superficial similarities to traditional understandings of liberty, it was radical even for Mill, who in his other writings "represents a quite different mode of liberal thought" (p. xi). The other Mill "belongs to an older liberal tradition, the tradition of Montesquieu, Burke, the Founding Fathers, and Tocqueville" (p. xxi). So different are the two traditions that one becomes the basis of criticism of the other; indeed, the "other Mill" is the best critic of *On Liberty*. The implication (drawn out at the end of the book) is that the "other Mill" would disapprove of many developments that have taken place in our time which have been justified by arguments found in the essay *On Liberty*.

What, it should be asked, are the distinguishing characteristics that give the essay *On Liberty* its radicalism and, in the context of Mill's thought, its uniqueness? First of all, our attention is directed to Mill's defense of what he called "one very simple principle" that should "govern absolutely" the dealings of society with the individual with regard to compulsion and control; this principle provided that interference in the liberty of action of any individual can only be justified by self-protection. The emphasis, Himmelfarb notes, is on the single-mindedness of the doctrine—and she points to Mill's use of such terms as "one," "sole," "only," and "absolute" (pp. 14–15). The consequence is to set up an opposition between the individual and society and to make the claim of the individual absolute and uncompromising, whereas society is left unprotected as its claims on the obligations of individuals are greatly diminished. In addition, it is noted that for Mill the obstacles to liberty were not the traditional ones located in an oppressive political and legal system; on the contrary, public opinion, custom and tradition, religion and moral beliefs were regarded as the instruments of oppression. Thus society was the individual's chief antagonist, and as a consequence, the doctrine encouraged deep distrust of society, a contempt for convention, and extreme anti-authoritarianism. Furthermore, Himmelfarb notes that the diverse society that Mill wanted, which maximized spontaneity, individuality, and "experiments in living," also encouraged an unhealthy isolation. And by not providing support for morality, which usually came from social affections and public authority, a diversity in conduct is allowed to be so great that there is a problem of social cohesion.

The history of political philosophy contains many theoretical positions that would be critical of this understanding of liberty. But Himmelfarb argues that the "other Mill" offers the best critique of this view of liberty. Suggesting that we too easily assume a compatibility between the doctrine of liberty presented in 1859 and his comparable pronouncements in other writings, she has systematically made this comparison. She finds that whereas the Mill of *On Liberty* absolutizes liberty and defends it in terms of a single principle, the "other Mill" is part of a tradition "that can accommodate liberty together with such other values as justice, virtue, community, tradition, prudence, and moderation" (p. xxii).

Himmelfarb reminds us that, far from welcoming fully free expression, the "other Mill" held that permanent political society required allegiance; he said that there had to be "*something* which is settled, something permanent and not to be called in question" (p. 46). Mill was quite explicit: the core idea for the state had to be "*above* discussion"; there had to be "some fixed point; something which men agreed in holding sacred" (p. 46). These passages (appearing in "Coleridge" [1840] and repeated in *A System of Logic*) are a far cry from the essay *On Liberty* where even the sacred is held subject to completely free discussion. In addition, instead of celebrating the ideal of autonomy and self-determination, the "other Mill" often emphasized the need to give up the exercise of private judgment as one deferred to the authority of an educated elite. Furthermore, far from idealizing a diverse society, there is evidence that Mill thought extreme diversity contained the germ of civil war (pp. 46–47).

Himmelfarb also points to differences between the two Mills on man's capacity for virtue. Whereas *On Liberty* reveals no concern that fully free expression might lead to a proliferation of amoral and immoral conduct, elsewhere Mill was worried that by acting on passions, without regard for moral rules, people would behave immorally and would not perform their obligations to others. Whereas in the essay *On Liberty* conscience was an obstacle to individuality and freedom, the "other Mill" advocated education for the individual so as to produce habits "of subordinating his personal impulses and aims to what were considered the ends of society; of adhering, against all temptation, to the course of conduct which those ends prescribed; of controlling in himself all the feelings which were liable to militate against those ends" (p. 78). Here Mill called for a "restraining discipline" that is contrasted by Himmelfarb to the words and spirit of the essay *On Liberty*. Furthermore, such self-control was not only a matter of education and internalized norms; it was to be supported by the resources of society—laws and institutions and education and public opinion (pp. 73, 78, 106–07).

So much for Himmelfarb's claim that there are two Mills. Is it correct? In view of Himmelfarb's impressive marshalling of evidence, we may conclude that there are important differences between *On Liberty* and other parts of Mill's writings. However, while there are differences, the two Mills are not as separate as she makes out, for some of the argument of *On Liberty* may be found in that portion of Mill's thinking that was a by-product of St. Simonian influences—a part, according to Himmelfarb, that was an essential ingredient in the "other Mill." There is evidence of this in Mill's discussion of transitional eras—periods of history characterized by great intellectual diversity and therefore by the need for liberty. During transitional eras old doctrines had become obsolete, new ones had not yet been acquired. Authority and deference ceased to exist, and the various elites (intellectual, religious, political) disagreed and came into open conflict; and this led to the erosion of the authority on which the social system depended. During transitional eras, Mill agreed, liberty had two important functions. The first was negative, and it is exemplified by the Benthamite use of liberty, which was to discredit, undermine, and even destroy old doctrines and institutions that were vulnerable to rational criticism. The second function of free discussion was to discover and diffuse true opinions; in other words, to provide for the competitive struggle of the varied creeds that were in conflict during the intellectually and morally chaotic transitional period. Out of such rivalry would emerge a new doctrine that would make possible an escape from the unsatisfactory transitional era. This way of understanding liberty was different from the rationale for liberty that prevailed among either traditional constitutional theorists or the Benthamites, both of whom thought of liberty as a means of informing public opinion and providing security against abuse of political and legal power. And one can see Mill's speculations about the function of liberty in transitional eras as anticipations of that part of *On Liberty* that was concerned with liberty as a way of discovering new truths and improved social arrangements.

This understanding of the function of liberty during transitional eras was not confined to the brief but important early period when Mill was most intensely interested in St. Simonian ideas. It formed the immediate background to his thinking during the 1830s and '40s when he cultivated an eclecticism that was intended to comprehend diverse versions of the truth; after encountering one another, these versions were to be synthesized into a yet more comprehensive understanding. With this outlook Mill evaluated and sifted the "half-truths" from both Bentham and Coleridge, democracy and elitism, empiricism and deduction, laissez-faire and socialism. The evidence of his attempts to synthesize such partial views into a new philosophy is to be found in many of his essays, in the *Political Economy*, and in the *System of Logic*. And this road to the truth assumed an understanding of the function of liberty that originated during the St. Simonian period (1829–31) and carried over to the time when he wrote *On Liberty*. In other words, it was a view of liberty shared by the Mill who wrote *On Liberty* and the "other Mill." Thus it may be concluded that either of the "two Mills," despite their differences, would not have regarded the other as alien.

There is another part of this book that should be mentioned. Himmelfarb argues persuasively that Harriet Taylor played an important part in the inspiration and in formulating the ideas for the essay *On Liberty*. For Himmelfarb this explains how an alien ingredient was introduced into the body of Mill's work. But acknowledging that Harriet had a large role does not require that we agree that the two Mills were separate. For the comprehensive framework of Mill's thought, especially that part of it that had been shaped by the ideas of the St. Simonians and by German romanticism, included a place where her ideas could fit, not as the result of intrusion but as a matter of affinity. In passing it should be said that Harriet's ideas are seriously analyzed in this book for the first time.

In the last section of her book, Himmelfarb points to the dangers of Mill's doctrines in *On Liberty*. She argues that he does not adequately provide for principled limits to the assertion of liberty, so that there is no boundary to the extension of liberty, even if it carries one to such revolutionary lengths as to undermine the social basis of liberty itself, to say nothing of other values. In other words, liberty is absolutized, making it impossible to defend any kind of morality or authority. She also argues that this absolute liberty allows for the exploration of not only man's higher nature but also his capacity to be depraved. And she points out that Mill's doctrine justifies a moral revolution which, although it has been under way for more than half a century, has become visible only in our time when it has become democratized.

Without disagreeing with this analysis, one may dispute Himmelfarb's view that since the two Mills are quite separate and incompatible, the "other Mill" is the best critic of the doctrine of *On Liberty*. She holds that the "other Mill's" ideas reflected an older tradition that recognized the multiplicity of ends and therefore the need to prudently combine them, so that liberty will be balanced against concerns for morality, stability, fairness, and community. In other words, the "other Mill's" ideas reflected the "Whiggish mode of liberalism" that "prized liberty as one of several virtues, that had a healthy respect for social institutions and conventions, that was willing to use the resources of society and the state to further the public good, and that above all distrusted the application of simple formulas to complicated affairs" (p. 299).

The difficulty here is that in the thinking of the "other Mill" there were ingredients that were incompatible with the "Whiggish mode of liberalism." First, there was his early Benthamism, which, with its Hobbesian origins, established an opposition between liberty and law that is incompatible with an understanding of liberty that allows for its prudent combination with morality, stability, and authority. In addition, the "other Mill," because he had incorporated St. Simonian ideas, was committed to a philosophy of history and a doctrine of progress that encouraged the visualization of vast changes which, though not involving physical violence, were still revolutionary and therefore inimical to the older Whiggish mode of liberalism—which was distinguished by a concern with gradualism, continuity, and moderation. Furthermore, the "other Mill" consistently expressed irritability with his own country's inclination to compromise; yet such compromise was at the heart of the older Whiggish tradition, whether found in Burke's observations on prudence, or in more traditional defenses of moderation, or in eulogies of British practice. In fact, the "other Mill," having confessed to "youthful fanaticism," rejected Whiggish moderation. Indeed, much of his political activity during the 1830s was a sectarian pursuit of doctrinaire goals, and, it should be added, throughout this period he was bitterly hostile to the Whig party and to the idea of Whiggism. And finally, as symptomatic of these themes, there was Mill's consistent criticism of those who were spokesmen for traditional Whiggism. Consequently, Mill was not likely to be an adequate spokesman for the Whig critique of *On Liberty*. Indeed, since the doctrine of *On Liberty* in important ways originated in the ideas of the other (and early) Mill, we should not expect to find an adequate basis for evaluating *On Liberty* in those ideas.

Learned and original, this book is a notable contribution to Mill scholarship. Despite one's disagreements with parts of it, the book is welcome, not only for the way it has stimulated healthy controversy and exploration, but also because it exemplifies how investigations in history and political philosophy can be relevant to our politics.

JOSEPH HAMBURGER

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Organized Social Complexity: Challenge to Politics and Policy. Edited by Todd R. La Porte. (Princeton: Princeton University Press, 1975. Pp. ix + 356. $16.50.)

During the 20 year period from 1948 to 1968, a number of articles were written on the general topic of complexity that have become interdisciplinary classics. The term "classic" should not be taken simply as hyperbole. Their authors are a distinguished lot—Christopher Alexander, Albert Ando, Franklin Fisher, G. A. Miller, Herbert Simon, John Platt, and Warren Weaver among others—and in each case their examinations of complexity proved to be characteristically insightful and contained profound, if as yet commonly unrecognized, implications for model building, data analysis, and theory construction in the social sciences. An attempt to integrate, criticize, and extend this body of work is long overdue, and it is a credit to Todd La Porte and the other contributors that they have attempted to remedy the situation.

This volume consists of eight loosely related papers on organized social complexity plus an introductory and concluding chapter by the editor. In the Preface, La Porte tells us that the book grew out of a small seminar at Berkeley that had been convened to study the topic. Six of the chapters are revisions of papers prepared for that seminar; two others, those by Garry Brewer and Daniel Metlay, were added later. For the most part, the chapters reflect their origin. The majority of those which were generated in the seminar are self-consciously discursive and are primarily interested in raising provocative questions and airing assorted speculations. Given the importance of familiarizing more political scientists with the many issues involved this may be a wise strategy and, collectively, the articles should generate reflection on the part of individuals adhering to a variety of methodological and theoretical persuasions.

It should be noted, however, that occasionally this discursive approach tends to sacrifice some of the theoretical and prescriptive benefits that might have accrued if the authors had chosen to take a closer look at some of the verbal and mathematical metaphors they employ and points that they raise. One example of this is the La Porte-Metlay treatment of complexity in the introductory chapter and its technical appendix. Central to this discussion in the statement that the complexity of an organized social system is a "function of the number of system components, the relative differentiation or variety of these components, and the degree of interdependence among these components" (p. 6). Unfortunately, this definition and the sociometric exchange theory terminology in which it is embedded, make it unclear whether the degree of interdependence refers to the number of interconnections, the pattern which they form, or the process of information and resource flow. A focus on the latter, taking off from Simon's "The Architecture of Complexity," would probably have proved the most fruitful but it is the aspect of interdependence least developed here.

La Porte and Metlay proceed to explore system complexity solely on the basis of an influence graph and its matrix representation. The principal drawback of this approach is that it permits only limited inferences about the nature and impact of complexity since it considers the existence or nonexistence of interconnections but not their character (nonlinearity, instability, etc.) or the processes that they represent.

In addition, there also appear to be a number of technical problems. To be more specific: On a technical level much is made of the fact that complex systems frequently take the form of "semi-lattices." However, such a mathematical structure possesses a number of formal properties (e.g., that there exists a least upper bound for every two elements) which are not shared by those graphs to which the term is applied here (e.g., Figure 1.3 on page 9). Nor is it true that a complete lattice is equivalent to a complete and symmetric diagraph, as is claimed on page 9. Along the same lines, the statement "If the ones in the matrix are replaced by numbers representing, in some sense, the weights of influence, the rank will either increase or remain the same" (p. 33, fn. 37) is false for matrices larger than 3 x 3.

Also suspect is Metlay's assertion that rank of an adjacency matrix can be used to represent the degree of system complexity. Rank is a property of linear transformation and can only be applied meaningfully to a matrix that represents one. It is not clear that an adjacency matrix of a diagraph has any such relationship to a linear transformation. Further, rank fails to have the properties that Metlay himself (pp. 26–27) claims to be desirable for a measure of complexity. Rank is not a monotonic function of the number of units, the degree of differentiation (whatever that is), and the number of interconnections; and it is not inversely related to decomposability. Individually such points may seem relatively insignificant but collectively they make the reader skeptical of how successful the authors have been in their attempt to overcome the formidable difficulties involved in relating mathematical models to social phenomena.

More serious are the theoretical and conceptual limitations that stem from the decision to depict complexity and investigate its properties with a simple influence graph and its matrix equivalent (an adjacency matrix of ones and zeros). Aside from the fact that such a representation seems in danger of robbing complexity of its fundamental property of nonsimple interaction among elements, the problem with this approach is that the elements in the matrix describe states instead of processes. This is a problem because a number of authors want to interpret successive powers of such a matrix as future states of the system in order to study the impact of complexity over time and the manner in which a complex system evolves. Yet because the original elements of the matrix contain no process data, as in an authentic input-output or Markov model, higher powers of the matrix yield no such information. In fact, it is doubtful that the resultant matrix has any interpretation other than an esoteric combinatorial one.

Naturally, such comments do not accurately characterize each of the eight chapters. There are a number of thoughtful observations in virtually all of them, particularly those by Taylor and Brewer, and this book would be a useful addition to the reading lists of a variety of graduate courses ranging from political theory to model building. On the other hand, the observations above have not been made gratuitously. All too frequently in these chapters a lack of focus and development has obscured the insights that are present and postponed the discovery of others.

GEORGE DOWNS

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Percepts, Concepts and Theoretic Knowledge: A Study in Epistemology. By Harold N. Lee. (Memphis, Tenn.: Memphis State University Press, 1973. Pp. 257. $10.50.)

The idea of "the furniture of the world" epitomizes a fundamental misconception according to Professor Lee, namely, the notion that the world is constituted of discrete, static, enduring objects. These static objects, as conceived by the erroneous view, become the subject matter of knowledge in everyday experience and science, and their existence and character is independent of human activities involved in the attainment of knowledge. Lee finds this error at the heart of numerous misguided efforts in philosophy and science. The error is found in Descartes' metaphysical perspective; it generates mind-body dualism and solipsistic problems like the problem of the external world and is presupposed in phenomenalist epistemologies like sense-datum theories. The same mistake encourages the idea that science is passively descriptive rather than interpretive, that contemplation is the highest form of knowledge, and it even lurks in dispositional accounts of belief and in behaviorism. Borrowing "ready-made" from Lee's many descriptions of this focal error, we can call it the fallacy of ready-made objects.

Lee avoids all these consequences of the alleged fallacy with an epistemological system ("a conceptual scheme or categorical scheme" in his terminology) based on the metaphysics of process. Events, change, flux, ongoing activity, these are fundamental, while objects along with concepts and generalities come into our thinking only as a consequence of divisions and selections made in the seamless continuum of process in the course of human cognitive activities.

The development of all this is a large undertaking, and Lee tells us in his introduction that the undertaking is speculative. Academic philosophy today tends to deploy scholastic techniques in arid and narrowly conceived investigations, and we are deficient in projects of great scope and imagination, so a work like Lee's is welcome. Speculative though it is, however, it is not wholly satisfying and that is because Lee relies on the mere repetition of general metaphysical formulas and falls into a shallow honorific use of the rhetoric of flux and continuity on many of his main points. The attractions of process philosophy have been known to philosophers since the time of Heraclitus, and since that time process metaphysics has seemed to carry with it its own paradoxes.

The single greatest fault of Lee's work is its failure to offer any substantial guidance concerning these problems. For example, Lee presents a naturalistic account of mind as coming to exist within the totality of process, a totality that also includes the ultimate constituents of the world that minds are conscious of. In this, his aspiration is widely shared. It amounts to the effort to close the gap between mind and nature. But what he says on this wonderful theme comes down to the claim that everything will be all right in the philosophy of mind if we think in terms of continuity and flux rather than of static ready-made objects.

Stretches of the primordial flux are called "episodes," and episodes interact with one another. "Sensitivity" is measured by the degree to which an episode "responds" to its environment of other episodes. Then somehow "experience" is generated, although presumably at this primitive level it is not the experience of anyone. "Experience is the result of participation, or being part of the flux. Hence those episodes that are more active and sensitive constitute a richer and more varied experience than those that are less active and sensitive" (p. 51). To go along with Lee here, we have to accept the idea of flux without raising the question, What is in flux? And then we are led, mostly by means of gradual shifts in vocabulary, from the mere idea of a flux to "response," "sensitivity," and "experience" and, thence, to "proto-generalization," "perception," and "consciousness."

We can certainly endorse Lee's conviction that the philosophy of mind ought to be naturalistic and his critical claim that the modern empiricist tradition has not provided an adequate understanding of mentality. Though his view is in the right spirit, however, it does not really give us anything definite enough to be called an articulated philosophy of mind. The same mysteries that beset the idea of consciousness in the epistemologies that commit the ready-made object fallacy will reappear in Lee's process metaphysics. Why are we entitled to think of one part of the flux as "responding" to another? How does an episode "participate" in the flux, rather than merely being part of it? Why do interactions deserve the title "sensitivity"? These questions raise the same difficulties as the question, framed in the metaphysics of objects, "How can causal interactions between the nervous system and outer things eventuate in conscious perception of those things?"

Lee gives us no guidance on these matters and we need much more to judge his account of mind than the loose metaphorical story he tells. Again, we are repeatedly warned that the existence of objects is never independent of our own cognitive activities. By way of illustration, Lee discusses the existence of subatomic particles (pp. 207–08), which is a very easy case for a philosopher who wants to show theory-dependence in ontological commitments. Lee does not try to rationalize his view that less obviously theoretical objects are theory-dependent too. Is the earth really theory-dependent? Does it really owe its discreteness to human cognition? An explanation is owed the reader here. Without it we are free to think, as indeed we do think, that some of the furniture of the world does exist without reliance on our cognition and, if that is so, then the ready-made object fallacy is not a fallacy after all.

ARTHUR COLLINS

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The Pseudo-Science of B. F. Skinner. By Tibor R. Machan. (New Rochelle, N.Y.: Arlington House Publishers, 1974. Pp. 224. $9.95.)

This book brilliantly fills the need for cerebral dialogue on the ideas of Burrhus Frederic Skinner—a need which derives a little from the visceral verbiage that emits from both those who agree and disagree with Skinner and very much from his premises and their profoundly dangerous implications. The premises include the easy manipulability and the rightness of using most people as instruments of the purposes of a few people. These premises have been shared by those dedicated to the service of a church or God (like John Calvin and the Spanish Inquisitors), those dedicated to the service of the state or society or humanity (like Hitler, Marx, Lenin, and Stalin), those who would have most people subserve the ends of a few gifted humans (like Nietzsche, Ayn Rand, and other "Renaissance" individuals who argue for the glorious fulfillment of talented individuals and neglect the cost of this fulfillment in the exploitation of untalented individuals).

Machan's book penetrates to these premises as they prevail in Skinnerism, with a deep clarity, intelligence, and open-mindedness that contrast markedly with the superficial clarity and deep muddiness of Skinner's writing. Perhaps because he denies the existence of consciousness, Skinner seems to be unconscious of the implications of his writings. Perhaps because he makes consciousness central to his critique, Machan makes the implications clear. But then Machan shows his own lack of awareness of the implications of his own values.

Skinner argues that human beings can and should be manipulated to ensure the survival of culture. Just what aspects of culture merit survival are not clear because Skinner innocently maintains that his moral standards are morally neutral and scientific. Presumably the culture he wants to survive is not the street culture of theft, mayhem, rape, murder, and assassination. But Skinner does not say.

Machan argues that indeed humans in some circumstances need conditioning. But the ends that Machan specifies are not abstractions like divine will, humanity, the state, or even a social class. Rather, they are the ends of the individual—more exactly, it appears, the extraordinary individual, and, more particularly, the economic entrepreneur.

Both Skinner and Machan, so considered, emerge as elitists. Skinner is the scientific elitist, basing his advocacy of human manipulation on the supreme value of maintaining culture, and making stimulus-response psychologists the manipulators. Machan is the Renaissance elitist, justifying the subservience of society to gifted individuals on the implicit grounds that individual humans are the only inherently valuable entity. Contrary to Kant, he does not consider whether every human individual, even if ungifted, is an entity of inherent value. He quotes Ayn Rand at least twice for saying (p. 177) that "individual rights are the means of subordinating society to moral law." Taken at face value, this statement sounds like the highest morality. Taken at what Skinner would call the operant level, it means that those humans who are not extraordinarily gifted are to be used as instruments of those who are.

What both Skinner and Machan have unwittingly neglected is one very basic aspect of human nature and a basic moral derivation from it. All people wish to be regarded in some ways as equal to all other individuals. This demand for equality in worth, for dignity, is so universal that it can only be deemed part of human nature. People do not in all ways want to be subordinated to the purposes of others, even to elitists who rationalize their ends in moral or scientific terms. The morality of Christ and Kant is based on the inherent value of each individual, regardless of ordinariness or extraordinariness. The morality of Marx is based on the inherent value of each ordinary individual. The morality of Nietzsche, Rand, and Machan is based on the inherent value of each extraordinary individual.

There does not seem to be much fundamental difference between the ideologies housing those who would make people altogether into instruments of some "higher" good, whether it be the race, the state, society, a class, or even humanity and the ideologies housing those who would make ordinary people subserve the purposes of extraordinary people, whether psychologists or entrepreneurs. Maybe the latter are more candid.

A plague on both their houses. Both kinds of elitists need the positive reinforcement of reflecting on the teachings of Christ, Kant, and the Marx of *The German Ideology*. If Skinner and Machan were voluntarily to condition themselves thus, they would find that they like themselves and others better, but also that others liked their ideas better for having nonparochial foundations. For this lack of arrogance and for making the individual the valued entity, Machan is more likable than Skinner. For their elitism, both are equally likable to elitists.

JAMES CHOWNING DAVIES

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From Affluence to Praxis: Philosophy and Social Criticism. By Mihailo Markovic. (Ann Arbor: University of Michigan Press, 1974. Pp. xviii + 265. $12.95, cloth; $3.95, paper.)

"I find myself in profound disagreement with most of the people who describe themselves as Marxists" (p. xiii). The Yugoslav philosopher Mihailo Markovic, however, who makes this statement in the preface to his book *From Affluence to Praxis,* is just that, namely a Marxist. Belonging to the prominent *Praxis* group, so-called after their famous and, since 1975, by party decree prohibited journal, Markovic's future is somewhat in jeopardy. *Praxis* friends of his at the University of Belgrade have been fired for ideological reasons. Reading, therefore, Markovic's book, which is based on earlier essays and lectures given at the University of Michigan in 1969/70, turns out to be an introduction into the vicissitudes of intellectual survival in Yugoslavia.

Having been an active member in the Yugoslav resistance movement during the German occupation of Yugoslavia, Markovic, however, is familiar with the lifestyle of a partisan in the literal and figurative sense of the word. Even though he does not refer to these personal experiences—Erich Fromm provides the reader with the biographical information about the author in his sympathetic foreword—the figurative meaning of the partisan circumscribes beautifully the thrust of his Marxist humanism.

Markovic retraces his humanism to the early writings of Marx. Here he finds, like so many other intellectuals on the Left in our century, the genuine spirit of a revolutionary *Weltanschauung.* Without trying to divorce a 'young' from an 'old' Marx he, nevertheless, blames the ideologues of the Communist movement for having reified the 'old' Marx and, thereby, making it impossible to invoke the radical criticism of the 'young' Marx against Marxist dogmatism and orthodoxy. The Belgrade philosopher wants to overcome reified Marxism, to restitute the basic activism of Marx's thought, and then to live the way of praxis: "A philosopher who intends to change the world, not merely to interpret it, does not stop at reaching reliable knowledge of what is. He also examines the realistic possibilities of what might be and engages in the practical realization of those possibilities which best correspond to certain objective human needs. A revolutionary social philosophy is, therefore, both knowledge and evaluation, discovery of general truths, and a radical social criticism" (p. 22).

Markovic's understanding of social philosophy is not only directed against the reified Marxism of the bureaucratized societies in Communist Europe. Being a Marxist at heart he has neither forsaken his belief in the possibility of a revolutionary future, nor has he become a fellow traveller of liberal democracy in the capitalist West. His critical analysis of Western societies becomes merciless when he discusses the ideological premises of technological economism, the economism that led to affluence: "The growth of technology for its own sake is meaningless and dangerous. If the technostructure has any philosophy, it is a philosophy of complete nihilism, and of complete social irresponsibility" (p. 98). The social sciences in the West do not challenge this abysmal situation since they are dominated by positivism," . . . according to which the sole function of science is to describe and explain what there is, and, if at least some laws are known, to extrapolate what might probably be. All evaluation in terms of needs, feelings, ideals or ethical, aesthetic, and other standards—is considered basically irrational and, from the scientific point of view, pointless" (p. 48ff). At best, this neutrality amounts to the silent affirmation of the status quo, at its worst to the open collaboration of science with power.

Markovic's belief in the possibility of a more humane future has survived the experiences of inhumanity in the twentieth century. But he is the opposite of a true believer when articulating inhumane truths: "The experiences of the twentieth century . . . do not give us reason to believe that evil in man exists only in the sphere of 'facticity' and only in the time which precedes genuine human history. . . . The scope and character of bloodshed and mass madness . . . can no longer be explained by the romantic, dualistic picture of a latent positive essence and a transient bad appearance. Evil as a human disposition must lie very deep" (p. 219). Existentialist-sounding passages like these, which one encounters in the writings of most members of the Praxis group, add persuasive honesty to Markovic's book, even though they don't alter his Marxist convictions.

These convictions are best exemplified when he uses the mixed Yugoslav system of central bureaucracy and organizational self-management to illustrate the future possibilities and present deficiencies of a post-capitalist and post-bureaucratic society. Yugoslavia is not Paradise regained, but discussed as one concrete historical possibility among others. "All possibilities are open in logic, but not in history. Human nature is not an abstract, fixed, transcendental entity. But neither is it something which can be created by any arbitrary decision of a free individual. In each historical epoch there is a general structure of human being, as a crystallization of the whole past history of human praxis" (p. 209).

The moderate historical determinism of these lines becomes only occasionally threatened when Markovic is surfing on the wave of an ideologically predestined future. Then, when for example attempting to justify truly revolutionary Russia against the Stalinist aberration, he sounds somewhat shrill and out of step with the general tone of the book (p. 158). The overall impression, however, lets one forget these rather superfluous exercises in ideological rhetoric.

Less excusable in this book are the absence of a name index and the sloppy editing. To make Campanella (1568–1639) a contemporary of the eighteenth century (p. 211), to attribute to Hegel a nonexisting book, entitled *Philosophie des Geistes* (p. 123), and to misspell the Italian Marxist Antonio Gramsci persistently as Gramshi is bad enough, but nothing compared to the atrocious spelling of German titles and quotations in the footnotes.

MANFRED HENNINGSEN

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The Political Thought of William of Ockham: Personal and Institutional Principles. By Arthur Stephen McGrade. (New York: Cambridge University Press, 1974. Pp. xiii + 269. $18.50.)

The present monograph constitutes a valuable addition to the growing body of literature on the political thought of William of Ockham. It is all the more welcome as the spectrum of scholarly opinion regarding the matter at hand is unusually broad and varied. Ockham has been alternately described as a moderate and an extremist, a conservative and a brash innovator, a papalist and a conciliarist, an individualist and a constitutionalist, an Erastian and a theological positivist. Some have even questioned whether he had anything like a coherent political theory or, if he did, whether it bore any relationship to his nominalist principles. Setting his own independent course through these muddied waters, McGrade offers a synthesis of Ockham's thought that does justice to and reconciles all or most of its apparently contradictory strands. The book is divided into two uneven parts, the first of which deals with Ockham's expressly political views, and the second with the elements of his academic philosophy and theology that may be deemed relevant to those views.

The Ockham presented in these pages is not a skeptic or a mere critic of existing institutions but a constructive thinker to whom may be traced some of the most innovative ideas of the time. His overriding concern was with the dignity and freedom of the individual, whose rights he defended among other ways by insisting that no one could be corrected without first being shown his error (cf. pp. 48–63). Although he was not the only theologian of his generation to hold that the head of the Roman church could lapse into heresy, he was the first to devise a process of doctrinal correction whereby, once convicted, an erring and pertinacious pope could be brought to heel. On the institutional side his attacks were pitched for the most part at the hierocratic conception of society which claimed for the pope the fullness of spiritual as well as temporal power. Yet he never challenged the fundamental assumptions of medieval society. His sole intention was to explore new ways in which the harmonious cooperation of the spiritual and the lay powers could be secured. The position that he advocated in that regard is characterized as a balanced dualism. Others before him (e.g., Thomas Aquinas and John of Paris) had rejected the monism of the curialists, but Ockham went further than all of them in calling for a disengagement of the two powers, a disengagement that he sought to promote by emphasizing the spiritual nature of the church and simultaneously limiting the sphere of governmental activity to the punishment of wrongdoers.

Although consistent with his nominalism, Ockham's politics cannot be said to be deduced from it. His own proposals for the reorganization of society, both ecclesiastical and lay, were defended on grounds of practicality rather than of theoretical necessity. This is not to be taken to mean that he was an unrestrained voluntarist or a proponent of reason of state politics. As McGrade rightly points out (pp. 189–96), he fully acknowledged the role of reason in human affairs and did not doubt for one moment that moral conduct should be governed by objective principles. One wonders, however, to what extent the argument in this instance goes to the root of the problem. The basic issue is not whether Ockham admitted the existence of nonsubjective norms of morality but whether these norms are ultimately founded on anything other than an act of God's free will. In an effort to preserve the divine omnipotence against the encroachments of philosophical necessitarianism, Ockham reduced the natural law in the strictest sense to a single negative precept prohibiting the hatred of God. He was thereby led to interpret differently the classic question of the dispensability of the precepts of the Decalogue. While in many particulars his theory resembles the older view, the spirit that informs it can hardly be regarded as traditional. Similar reservations may be voiced in regard to the significance attached to Ockham's threefold division of natural right into absolute, nonabsolute ideal, and suppositional. By referring to this so-called suppositional right as "natural," Ockham has invested it with a higher dignity than it might otherwise be thought to possess. Though less conservative than that of the political Augustinians of his age, his position may thus be more conservative than that of Augustine himself.

For want of space McGrade has deliberately avoided lengthy comparisons with other writers. The occasional references to Marsilius of Padua and Dante, helpful as they may be, betray a lack of awareness of the element of accommodation in the works of both of these authors. In view of the role that he assigns to the "stronger or superior part of the whole citizen body," it is doubtful whether Marsilius can be considered a champion of popular sovereignty (cf. pp. 62, 83, 109, 123, 214, 226). Dante's views on the subject of Christianity and its relation to civil society may likewise be considerably more radical than his own generally respectful statements would seem to suggest (cf. pp. 84, 99, 217). A larger question arises concerning the "political" character of Ockham's writings. That, as a result of his involvement with Ludwig of Bavaria and of his own quarrels with three successive popes, Ockham developed a passionate interest in politics during the last 20 years of his life is undeniable. There is a sense, however, in which even the works of this period may be said to have been somewhat less than political. Ockham's success would undoubtedly have been greater had he been less outspoken and especially less bitterly polemical in the public utterance of certain legitimate but potentially disruptive ideas.

The foregoing strictures are not meant to detract from the merits of a book that abounds in illuminating insights and pointed formulations. McGrade's essay not only provides a competent and remarkably lucid account of the politics of this "most formidably technical of the great scholastics" (p. 230), it can also serve as a fresh point of departure for a study of the impact of political Ockhamism during the century and a half that immediately preceded the break with premodern thought consummated by Machiavelli and his followers.

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How Family Members Perceive Each Other: Political and Social Attitudes in Two Generations. By Richard G. Niemi. (New Haven, Conn.: Yale University Press, 1974. Pp. 214. $10.00.)

The main goal of this book was to assess the accuracy, in sample survey data, of various family members' reports on other family members' attitudes and demographic characteristics. The author's primary conclusions are somewhat narrowly methodological, recommending various "nuts and bolts" techniques to maximize validity of data collection. For example, he suggests specifying explicitly which target individuals the reporting family member is to consider in answering each question, and paying greater attention to such measurement techniques as scaling or factor analysis that will indicate whether or not disparate questions are in fact measuring the same concepts, and so on. Of more interest to political scientists, however, will be the excellent detailed descriptive data on variables relevant to political socialization. The data-base utilized is the now well-known 1965 Jennings and Niemi survey of a national probability sample of 1669 high school seniors, which also yielded interviews in one-third of the cases with both parents, in another third with just the father, and in the remaining third with just the mother. This study provides by far the most complete and valid data extant about parent and off-spring political characteristics.

The book essentially analyzes the accuracy of each family member's observations about each other's demographic, attitudinal, and behavioral characteristics. It proceeds by considering the student's observations of the parent; husband and wife of each other; and parent of student. In each case the author assesses, in turn, the aggregate level of agreement across the relevant family members, their pairwise agreement, the determinants of inaccuracy, and systematic biases affecting accuracy.

There are two aggregate differences of consequence. All observers underestimate the frequency of political Independents among their targets, by as much as 13 percent in one case (parents' underestimating students' Independence). And, parents systematically perceived less conflict, rancor, and tension in the family relationships than did their children, though these differences tended not to be enormous (less than 10 percent on most items). Hence the author concludes, probably justly, that at an aggregate level, there is little to be lost by using one set of family members' judgments about another's characteristics.

Viewed pairwise, family members are much less accurate about each other. More important, accuracy varies enormously across different characteristics. At one extreme, family demographic characteristics seem to be perceived quite accurately by all (though it must be remembered that the children in this case are 17 years old, probably an optimum age for accurate perception of other family members). Among political attitudes, though, the picture is much more variable. The direction of presidential vote is generally accurately perceived, for example, the student's perception of the parent's presidential vote correlates .82 with actual vote. Somewhat less accurate are voter turnout (.68) and party identification (.59), and way at the bottom is level of political interest (.25). Finally, reports of family structure and relationships show, remarkably, almost no consensus at all, beyond some minimal levels of agreement on how well the parents get along together.

The author identifies three rather interesting sources of systematic bias. One is just "good guessing." Accuracy improves as the real attribute becomes more common. For example, the vast majority of children whose parents voted in 1964 perceived parental turnout more accurately than did the small minority of children whose parents did not vote. "Social-desirability" biases tended graciously to overestimate the frequency of others' socially desirable attributes, such as doing one's citizen duty by casting a vote or voting for a victorious candidate. And "self-directed" biases systematically underestimated political disagreement between the observer and any other family member, by distorting the other's position (i.e., an "assimilation" effect).

But there is a great deal of unsystematic inaccuracy as well. Its major correlates tend to be fairly obvious. For example, broken families make children less accurate about their parents, remarriages produce inaccuracy between husbands and wives, parental agreement makes children more accurate about either parent, strongly partisan and more politicized individuals and families induce more accurate perceptions of political dispositions, and so on.

Most provocative, however, are the dramatic differences in accuracy across attributes. Everyone perceives others' presidential votes quite accurately, but no one can predict how anyone else feels about the emotional tone of their family life. A fairly static and well-known object such as party identification is perceived fairly accurately, but estimates of others' thermometer ratings of social groups are wildly inaccurate.

Inaccuracy could potentially be caused by a number of variables, most prominently (1) ambiguous or unstable target dispositions, (2) inadequate infrafamilial communication, (3) motivated distortions, (4) sloppy perceptions, and (5) inadequate measuring instruments. For example, why does everyone underestimate each other's Independence, or show lack of consensus about family emotional relationships? These attributes may be harder to detect than simple, overt, concrete attributes such as a presidential preference or vote. Or they may be less stable, and so come across in confusing ways. Maybe they are quite stable and meaningful, but not regularly or honestly discussed. This research has not yet succeeded in picking these apart. However it goes a long way toward providing some excellent descriptive baseline data from which additional research can begin to do so.

Finally, there is one curious theme running through this description of adolescents and their parents. Parents are amazingly ignorant of their children's political preferences (37 percent completely unable to place them on the party identification scale) and when they do perceive a preference, it is highly subject to the "self-directed bias." That is, parents either don't know what their kids think, or simply (and often wrongly) claim "they think just like me." And, again, parents seriously underestimate (from the child's point of view) the level of emotional conflict in the family. The impression one gets is of parents trying to present to the interviewer an erroneously harmonious, united, and intimate picture of the family, glossing over the real emotional and attitudinal conflicts that are there. But maybe the family is not such a Pandora's Box as the parents seem to think. Emotional conflicts do not carry over into political conflicts, and the children seem to want to attribute their own preferences and/or socially desirable behaviors to their parents, even when they have to distort a little. American families could perhaps afford to be a bit more open than they seem to be, without utter anarchy breaking out.

DAVID O. SEARS

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The Politics of Organizational Decision-Making. By Andrew M. Pettigrew. (London: Tavistock, 1973. Pp. 302. $13.50.)

This is a case study of innovative organizational decision making in the business firm of Brian Michaels (a pseudonym), a British clothing and furniture retailing concern. It examines the events leading to the selection of a third generation computer for the firm's central administrative offices. The author employs a longitudinal research design covering the period 1964–68 during part of which he was a participant-observer in the Management Services division of the firm.

The study is of interest to political scientists because Dr. Pettigrew conceives of the organization as a political system (following Norton Long and adapting Eastonian concepts) and he treats decision making as a political process. The work of March and Simon (*Organizations*, 1958) and Cyert and March (*A Behavioral Theory of the Firm*, 1963) serves as the theoretical point of departure. Pettigrew criticizes March and Simon for making a theory of individual decision making into a theory of organizational decision making thus neglecting the impact of the outside world on the decision maker's perspective. He argues that although Cyert and March recognized organizational decision making as a political process, they paid insufficient attention to the determinants of conflict, the role of power, and the generation of support in the decision-making process. Pettigrew's objective is to complement the political approach of Cyert and March by examining the generation of demands and the mobilization of power in the process that led to Brian Michaels' innovative decision to acquire a more advanced computer.

Dr. Pettigrew's findings concerning the generation of demands are not surprising: "Demands are generated and processes in the context of social structures in which individuals are differentially located and have, by implication, access to varying amounts of resources that are the bases of power. . . . [W]here a demand is voiced, who articulates it, and how it is diffused are all crucial determinants of the way in which it is received" (p. 266). More important is the mobilization of political support without which a demand fails. Pettigrew concludes that the mobilization of support is a function of strategy and structure which are linked by perception. Those who understand the structure of an organization can use it to their advantage in formulating strategy. In addition to structural position the mobilization of power by a decision maker depends upon: control over information flow in the decision process; political access to superiors; the extent of his/her role set; his/her assessed stature (by superiors and subordinates); and the strategies of relevant extra-organizational actors.

Dr. Pettigrew's study produces three additional results worth noting. First, he demonstrates the dynamic character of organizational decision making as reflected in the elaboration of social structure and the fluctuation of key variables over time. Organizational power relations tend to be unstable and the power balance changes, especially during an innovative period. Second, he reveals the political character of innovative decision processes. Innovative decisions are a source of conflict as they affect the distribution of resources. Even if the resources are expanded (he does not see power in zero-sum terms) and the power of all decision makers increases, the uncertainty resulting from the allocation of incremental resources will provoke "political behavior" (p. 273).

Finally, Pettigrew provides a perspective on organizational innovation that differs from the predominant orientation of those who have studied it most intensively, social psychologists and most organizational development (O.D.) advocates (e.g., Lewin, Likert, Bennis, and Slater). They view change positively as a desirable event or a goal to be achieved and managed through more sensitive human relations on the part of the leadership. Pettigrew sees change as a neutral happening (in this study a response to an external stimulus) that produces conflict and results in internal politicking and cannot be neatly managed to the satisfaction of all whom it affects. Surprisingly, he does not appear to recognize the extent to which his analysis departs from the O.D. perspective.

This book is a valuable contribution to the theory of organizational decision making, especially for political scientists. This raises the question of why our discipline has largely ignored the political nature of organizational behavior. Pettigrew is a sociologist and most of his references are to persons outside political science. He makes us aware of the wide utility of the theories and concepts developed in the analysis of conventional electoral and governmental politics.

NORMAN C. THOMAS

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Fascism and Dictatorship. By Nicos Poulantzas. Translated from the French by Judith White. (London: NLB and Atlantic Highlands, N.J.: Humanities Press, 1974. Pp. 366. £5.75.)

This is at once both an interesting and curious book. On the one hand it documents the significant changes that the "standard version" of the "Marxist" interpretation of "Fascism" has suffered at the hands of Marxist theoreticians since the termination of the Second World War. On the other, it reveals how far such commentators have yet to travel to provide a convincing account.

First and foremost, it becomes immediately clear that Fascism (and this refers to an indeterminate number of political regimes including at least a Spanish and a Japanese, as well as a German and Italian, "Fascism" [cf. p. 13]) is *not* to be conceived as the simple creature and agent of "finance capital" as it was characterized by the theoreticians of the Third International (pp. 72f., 88, 96f., 98, 110). Not only was Fascism not their creature, we find that "the big monied capitalists" frequently found themselves "discomfited" and "fettered" by the "relatively autonomous" fascist regimes in both Italy and Germany (pp. 83, 85, 88, 96, 110–113, 134f.). Moreover, in the author's judgment, Fascism—rather than being unremittingly "reactionary"—displayed some clearly "progressive" features: in Germany, for example, "industrial recovery" was "stronger than anywhere in the world." Between 1933 and 1939 industrial production in Germany "had more than doubled." "As for Italy, industrial recovery between 1922 and 1929 was the strongest in capitalist Europe." Fascist Italy's recovery after the crisis of 1929 "was quite spectacular" (p. 99). Nor were these achievements restricted to industrial production. Poulantzas is quite explicit: ". . . the massive introduction of capitalism into agriculture," we are informed, "produced some spectacular results in Italy—notably the yield of cereal production . . ." (p. 119). There was also a significant reduction in unemployment (pp. 191, 220) and an expansion of social welfare legislation (p. 221). Moreover, the standard of living of industrial workers did not fall as a whole, and in some cases significantly improved under Fascism (pp. 192f.).

The "standard version" of the Third International had informed us that Fascism was the simple tool of "finance capital." Now we learn that Fascism was far from a simple device. Fascism, we are told, was a "means" for the "establishment" of the hegemony of "big capital" that somehow "confined," "fettered," and "antagonized" that same "big capital" whose hegemony it served!

Where the theoreticians of the Third International informed us that Fascism "wound down the forces of production" to the "stagnant levels" required by "monopoly capital," Poulantzas tells us that Fascism (still somehow in the service of that same "monopoly capital") accelerated industrial and agricultural production. Where the Third International told us that Fascism produced pandemic unemployment and a grievous decline in the conditions of the working class, Poulantzas informs us that Fascism reduced unemployment, maintained stable wage levels, and in some cases significantly improved them. None of which should really surprise us because, as Poulantzas tells us, the theoreticians of Stalin's International were sadly mistaken about the "essence" of Fascism (pp. 18f, 36–52, 58f.). But then, in Poulantzas' judgment, *everyone* was mistaken about the "essence" of fascism. The reason seems to be that one one had, at that time, the benefit of "Mao's thought"!

Poulantzas seems to think that the "thought of Chairman Mao" provides some precious insight into the entire complex phenomenon (pp. 140, 228–230, 271). To be sure it is not very clear what these insights are, but they seem to turn on what might be called the "metaphysics of class struggle." Thus we learn that the history of the Soviet Union itself is to be understood only by recognizing the role of the "soviet bourgeoisie" in the "class struggle" that continues in contemporary Soviet society (pp. 230–33). If contemporary Soviet history is to be explained in terms of "class struggle" then it is not surprising that fascist history must be similarly interpreted. Fascists, capitalists, and the present leaders of the Soviet Union all serve "bourgeois interests" and invoke a "bourgeois ideology." Anyone who is "elitist" is surely "bourgeois." Anyone who is "nationalistic," or "militaristic," or "anticlerical"—or who emphasizes the special ideological role of education, or who is "anti-intellectualistic"—is clearly given to "bourgeois ideology" and must, in some sense, be serving bourgeois interests (pp. 254–56).

Only convinced "Marxists" seem to be capable of making such neat distinctions with such assurance. That Maoism, a "true proletarian ideology" serving proletarian interests, is neither elitist, nationalistic, militaristic, anti-clerical, "thought-reformist," or "anti-intellectualistic" is accepted without the least reservation. Contemporary "Marxists" seem to be gifted with that special insight that permits them to distinguish between a "proletarian" nationalism, a "proletarian" militarism and a "proletarian" elitism, and those that are "bourgeois." Thus, Marxists, unlike almost everyone else, never have difficulty in sorting out the species traits of "fascist" from "Marxist" authoritarianism, "fascist" from "Marxist" totalitarianism.

In effect, the principal merits of Poulantzas' book arise out of its recognition of the developmental character of fascist ideology, Fascism's multiclass recruitment base, and Fascism's "relative autonomy" from its putative "class sponsors." Its shortcomings are those generic to books written from a "Marxist perspective": the assumption of conspiratorial politics; the convictions that "classes" have specific "class ideologies," and that political regimes are actually the by-products of identifiable "class interests." There is far too much documentary material available that details the relationship between fascist political regimes and the organized class agencies in their respective societies to permit such arguments to pass without serious reservations. Poulantzas' grudging admission of Fascism's autonomy, as well as his recognition of Fascism's mistreatment of its putative beneficiaries, goes a long way toward laying the ghost of the "Marxist interpretation of Fascism." That Poulantzas seems unaware of his achievement provides some insight into the nature of ideological thinking.

A. JAMES GREGOR

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In Defense of Political Philosophy: A Reply to Robert Paul Wolff's "In Defense of Anarchism." By Jeffrey H. Reiman. (New York: Harper Torchbooks, 1972. Pp. xxxii + 87. $1.95, paper.)

Reiman's book is a polemical critique of Wolff's much-discussed *Defense of Anarchism* (New York: Harper Torchbooks, 1970). Written with verve, it is readable, even agreeable, in style. The author summarizes his argument at a number of points, particularly in the preface and long introduction (especially pp. xxvi–xxxi).

Reiman's main argument is that Wolff first identifies the political with the moral realm, political authority with moral authority (pp. xxii, xxiv, 36–37, 44, 48), and then goes on to argue that the fact of a moral authority (i.e., one which could issue morally binding commands) would be incompatible with the moral autonomy of individual persons (pp. xxi, 35, 49, 79). Reiman asserts, however, that the very notion of a moral authority is incoherent (pp. xxii, 4, 9, 51, 62). Accordingly, it is simply pointless to attempt to reconcile such authority with moral autonomy. Hence, Wolff's mapping of political authority onto the unthinkable notion of moral authority serves simply to create a straw-man issue.

Once Wolff's problem is exposed as a pseudo-problem (pp. 6, 61), and thus dissolved, we are free to ask whether a political system could be *morally* legitimate (or justified). This is really the question whether a given set of laws (with a specified form of official coercion) *tends* to "yield more morally worthy results" than can be expected from (1) the absence of *any* laws or from (2) other practicable alternative political systems (p. 32). This question must be asked by reference to the "general foreseeable effects" (p. 32) of such systems—and of their total absence. Legitimacy, when it can be established, provides *prima facie* reasons for obeying particular laws; but these reasons can be overridden by other *moral* considerations in a particular instance (p. 35). On this view moral autonomy is not sacrificed; for the agent can always decide, on a given occasion and for moral reasons, *not* to obey a morally sanctioned civil law, e.g., a traffic law (see especially pp. 37–39). However, there is still a general presumption in favor of complying with laws, given the overlap, in general, of the *effect* of these laws (in the behavior mandated) with morally good results (pp. 35, 43, 46, 53–55).

Moral autonomy pertains to individual occasions, when we weigh our moral reasons and decide what we ought to do; political authority pertains to the overall tendency of the laws to be good-producing on *most* occasions. Hence the two notions can be reconciled (see pp. 41, 42–46, 80). Indeed, civil disobedience is the classical example of their reconciliation at the point where moral autonomy and legitimate political authority do conflict (pp. 57–59).

Now that I have summarized Reiman's argument briefly, I have three questions. How accurate is his interpretation of Wolff? How telling is his critique? What contribution has Reiman made to political philosophy in his constructive account of the nature of political authority and of the moral presumption in favor of obedience to law?

I think Reiman misinterprets Wolff on a fundamental point. He represents Wolff's position as based on denying any so-called executive moral authority (e.g., God), whose very commands themselves constitute moral duties (pp. xx, 1, 36, and also 45, 54). But Wolff is not concerned with whether governments can create moral duties *ex nihilo*. His point is the rather different one that we have no moral duty to obey laws as such (as distinct from duties to do any good thing, whether mandated by law or not).

Wolff is a rule man (in that all our moral duties are specified by *strict* rules). Since he believes we have no *strict* obligation to do whatever the law says, simply in virtue of its being valid law, he concludes that laws qua laws have no special or peculiar moral standing at all (unlike promises, contracts, lies and so on). Obedience to law, insofar as the matter does come under moral scrutiny, is not categorically imperative and, hence, is not one of our moral duties at all.

Reiman, on the other hand, has no place for these *strict* rules, even about such things as promises (p. 55), in his account of duties. He is a rather standard sort of utilitarian (see p. 38). Keeping promises usually has "morally valuable results"; hence we have developed a *moral maxim* (that says they should be kept) which can, nonetheless, be overridden on a given occasion. It is no different with laws. In a legitimate state the laws have a tendency to "morally good results" (indeed it is this tendency that actually legitimates such a state in the first place [pp. 56, 65–66]). Here "Obey the Law!" is a moral maxim; but like all such maxims, though it points to "morally good results," it is not a strict or absolutely imperative rule (pp. 54–55, and also 9, 43).

This brings us to an interesting point of convergence in the two positions: we may have obligations to the law but these can never be strict ones (which is what Reiman affirms); none of the strict obligations we may have can be obligations to the laws per se (which is what Wolff affirms). We can only conclude that we have no strict political obligations at all. This is important. For if we make strict political authority turn on it (as did the great "obligationists" in the history of political philosophy, such as the Socrates of Plato's *Crito* or Hobbes or Locke or Kant), then to deny strict political obligation is to deny political authority.

It is the perception of this fact that has been seized on by Wolff in his *Defense of Anarchism*. And, since Reiman has mislocated Wolff's animus, as being against "executive moral authority" (the phrase is De George's), his basic critique of Wolff is largely beside the point. Reiman accordingly fails to see that Wolff's strictures really are telling against traditional justifications of political authority which rely on grounding that authority in the citizen's strict obligation to obey the laws issued.

The implications of this "strict obligation" view (which Wolff's anarchist shares with those who *affirm* an obligation to obey the law) are equally striking: they serve to reveal an important pattern in the logic of justifying political authority. For where obligation is the prior issue, as it is in the perspective under discussion, we cannot ground that obligation by reference to the *other* political concepts (such as the capacity to issue rules). Rather, the task of justifying the *basic* political notion, whatever it is (and in the case at hand it is our strict obligation to obey laws), requires that we go outside the entire system of political concepts. Hence, the justification of political authority, whether it can be accomplished or not, must be attempted by reference to some external, nonpolitical standard.

The question, then, becomes whether Reiman offers a significant alternative to this externalist program for justifying political authority. I would suggest that he does not. His argument remains externally grounded in that he simply identifies justified political authority with *morally* justified political authority (see especially pp. 44–45, 53–55). Authority proper attaches not to the lawmaking role or to the political institutions in which it is embedded but to the external ground, to the "morally worthy results" (as determined by some superordinate ethical standard).

Reiman's book, then, is a conventional defense of political authority on *moral* grounds. It opens no new territory and stays well within the traditional argument pattern, as found in the externalist program for justifying political authority.

REX MARTIN

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The Study of Government: Political Science and Public Administration. By F. F. Ridley. (New York: Crane, Russak, and Co., 1975. Pp. 240. $9.00, paper.)

The title of Professor Ridley's book is deceptive, for *The Study of Government* is neither an introductory text nor a systematic advanced treatise on government. Rather this book consists largely of a number of the author's essays that have previously been published within the last ten years on various aspects of public administration and political science. As with any book made up of spliced-together essays, the style is uneven and the theme wanders; hence, the book is likely to exasperate a careful reader who wants more details at certain points, tighter logic to reinforce speculation, and more polish given to arguments which at places are repetitious or half developed. Yet, as Professor Ridley admits at the outset, his work is "a polemic" that "advocates a certain approach to the study of politics and public administration." The thesis the author vigorously presses throughout is: "The focus of study [of government] ... should be *institutional*, the method comparative, the purpose practical."

The first half of the book is devoted to examining political science as an academic discipline and here the author argues for political scientists to return to a "more formalistic and utilitarian approach" and move away from the behavioral and value-free scholarship. This theme no doubt will sound old-fashioned to many contemporary American political scientists who now by-and-large recognize the 1950s behavioral revolution as having been won and accept the existence of the so-called "post-behavioral" or "post-post-behavioral" era where presumably the best of both "traditional" and "behavioral" are blended and utilized. The recent growth of the policy analysis schools and public administration programs that combine both the behavioral and the applied, best reflects this "post-post-behavioral" trend in America. Even though British Professor Ridley's message may have arrived too late to be meaningful for American political scientists, his vigorous "polemic" for the traditional at least can be used to provoke plenty of lively debate among undergraduates in introductory methods courses.

The second half of *The Study of Government* deals with public administration and here the author is on firmer ground for this field has been his specialty for some time. Professor Ridley has published several noteworthy books on European public administration, including *Public Administration in France*, and *Specialists and Generalists*. Students in public administration will especially find the second half of Ridley's book useful when placed alongside several recent American writings such as Vincent Ostrom, *The Intellectual Crisis in American Public Administration;* Frederick C. Mosher (editor), *American Public Administration: Past, Present, Future;* Frank Marini (editor), *Toward New Public Administration;* or Dwight Waldo (editor), *Public Administration in the Time of Turbulence*—all of which wrestle with the central issues: what is public administration? and what should it become? Unlike, for example, the American theorist Vincent Ostrom, who believes that American public administration should cut its ties with its Wilsonian-Weberian past and undergo a "radical paradigm shift" by adopting contemporary public choice theories, British Professor Ridley argues for the discipline to focus on the pragmatic issues of institutional reform—an approach that stands very much in the mainstream of American public administration doctrines dating from Wilson, Whites, and Willoughby. Here again, this portion of Ridley's book which stridently advocates the "traditional" should stimulate provocative debate in introductory administration methods courses, even though the nagging question is never really confronted by the author—why, given all the interesting varieties of models, paradigms, and approaches offered in the field today, is the "traditional" *the* soundest model for public administration to adopt? Many readers will find Ridley's vision of public administration uncomfortably restricted given the diversity of administrative methodologies developed within the last 30 years.

RICHARD J. STILLMAN, II

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Sozialdemokratie und Marxismus: Zum Spannungsverhältnis von Godesberger Programm und marxistischer Theorie. By Alexander Schwan and Gesine Schwan. (Hamburg, Germany: Hoffmann und Campe, 1974. Pp. 397. Price not listed.)

The purpose of this book by Alexander and Gesine Schwan, professors of political science at the Free University of Berlin, is to examine the relevance of Marxism for current social democratic theory in Germany. The authors discuss the intellectual foundations of the SPD's 1959 Godesberg Program, criticize Marx, Lenin, and contemporary Marxist system theorists, and finally develop some of their own ideas about democratic socialism. Taking their own positions from the Aristotelian and Christian-humanist tradition, the authors focus their criticism and polemics on the young socialists (Jusos) in the Social Democratic Party, whom the Schwans regard as being dominated or at least highly influenced by various currents of Marxism. They conclude that Marxist theory has to be considered irrelevant for the positive formulation of a theory of democratic socialism, because even Marx himself in his conceptual formulations and practical intentions became a traitor to and rejected his original ideas of emancipation and realization of human potentials (p. 166).

For the reader who followed the authors' argumentation up to this point it is surprising that in the final chapter Yugoslav and Czechoslovak reform Communists are presented as potential contributors to a theory of democratic socialism. There can be no doubt that reform Communists in Yugoslavia with their municipal and industrial self-government or the initiators of the Dubcek spring of 1968 turned Marx against Lenin and Stalin by actualizing certain fundamental democratic and human principles implied in Marxism. But in pointing to these modifications of communist practice the authors contradict their own line of thought by advocating Marxist theorems in reform communism while condemning them for German young socialists.

Furthermore, the Schwans neglect to mention those Marxist-reformist traditions which influenced the Godesberg Program and which are—in the Hegelian sense of the word—preserved in this still-valid declaration of social democratic principles. Among the best known examples are Rudolf Hilferding's concept of organized capitalism and Fritz Naphtali's notion of industrial democracy (Wirtschaftsdemokratie). To have responded to writers like Otto Bauer and Karl Renner (both Austro-Marxists), to Max Adler, Max Seydewitz, Arkadij Gurland, Fritz Sternberg, Richard Löwenthal-Sering would have meant a real challenge to the Schwans, since these often-forgotten theorists influenced social democracy not only in the Weimar Republic but in the Federal Republic as well. Their arguments are more coherent and on a higher analytic level than are those of contemporary young socialists. As a result their contributions to a theory of democratic socialism should not be ignored, rather they should be regarded as a field for fruitful social, economic, and political analysis.

Despite such reservations, I think the Schwans have done a very creditable job of promoting a hot discussion on democratic socialism in Germany by defining their own position and turning it against that of the young socialists.

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Social Policy: An Introduction. By Richard Titmuss. (New York: Pantheon Books, 1975. Pp. 160. $8.95.)

It is of great importance to establish at once what this book is not. It is not really a book (if by book we mean a unified and coherent work), nor is it really written by the late Richard Titmuss, the widely known and respected social philosopher and both proponent and critic of the British welfare state. Instead it is a series of Titmuss' lectures prepared for his Introduction to Social Policy course at the London School of Economics and compiled and edited after his death by his wife, Kay Titmuss, and Brian Abel-Smith. Despite a fine effort by the editors, the work suffers from its form on two accounts: as a series of rather disconnected lectures it lacks a coherent theme, and as an introduction designed for first-year graduate students it lacks the analytical sophistication found in Titmuss' previous works. Nonetheless the work is worthwhile as a distillation and reiteration of Titmuss' thinking, reflecting the tremendous insight which has come to be expected of him and reflecting, as well, some less readily acknowledged limitations.

His concern is with the subject of social policy, an academic discipline much more common in England than in the United States, where it appears to exist, if at all, as a subfield in social work schools. "Social policy" is a difficult term to come to grips with; it appears to be one of those terms which everyone knows the meaning of even though no one can define it. Titmuss' best effort (p. 49), though lacking in precision, conveys the concept in a general sense: "Social policy is basically about *choices* between conflicting political objectives and goals and how they are formulated; what constitutes the good society or that part of a good society which culturally distinguishes between the needs and aspirations of social man in contradiction to the needs and aspirations of economic man."

Studying social policy does not, however, necessarily imply (though it does not rule out either) studying how decisions are made or the objective analysis of the social impact of decisions. Instead social policy as a discipline (profession?) is unabashedly concerned with normative questions of social change and redistribution as well as analytical questions of the impact of various policies and programs on these normative concerns.

Titmuss repeats the basic concerns of his previous works: the distinction of social man from economic man; the inhumane consequences of private market mechanisms and institutions; the importance of the "gift relationship" in social policy; and the backwardness, if not barbarism of the United States in matters of social policy and institutions.

As before, he does not concern himself with the problem of how the universalist public and social welfare services he advocates are to be financed. Nor does he deal adequately with the problem of accountability in the public sector. He argues (p. 55) that, since the discipline of the market is not present, other kinds of discipline must be fashioned for the public sector and its personnel: "more education, training and in-training; an incorruptible, secure civil service, developing its own codes of behavior, rule-making, and procedures, and continually pre-occupied with problems of equity and fairness as between one citizen and another; the use of 'quality controls' through Parliament, investigating committees, research, inspectorates, pressure groups and so forth. In short, if social services are to be delivered effectively, equitably and humanely, more and better educated social administrators are required." This is a singularly lame and unconvincing response to the problems of bureaucracy, begging, as it surely does, the question of how one trains administrators to be "continually pre-occupied with problems of equity and fairness," and also ignoring the institutional settings into which these well-trained civil servants will be placed and the resulting dynamics to which they must respond.

Titmuss' insensitivity to the accountability question is further illustrated in his discussion of the Gary, Indiana, education performance contracting experiment which he criticizes as an example of the American mania for private markets and the reprivatization of even previously accepted public services. He observes that "the American arguments in favor of introducing criteria of profit into the educational system are that (a) teachers work harder, (b) fewer teachers are needed, (c) parents are brought into participation, and (d) the teaching profession is brought under control and thus there is less professional bias in favor of the bright, higher I.Q. white child" (p. 41). Nowhere does Titmuss mention the primary—and often stated—rationale for education performance contracting, namely, that they are a means of improving educational quality by holding schools accountable for their results.

In many ways Titmuss is at his best in his broad-scale global analysis of U.S. social policy which he characterizes as "Model A, the residual welfare model" as opposed to "Model C, the institutional redistributive model" which characterizes—or he would like to see characterize—the United Kingdom. Unfortunately he is at his very worst in dealing with specific examples from the American context such as the one quoted above. Some of his examples, used as a foil more than anything else, are little short of outrageous as when he observes (p. 43) that "few black people in the U.S.A. can afford to pay the price of private medical care in hospital (now probably approaching something like £500 per week)." It should not be construed as an apology for the present American health system to note that private health insurance, medicare, and medicaid undoubtedly enable the majority of blacks—and whites—to obtain hospital treatment for out-of-pocket sums a fraction of the total charges he cites. The portrayal of the United States in this, as in his other books is too stark, inflexible, one-sided, not to mention inaccurate.

This is all too bad. There is enough wrong with U.S. social policy and enough good sense in what Titmuss has to say to make these kinds of embellishments unnecessary.

HAROLD WOLMAN

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Critique of Earth. By Arend Theodoor van Leeuwen. (New York: Charles Scribner's Sons, 1974. Pp. 295. $10.00.)

This is the second series of Gifford Lectures delivered by van Leeuwen in 1970 and 1972, respectively. The first series, entitled *Critique of Heaven,* was published earlier and is in fact already out of print. It is a pity that the two series do not appear in a single volume; they are much more a continuum than a contrast, and some of van Leeuwen's initial remarks in *Critique of Heaven* help explain his project with an explicitness that does not recur until the final chapter of the present book.

The subject of the lectures is the thought of Marx. Van Leeuwen is a theologian by trade. These facts might lead one to anticipate an exercise in theologizing Marx, and indeed an element of this exists; for one thing, certain of Marx's writings have never before been so closely scrutinized for their religious allusions and redolences as they are by van Leeuwen. But the author's more prominent aim is to "Marx-ize" theology by presenting Marx as a pivotal figure in the history of that subject and an indispensable guide for contemporary religion. Since the bulk of van Leeuwen's work consists of textual exegeses of Marx's writings and embodies some of the best qualities of that kind of approach, the book is of considerable value even for those political theorists who may share none of van Leeuwen's concerns as a theologian.

The titles of both lecture series are derived from a famous passage in the *Introduction to the Critique of Hegel's Philosophy of Right* in which Marx stresses the theoretical necessity, as he sees it, for transmuting the critique of "heaven," i.e., of religious notions, into that of "earth," that is, of political economy and of the socio-economic structures for which it furnishes ideological support. Van Leeuwen's division of labor between the two books does not, therefore, conform to the most standard chronological division between the "earlier" (pre-1845) and the "later" (post-1845) Marx, according to which the *Theses on Feuerbach* constitute, to some degree or other, a turning point. Rather, the bulk of *Critique of Earth,* like all of its companion volume, concerns various earlier Marxian writings. Only in the final chapter does van Leeuwen finally succumb to the temptation, overwhelming for a theologian with an interest in Marx, to analyze the "Fetishism of Commodities" section of *Capital,* in which Marx himself drew lengthy analogies between the religious sphere and that of political economy—and even then he does so only very briefly.

The book contains chapters about Marx's newspaper articles on the new Prussian draft legislation against wood-theft and concerning divorce, about Hegel's political theory and Marx's critique of it, and about some themes in Marx's *Critique of Political Economy* and *Capital.* One of van Leeuwen's most useful contributions is his systematic analysis of the pejorative connotations assigned by Marx to the terms "materialism" (as in the biblically based expression, "*verworfene* [reprobate] *Materialismus*") and "material interests" (of the landlords), in his attack on the proposed wood law. This legislation was a dramatic example of the historical transition from feudal to capitalist relationships (the landholders had become aware of the commercial possibilities of the dead wood that it had formerly been the peasants' custom to glean), and van Leeuwen admirably shows the genesis of some of the mature Marx's major themes in Marx's groping attempts to understand the broader implications of what was occurring in 1842.

On the other hand, Chapter 8, entitled "Money as Mediator," illustrates some of the possibilities of excess inherent in van Leeuwen's exegetical approach. It is true that Marx, early in Volume I of *Capital,* speaks of the "transubstantiation" of commodities into gold and cites three lines from Dante's *Paradiso* by say of making a slightly humorous point about the need to have real money in one's purse in order to enter into an actual exchange transaction. It is also true that Marx cites Dante on two other, more familiar occasions in his later work. But van Leeuwen's excursus on the "coinage" of faith, which was at issue in the original Dantean verse, leads to interesting, brilliant, but unnecessarily complex extensions of the original analogy that go far beyond what it is reasonable to impute to Marx's train of thought when he wrote it down. In van Leeuwen's defense, of course, it could be argued that he is simply drawing systematic interconnections among regions of Marx's thought about which Marx himself, who had obviously absorbed so much from so many cultural traditions, was not fully conscious.

In his final chapter, van Leeuwen returns to an attack, initiated at the beginning of *Critique of Heaven,* on Karl Barth—first for his failure to include Marx in his history of nineteenth-century theology (even while Barth assigns a very prominent place to Feuerbach), and then for the narrowness, compared to Marx, of Barth's own theological outlook. Van Leeuwen finds it unacceptable to undertake critical theological thinking today without engaging, as part of that thinking, in political and economic analyses. When all is said and done, says van Leeuwen, "Barth's is indeed a 'Swiss theology' *par excellence*" (p. 269); nowhere does one find him raising questions about the basic political and economic structures of the country of which he was a citizen. Consequently, Barth and his many admirers miss Marx's most basic conclusion about modern Christianity, namely, that it is the most adequate form of religion for the bourgeois state.

Unless theology is capable of passing through both aspects of radical Marxist criticism, the "heavenly" and the "earthly," according to van Leeuwen, it has no future. His conclusion is an easy one with which to agree, it seems to me, but there is certain to be disagreement as to whether or not one finds it troubling. Van Leeuwen clearly does, while Marx, it seems plain to me, did not. Although van Leeuwen does not make any systematic use of the expression, "political theology," as a label for what he is advocating, I found his book reviving in me the old questions that arose when I once had some involvement in a course in that putative subject-area: is there really such a thing? and *can* there be such a thing?

At any rate, persons with serious interests in religion or in political theory or in both are bound to derive great profit from reading van Leeuwen's careful, scholarly book—whatever the intellectual coinage in which they choose to draw their profit! *Critique of Earth* is also a *direct* contribution to Marxist-Christian dialogue, much richer in content than many of those metacontributions that confine themselves to talking about the fact either that such a dialogue is taking place or that it *could* take place.

WILLIAM LEON MC BRIDE

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Roscoe Pound: Philosopher of Law. By David Wigdor. (Westport, Conn.: Greenwood Press, 1974. Pp. 356. $12.95.)

In some ways the career of Roscoe Pound is a bit of an embarrassment to the law. The road from St. Paul and "The Causes of Popular Dissatisfaction with the Administration of Justice," to Harvard and praise for *Stolberg* v. *California* is hardly an inspiring one. Conceivably, Mr. Wigdor agrees, for his story seems to run downhill when, after an exciting sojourn in Chicago, Pound leaves for Harvard. Suddenly the human Pound who could rouse the entire student body at Nebraska to torchlight protest parades at the word of his impending departure becomes a cardboard character.

I suspect that the story need not have been told that way. Leon Green reported to me that Dean Pound, for all his orotund pomposity, was generally beloved by the bar for what were known as the "sonofabitch" stories and for an ability to drink most any bar luminary under the table. And, arguably, the Harvard years brought his greatest achievements: a Supreme Court—indeed a world of legal scholarships—that used "interest analysis" as a daily tool of its work, and a corps of loyal graduate students whose views stretched the ideological spectrum from the Lawyer's Guild to the Liberty Lobby.

The Pound who could have seen triumph in his declining years is, however, Pound the Dean, not Pound the Philosopher of Law. The choice to emphasize the one and not the other, as Mr. Wigdor has done, is plausible. Pound was known as a philosopher, whether or not he was much of one. But I for one would have preferred to know more about Pound at Harvard. Possibly that preference reflects my narrow, law school bias, but it also reflects a deficiency in Mr. Wigdor's analysis and understanding of Pound's career and jurisprudential work.

Mr. Wigdor argues that there are two threads to Pound's thought—a sort of Hegelian, but not idealist, organicism and a Deweyan instrumentalism. The case for this continuing dualism is a good one, but Pound's Harvard years suggest that there were some regularities in his actions that Mr. Wigdor's analysis simply does not explain.

Examples are easy to find. Consider that Pound was brought to Harvard with a national reputation as a progressive reformer, was made dean three years later, and yet his philosophy, indeed even his concern for the administration of justice, apparently never led him to make an alteration in the routine of educating law students. Or wonder how, between the Cleveland Crime Survey of 1927 and the Anti-Realist Manifesto of 1931, fact research changed from something good to something bad. Or puzzle over what happened between the red scare "Heresy Trial" at Harvard in 1921, in which Pound stood among the accused, and the Wigmore/Frankfurter controversy over the Sacco-Vanzetti case in 1927, in which he stood on the sidelines. To me the pattern is plain. Something made Pound sour on the remnants of progressivism that began to reshape old doctrine after World War I, for it is apparent that whenever Pound did not start out on the Right, he moved that way.

Mr. Wigdor does not note, much less explain, this pattern, but in avoiding explanations of the regularities in Pound's actions, he does no worse than his subject. For both men decisions are wrapped up in a mysterious balancing process in which results are self-evident, at least in retrospect. Thus, if Pound resolves a problem one way it is because one or the other of the dual elements dominated, just as, given the outcome of any case decided by today's interest-analysis Supreme Court, a competent constitutional scholar can guess the interests and the weight given to each. But if that same scholar tries to intuit the interests, not knowing the result, that scholar will find him- or herself talking about Mr. Justice Burger's values, for it is those values that seem to control the choice and weight of interests that, once known, mechanically determine results. Thus in practice Pound's philosophy is literally a formal mechanism in which the decisive questions—what interests of what weight—are hidden from view. Mr. Wigdor does not understand this fact as is made obvious by his failure to reach behind the formal dualism of instrumentalism and organicism to try to understand the values that determine which side of Pound's own balance compel his solution to a problem. And, since it is in Pound's role as dean that both the nature of his philosophical system and his values become simultaneously apparent, it is understandable why Mr. Wigdor slights this period in Pound's life.

But to explain is not to justify. If the system virtually drops out in determining results, what was its impact and thus the impact of Pound the Philosopher? Surely the answer to this question is to be found in the largely untold story of Pound the Dean for, if the system made little difference, what made Pound significant in the eyes of his colleagues and students, was his role as a Harvard Law School institution. That role we still do not know enough about.

JOHN HENRY SCHLEGEL

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The Psychology of Conservatism. Edited by Glenn D. Wilson. (London and New York: Academic Press, 1973. Pp. 277. $13.50.)

Here is an attempt to establish the concept of conservatism as a more or less one-dimensional, centrally organizing element of personality. It comes upon the scene at a time when the concept of political-economic conservatism, at least, is under attack: Converse has shown that it is not an organizing concept for the mass public; Rokeach has argued persuasively and with evidence that it comprises not one dimension but two (equality and liberty); Schoenberger has shown that for certain practicing conservatives it is indeed an organizing concept, but is in no way related to the personality characteristics that McClosky said informed and guided the conservatives he measured; the general attack on the methods of *The Authoritarian Personality* (ignoring its validity) has infected the belief in the relationship between authoritarianism and conservatism, and so forth. Thus, although Mr. Wilson and his associates are not joining issue with some of these critics of the concept (indeed they agree with the critics of *The Authoritarian Personality*), they must do some persuading to revive conservatism as a viable, single-voiced concept in socio-personality research.

Nor do they help their case by their implied and sometimes explicit metaphysics: there is something out there that *is* conservatism, if we could only devise the right instrument to capture it. Fortunately, this metaphysic does not impair their research (only their language about it), for the book is built around a simple 50 item "C" (for conservatism) Scale, from which all confusing prose is removed and the respondents simply circle "yes" or "no" or, "if absolutely uncertain," place a "?" next to the item itself. Some of these items are "death penalty," "evolutionary theory," "coeducation," "white supremacy," "computer music," "disarmament," and "Bible truth." It will be seen that these items embrace much more than political conservatism; indeed, one of the aims of the authors is to show the generality of the dimension and thus to be able to place political preferences within this larger construct, and so to help explain them. In this they have some success, partly, I think, because their conceptualization of conservatism is both broader and better than the concepts used by analysts of belief systems in mass publics.

To do this, of course, they must show the interrelationship of the items to each other to reveal the way they hang together. Under the scrutiny of factor analysis they can show that there is one general factor loading, which they label, appropriately enough, "conservatism," and several less embracing dimensions: "realism (versus idealism)," "militarism-punitiveness," "antihedonism," "ethnocentrism and out-group hostility," "religion-puritanism." As might be expected from these labels, the C-Scale is positively related to F-Scale, dogmatism, and ethnocentrism scales, giving some further plausibility to the authors' contentions that there is a great deal of commonality in the measures and concepts, but many will doubt that these concepts "are all orderable along a single dimension, best called liberalism-conservatism" (p. 33).

The articles are highly pragmatic, often devoid of even those limited "mini-theories" beloved of American psychologists (dissonance, attribution, equity, transaction, etc.), and steer a wide circle around that kind of major informing theory that graces (or deforms) *The Authoritarian Personality.* But there is an attempt to explain conservatism itself in an all too brief, interesting, theoretical chapter by Glenn Wilson (the author or co-author of eight of the 17 chapters). "The conservative attitude syndrome," he says, "is a generalized susceptibility to experiencing threat or anxiety in the face of uncertainty" (p. 259). It has sources in such genetic factors as "anxiety proneness" and "stimulus aversion" (sic), as well as low intelligence, female sex, and old age, and in such environmental factors as "parental coldness, punitiveness, rigidity," and lower class membership. These combine to create feelings of insecurity and inferiority, which, in turn, produce a generalized fear of uncertainty, expressed either as avoidance of stimulus uncertainty (dislike of innovation, etc.) or avoidance of response uncertainty (lack of self-reliance and search for external authority). Both kinds of uncertainty avoidance are then expressed in the conservative attitude syndrome which guides responses to particular issues such as those expressed in the C-Scale. The theory has a beautiful simplicity to it, seems to account for much of the evidence (indeed, is constructed from the findings in an inductive manner), and is close enough to observables to allow for further testing of the sources, as well as the consequences, of the conservatism measured.

With this apparatus, what do the authors discover? Children's conservatism is different from adults in that conventionalism (good behavior syndrome) is *negatively* related to ethnocentrism. (In line with the general "close-to-the-data" posture of the work, the author of the piece [Nias] does not examine how it is that these relationships become reversed in the process of growing up.) Among clergy, theological conservatism is part of the larger conservatism syndrome (Wilson and Bagley). In an interesting study of superstitious *behavior* (avoiding walking under a ladder), Boshier finds that it is the younger liberals and the older conservatives who behave in this inferred superstitious manner. He speculates that, after the counterculture, superstition is more associated with liberalism than formerly was the case. As was known before, conservatives have aesthetic preferences for less ambiguous art (Patterson). And in direct confirmation of the theory, Kish finds that liberals seek stimulus-rich situations and choose less conventional vocations.

The broad conceptualization of conservatism distinguishes this work from others in the area; the simplicity of the measure and the theory may have a reductionist tendency, but they have the great advantage of clarity. Perhaps the concept of a general factor of conservatism is on the way back; it might even help to explain *some* political conservatism. But it is important to remember that people are guided by their economic stakes and interests, as well.

ROBERT E. LANE

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American Politics

The Uncertain Search for Environmental Quality: A Case Study in the Failure of Modern Policy-Making. By Bruce A. Ackerman et al. (New York: The Free Press, 1974. Pp. 386. $13.95.)

The study of environmental policy can generate significant personal frustrations and uncertainties. It also can highlight uncertainties about the performance of our political system. *The Uncertain Search* is an effort to investigate the issues related to the latter without adding to the former.

Ackerman and his co-authors seek to "prepare the way for fruitful theorizing by carefully exploring an actual decision-making process which reveals in a paradigmatic fashion the nature of both the challenge and the opportunity that technocratic intelligence offers to a contemporary democracy" (p. 2). The case study analyzes the interrelationship between technocratic intelligence, political decision making and legal enforcement as exhibited in a "modern" governmental institution—the Delaware River Basin Commission (DRBC). The dilemmas of an interstate agency involved in complex technical policy area (water pollution) provide the focal point for this effort. Technical research, public involvement, cooperative federalism, and administrative decision making are scrutinized.

Two major problems receive emphasis: (1) the capability to link the results of comprehensive technical research into administrative decision-making processes; and (2) the ability of institutions to design and implement effective policy. The findings support the uncertainty of accomplishing either objective. The necessity of depending on poorly conceptualized technical research on water pollution, the absence of an in-house capability to review technical information, and the weaknesses interjected by the former in the implementation of policy are the focal point for the first problem. A discussion of effluent standards and pollution control strategies provides the medium for identifying and evaluating the second problem.

The findings of the study are not startlingly new. The DRBC, despite its "modern" design, suffers from common, contemporary problems. Quantitative research findings and recommendations are at odds with qualitative judgments on river use. A major DRBC cost-benefit analysis of alternative cleanup policies, compiled on the basis of poor information, did not provide effective standards for decision making. The expertise of decision makers and strong commitment of participating governmental and industrial actors could not produce consensus on policy objectives. Intergovernmental actors, representing divergent constituencies, could not generate a meaningful, basinwide community of interest despite the apparent commonality of problems. In short, "politics" reintroduced fragmentation and prevented the full integration of technical considerations with decision making. In this light, DRBC pollution control policy for the basic lacked the qualitative element of enforceability which would have made it effective.

Incomplete integration of disparate interests forces policy makers to ignore the demands of technical considerations. This breakdown in the linkage between technocratic intelligence and decision making appears endemic to contemporary governmental structures. It exists in many other policy areas. The authors raise a legitimate issue, yet, like many others, they suggest a solution (redesigning institutions) which does not fully appreciate the nature of the problem, particularly in the arena of environmental policy.

Historically, environmental policy has not exhibited the linkages desired by Ackerman and his co-authors. Policy has lagged consistently behind technological innovation, leaving the consistent impression of too little, too late. Incredibly swift social and technological change has made this defect unavoidable, rendering environmental policy obsolete on promulgation. Each change in existing policy (technological demands aside) has reflected a concern both for the effectiveness of policy and institutions and for new parameters/issues which policy should address. This latter aspect, reflecting shifts in policy maker perceptions of the nature and scope of the problem, is based partly in the need to bridge the technological lag. More importantly, it also reflects major shifts in political choices, preferences, and priorities. In effect, the *definition* of pollution has changed.

In light of these definitional changes, analysts have focused on technological and legalistic mechanisms as solutions to political problems rather than dealing with the political foundations of technology, policy, and law. The more fundamental issue of politics in a democratic society, and the unique pressures it raises for policy decision making, seem to messy or inelegant to deal with. Yet, it is here that root causes of "uncertainty" lie. The authors realize this, yet shy away from the political dimension of policy problems, and take refuge in the more mechanistic arena of designing institutions.

An example of this shortcoming can be found in the book's final chapter. The authors observe that the 1972 Federal Water Pollution Control Act poorly comprehends the technologically possible means of combating pollution. To them, "no discharge by 1985" represents wishful thinking rather than realistic policy making. To an extent they are right. Yet, this sort of policy goal also represents an attempt to make technology a servant of decision making rather than its master. The reversal raises a new "uncertainty." Can a democracy effectively formulate definitions of policy problems and issues which permit it to act upon rather than react to technology and thus formulate effective policy/law? The last few years of air and water pollution control efforts raise serious doubts.

The lesson of the DRBC is thus not solely its failure to make effective linkages (the authors' "uncertainty"). Its additional lesson is that it is necessary to look beyond purely technical and technocratic approaches in program and institutional design to the foundations of political policy making in a democracy: Can meaningful policy goals be set to guide decision making? The authors seem to brush by this fundamental problem.

Aside from the substantive (de)merits of the book, it is also weakened by its structure. It is a patchwork of chapters authored by different groupings of the collaborators (p. ix). The interdisciplinary efforts lack full integration. Also, the period of time between research and writing (1971–1974) leaves many new developments unevaluated—a regrettable necessity. This book can benefit those who have never experienced firsthand the dilemmas of environmental policy. It is, however, only a limited beginning toward suggesting where we go from here.

SHELDON M. EDNER

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Health Care Politics: Ideological and Interest Group Barriers to Reform. By Robert R. Alford. (Chicago and London: University of Chicago Press, 1975. Pp. 281. $12.50.)

Health Care Politics is a near-polemic analysis of the health care delivery system in terms of interest group politics. While such an analysis might seem quite conventional to a political scientist, it is novel within the health research community. So much so, that Alford's earlier article outlining his thesis created a considerable stir.

Alford contrasts his thesis with two other theories of health care reform, those propounded by the "market" and "bureaucratic" reformers. In Alford's view, the market reformers see the problem as solvable by market mechanisms, if the health care market were freed from the many restrictions on it, including both the government's heavy-handed regulation and the AMA's restriction on the number of physicians. The bureaucratic reformers, on the other hand, contend that since health care must be delivered in part by enormous institutions, the fragmentation of the existing system can only be overcome by forcing bureaucratic coordination on all the actors. The bureaucratic reformers' efforts to coordinate the pieces of the health care delivery system have failed to improve the quality of health care for the poor; their failure, according to Alford, can be understood from a " 'structural interest' perspective. Powerful interests benefit from the health care system precisely as it is—with its ineffective layers of bureaucratic 'planning' and 'administration' and its uncoordinated separate organizational and professional components responding to demands by the sick for care" (p. 6).

Alford's data consist of the reports and minutes of the half-dozen committees assembled to deal with the New York City health crisis, as well as reports by management consultant-type projects and the coordination committees required under various federal grants. The book looks briefly at other cities and concludes that New York is typical. It closes with a long theoretical essay explaining why the structural interests remain dominant in the contest.

Any judgment of this book must, of course, be a value judgment; one man's reform is another man's poison. I am inclined to see the overall structural interests argument as sound, probably because my biases are compatible with Alford's. The lengthy discussion of the work of the various committees is not very informative, however. The author's reliance on documents rather than on interviews prevents one from obtaining a clear picture of the permanent cleavages in the politics of health care in New York City. There are presumably a number of actors there and in Washington whose views of the health delivery system are close to Alford's. What conflicts have they weathered, and with what results? How does one conflict relate to the one occurring a few years later?

My main problem with the book is its heavy normative style. Not because I disagree, but because I suspect it obscures part of the truth. Alford concludes pessimistically that even when reforms work, they are undercut by the pervasive tendency of the system to restore itself to an equilibrium containing the same problems as before. He writes: "If the institutional or class analysis is even partly correct, then a much more fundamental struggle to change American social institutions is called for, and this struggle will require the emergence of a social movement and political leadership which is not yet visible" (p. 266). Maybe. But I am left wondering. I tend to reject the idea that New York's history is one of all failures. Perhaps a more systematic "objective" effort to sort them out might yield a more complex picture of when and under what conditions the Medical Societies and the Big Hospitals win and lose. This in turn might give us a more sophisticated understanding of the nature of the system.

Nevertheless, I hope that this book is widely read and debated within the health research community, wince it is capable of stimulating further needed research.

ROBERT L. CRAIN

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Guide to U.S. Elections. By Congressional Quarterly. (Washington, D.C.: Congressional Quarterly, 1975. Pp. xvi + 1103. $47.50.)

The value of any reference book rests largely with providing easy access to useful information not readily available elsewhere. By these standards Congressional Quarterly's *Guide to U.S. Elections* is an outstanding achievement. We now have available from a single source exhaustive election data on presidential, congressional, and gubernatorial elections. (Southern gubernatorial and senatorial primary results are provided in a separate chapter.) The elections are compiled in a manner that facilitates brief forays into the volume in search of specific information. The editors have thoughtfully included candidate name indexes for each office, which are indispensable for tracing an intermittent political career.

The *Guide* is more than an occasional reference, however. For compulsive browsers who have tired of looking for Galbraith's Swiss chalet in their world atlas, the *Guide* offers a welcome change. For the first time we can celebrate some of history's most hapless creatures—the chronic losers. Ebon Moody Boynton passed through the bicentennial unnoticed except by the *Guide*'s browsers. In his own way Boynton was a remarkable man. Between 1878 and 1898 he lost five attempts for Congress under three different party banners. Moreover, of his three parties only the Democrats managed to survive Boynton's candidacy. The 500 or so party labels worn by congressional candidates are also among the more interesting artifacts of the giant archaeological dig that resulted in this book. Who, we can ponder, were Missouri's Benton and anti-Benton Democrats, and why did their internecine warfare persist for six years?

Students of American political development have in the *Guide* a valuable set of data made more attractive by the fact that its contents are available on computer tape from the Inter-University Consortium at Michigan. Perhaps the best way to describe its research attractions and limitations is with a sample of its contents. Consider the career of Edward Joy Morris. In 1843 Morris, running as the Whig candidate in Pennsylvania's 1st Congressional District, handily defeated two Democratic contenders with 45 percent of the vote. According to the *Guide* Morris failed to run for re-election. Nine years later in 1854, a Whig candidate with the last name of Morris lost to the incumbent Democrat by a three percentage point margin. Whether this was the same man is unknown, but it is clear that two years later Edward Joy Morris ran as the Union party candidate in the 2nd Congressional District and won by less than 2,000 votes. After serving two terms as a Unionist, Morris then switched to the newly formed Republican party and doubled his margin of victory over his Democratic rival despite the fact that the Union party fielded a candidate. Here his congressional career ended, and the district reverted to Union control.

What do we know about this politician that we did not know before, and of what value is this information for understanding American political development? *The Biographical Directory of the American Congress* which most closely resembles the *Guide* in its content also identifies the terms that Morris served in Congress and notes too that he began his career as a Whig. Moreover, it reports that he lost re-election in 1844, a fact which escapes the *Guide;* and that he resigned from the House in 1861 to become Minister Resident to Turkey. The *Directory* fails, however, to provide the election results or record that Morris changed his party affiliation twice. On the whole our knowledge about Morris' career is substantially improved with the *Guide*'s information, and when used in conjunction with the *Directory*, we obtain a thorough sketch of this nineteenth century politician.

With such information available for a large proportion of congressmen, a number of interesting issues about America's political development that have heretofore eluded systematic scrutiny can now be studied: (1) With more complete information about party changes we can investigate whether the leaders of new political parties came from existing parties or constituted a new set of elites who emerged from the "grass roots." In the case of Morris, the former path of leadership recruitment was followed; (2) What were the effects of parties—more precisely, party labels—on election outcomes? The level and intensity of partisanship during the nineteenth century has in recent years attracted much attention and disagreement. Perusing these returns one finds that for much of the century political parties were highly fluid and occasionally, as with Morris, less stable elements of the political environment than the politicians whom they endorsed; (3) Growing congressional careerism in the late nineteenth century has been credited with working important qualitative changes in Congress as an institution. With the names of losing candidates available here for the first time, we can begin to sort out the effects of competition and ambition on congressional turnover.

In addition to the 700 pages of election returns, the *Guide* presents useful historical articles on nominating conventions and congressional redistricting, and provides capsule descriptions of the prominent political parties. There are also numerous supplementary tables some of which, however, are little more than filler. Unfortunately, figures available in the Consortium's files on the partisan division of state legislatures have been omitted. Serving as a ready reference, as a browsing book, and as a data bank, C.Q.'s *Guide to U.S. Elections* is destined to become one of the most worn volumes on the reference shelf. Even at $50, it is a bargain.

SAMUEL KERNELL

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The President Is Calling. By Milton S. Eisenhower. (Garden City, N.Y.: Doubleday, 1974. Pp. xxiii + 598. $12.50.)

Milton Eisenhower is a rare and gifted public servant, to whom every president for over a half century, from Hoover to Nixon, entrusted major responsibilities. After beginning as a civil servant in the Department of Agriculture in the Coolidge era, Eisenhower was chosen by Franklin Roosevelt to design the Office of War Information in World War II and to establish the War Relocation Authority to effect the removal of thousands of Japanese-Americans from the West Coast following the attack on Pearl Harbor. His subsequent posts included membership on Truman's Commission on Higher Education and the chairmanship of the Presidential (Johnson) Commission on the Causes and Prevention of Violence. Simultaneously Dr. Eisenhower pursued a distinguished career in educational administration as president of Kansas State, Pennsylvania State, and Johns Hopkins Universities.

This is a rich memoir, abounding with the perceptive insights of an astute administrator, wise in its diagnosis of the ills of the modern presidency, and bold in its prescriptions. Dr.

Eisenhower provides candid assessments of the operating methods of each president with whom he served. Thus Kennedy is admired for his style, but less commendable was his failure to keep a promise to inform the public that he had asked Eisenhower and other distinguished citizens to serve on a committee to secure the release of prisoners captured by Cuba in the abortive Bay of Pigs invasion. After bitter abuse from leaders of the president's own party while Kennedy remained resolutely silent, Dr. Eisenhower dispatched "a brutally frank letter to the President, charging bad faith on his part." The letter was referred to the State Department; henceforth Dr. Eisenhower was persona non grata and "the White House was in no better standing with me" (pp. 371–72).

Eisenhower affirms the image of Lyndon Johnson as practitioner of the "two-shift" working day, volcanic in temper, and cruel to his associates. But Johnson was also one who, over three decades of association, never forgot that Eisenhower provided him his first exposure to a national audience, when, in 1942, as associate director of the Office of War Information, he invited Johnson to participate in a national radio program.

Dr. Eisenhower never concurred in Dwight Eisenhower's approving estimate of Richard Nixon's suitability for the presidency. Milton Eisenhower was always struck by the contrast between the private Nixon–"gracious, warmhearted, friendly, humorous, and devoid of pretense"–and the public figure–"austere, remote, often devoid of personality, given to self-praise, . . . 'Tricky Dick'. . . (p. 335). Nixon's disastrous presidency is explicable to Eisenhower largely in terms of George Reedy's classic analysis, *The Twilight of the Presidency.* Nixon readily indulged in his preferences for isolation, for avoiding Congressional leaders and distinguished citizens, his abhorrence of the press, and sequestration in his Florida and California hideouts.

Especially rewarding is Milton Eisenhower's account of his relationship with his brother, President Dwight Eisenhower. Their close and affectionate ties were nurtured by the warm family life of their childhood. Milton and Dwight were also philosophically compatible. Dwight found it helpful to explore his innermost thoughts and plans with Milton who eschewed subservience, who perceived himself as representing no special interest and having no selfish purpose, and who sought to raise questions and provide information that might be helpful to the president. Milton was careful not to bruise the sensitivities of department heads and White House staff, as he once had watched, with distaste, Harry Hopkins do as confidant to Franklin Roosevelt.

Dr. Eisenhower compares his relationship with President Dwight Eisenhower with the relationship of John and Robert Kennedy. Unlike the Kennedys, the Eisenhowers agreed that the president's brother should not take on a cabinet post since it would smack of nepotism and that Milton's public pronouncements might be discounted simply as echoing the president.

Long involved in organizational issues of the presidency, Dr. Eisenhower offers definite prescriptions for the office's improvement. He advocates a single term of six years for the president and accords great weight to the argument that this arrangement will provide better leadership response to public problems than the trimming and adjusting of a president preoccupied with his own reelection. He opposes the nation-wide primary as unduly favoring the candidate who commands substantial financial backing. Dr. Eisenhower calls for stronger reorganization legislation for the president's use and for the line-item veto which, he contends, would avoid much of the Nixon-Congress conflict over impoundment, a confrontation in which he argues, with much effect, Congress must be allotted a substantial measure of blame.

LOUIS W. KOENIG

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The Community and the Police–Conflict and Cooperation? By Joseph Fink and Lloyd G. Sealy. (New York: John Wiley, 1974. Pp. 216. $9.95.)

In this book Joseph Fink and Lloyd Sealy argue that conflicts between the police and the "community" (i.e., neighborhoods in which lower class blacks live) could be reduced considerably if police departments, rather than focusing upon traditional law enforcement activities, were to devote more of their efforts to mediating conflicts within the ghetto and providing services to its residents. They recommend, for example, that the police help welfare recipients to obtain delayed checks and tenants to get heat in their apartments, that policemen iron out disputes between merchants and local people and play basketball with neighborhood children (pp. 196 f.).

Since this book is less a work of political science than "police science," it would be unfair to criticize Fink and Sealy for failing to consider questions that their argument raises for political scientists. In particular, they fail to analyze who is likely to support and who is likely to oppose police reforms such as they recommend. But its failure to consider such questions does not mean that this book is of no interest to students of urban politics. Fink and Sealy's book presents a brief for the sort of innovative programs which are advocated nowadays by police reformers and liberals (its preface was written by Senator Birch Bayh), and which are funded by the Law Enforcement Assistance Administration. As such, it is useful to consider this book as a primary source—as exemplifying a point of view that is important in American politics today—and to ask what this point of view signifies.

Although on its face a recommendation that the police shed their authoritarian demeanor and perform services for their clients appears benign, there is something disquieting—and puzzling—about it. Fink and Sealy's proposals would greatly enhance the power of the police. The ombudsmen function which they want the police to serve is performed currently—if not always adequately—by public officials, party organizations, community action agencies, and little city halls, and the policy areas into which they want the police to intervene fall currently within the jurisdiction of departments of welfare, housing, recreation, and the courts. Such a comprehensive understanding of the responsibilities of the police, of course, is not unprecedented. Etymologically, the term "police" is related to "policy," and in public law the "police powers" of the state encompass the state's general responsibilities for the health, welfare, and morals of the public. But traditionally liberals have rejected the notion that the jurisdiction of police agencies is coterminous with the police powers of the state, and they have castigated regimes which grant such comprehensive power over public policy to the police as "police states."

American practice, to be sure, has not always coincided precisely with liberal theory. James Q. Wilson, whom Fink and Sealy cite frequently, speaks of a "service" and a "watchman" style of policing—styles in which police behavior is not oriented primarily toward law enforcement. But Wilson notes that the service style is characteristic of upper income suburbs—communities which are quite orderly and where the services the police perform are of marginal importance in the lives of the community's residents. For these reasons the police acquire little power over their clients by involving themselves in matters other than law enforcement in such a setting. In cities where the watchman style characteristically prevails, expectations concerning the appropriate level of public order are not especially high and informal social institutions—family, neighborhood, church, political machine—play a significant role in the maintenance of order. Thus, the police may be *arbitrary* in such cities but their power is not *extensive*. But in ghetto neighborhoods, the services which Fink and Sealy want the police to perform would be of great salience to local residents, and in these neighborhoods, informal institutions of social control are not able to establish a level of public order satisfactory to a large segment of the local and extralocal community. Fink and Sealy's proposals thus would give the police the resources to exercise considerable power over the poor in an environment where they would have an incentive to make use of that power. It must be recognized, then, that there is a mailed fist within the velvet glove of proposals for police reform such as Fink and Sealy advocate.

Why are liberals, who traditionally have been hostile to the police and sympathetic to the poor, prepared nowadays to support programs that will increase the power of the police over the poor? Clues to answering this question can be found in Fink and Sealy's book. First, many of the programs which Fink and Sealy advocate were, as they note, developed and funded by universities, foundations, and federal agencies. Second, as Fink and Sealy observe, the reforms which they wish to see adopted presuppose a new type of policeman: "the same basic manpower pool from which personnel are drawn for various social service agencies could just as easily provide recruits for police agencies" (p. 190). These two facts suggest that liberals might be prepared to ignore the implications which the reforms they support have for the power of the police because these reforms will enable the institutions which they control, and the classes for which they speak, to gain influence over police departments. And this explains why such reforms are resisted by old-line police administrators, by rank-and-file policemen and their unions, and by elected officials who represent working and lower-middle class constituencies. These groups recognize that such reforms threaten the control they currently exercise over the police and the monopoly they traditionally have enjoyed over jobs within police departments.

MARTIN SHEFTER

Cornell University

Political Issues in U.S. Population Policy. Edited by Virginia Gray and Elihu Bergman. (Lexington, Mass.: D.C. Heath, 1974. Pp. 212. $14.00.)

This reviewer has always had mixed feelings about edited compilations, particularly those derived from conferences, workshops, and the like. While, on the one hand, he has contributed chapters to several in the spirit (delusion?) that they represent productions of authentic scholarly importance, on the other he has found them to be of little value in his own research and teaching. Obviously not all edited works are the same, but most tend to present rather unexciting fare. Instead of pulling together carefully selected and edited pieces of original research or fresh new ideas unavailable elsewhere, they tend to contain either a disparate collection of contributions rehashing work already published or very preliminary notions in need of further thought. The crucial variable separating the former from the latter types would appear to be the editor. Unfortunately, the editors of *Political Issues in U.S. Population Policy* have not exercised their functions to the extent necessary to produce an effective, coherent volume. Growing out of a postdoctoral institute on population for political scientists sponsored in 1973 by the Carolina Population Center, the book is lacking both as an introduction to the politics of population in the U.S. for the uninitiated and as a significant contribution to the small but expanding body of literature in political demography.

Blending individually authored papers into a coherent, readable collection is, to be sure, a difficult task. It is virtually impossible without an introduction to delineate the common theme, establish an organizational framework, and relate individual chapters to the overall effort. Likewise a conclusion is helpful if not essential. This volume has neither. The only cues offered to assist the reader in assimilating the 11 contributions are the title and a brief statement in the acknowledgments to the effect that each of the contributors linked "his or her research interests to a specific problem in population policy" (p. xv). The reader must decide whether these are significant problems and how they relate to each other. Furthermore, the division of the book into three parts ("Population Policy and Political Analysis," "Empirical Studies of Population Impact on Public Policies," and "Political Implications of Population Policies") occasionally seems arbitrary if not downright confusing. Because of these and other editorial omissions, *Political Issues in U.S. Population Policy* as a whole suffers. There seem to be certain generalizations running through it—most notably the comprehensive, indirect nature of U.S. population policy and circumspect impact of demographic conditions on policy—but these are not effectively articulated.

In evaluating the individual contributions, one is struck, as is often the case with edited compilations of original pieces, by the diversity in approach and quality. This reviewer found the chapters in Part I especially confusing, both individually and collectively. In Chapter 1, Elihu Bergman begins by stressing the broad range of government activities affecting population but then narrows his focus considerably to the politics of federal family planning programs and abortion. Thomas Blau follows with an interesting, albeit somewhat disjointed, critique of Bachrach and Bergman's thesis on population policy making presented in their book *Power and Choice.* In the last chapter of Part I, Vijai Singh, a sociologist, argues for the value of cohort analysis in population policy research.

The promise of Part II is to shed light on the effects of demographic change—most importantly but not exclusively population growth—on public policies. The five chapters do indeed provide us with at least preliminary evidence, but they also demonstrate some of the pitfalls of the cross-sectional ecological analysis characteristic of the American state policy literature. John Wanat and Virginia Gray, Richard Feld, Clifford Kaufman and Bryan D. Jones, and David Caputo sift through a variety of data to find apparent relationships between demographic characteristics and such things as state government efficiency, the spending patterns of local governments, and differences in the utilization of revenue sharing. In contrast to these somewhat intuitive, data-oriented contributions, Gregory Rathjen's chapter constitutes an exemplary piece of research on the impact of population size and growth on the federal judicial system. In it the author carefully establishes the significance of his topic, lays out the relevant theory, describes the data, presents the analysis, and discusses his findings. While not the last word on the subject, Rathjen's work certainly provides important new insights.

Part III begins with a thoughtful essay on black attitudes toward population control. The author, Clinton Jones, treats the black community as a political subsystem and traces its suspicion of organized population programs to the historical and institutional racism of American society. He then uses content analysis to document variations in black attitudes. Although raising some interesting points, Virginia Gray's chapter on women and population policy lacks cohesion. In the concluding contribution, Stanley Hauerwas and L. John Roos take the dubious, at least debatable, position that the ethical issues at stake in population policy making ought to be made explicit.

Political Issues in U.S. Population Policy is not without its merits, particularly the chapters by Rathjen and Jones, as well as aspects of the other contributions. Taken as a whole, however, the volume has problems. With more decisive editorial direction, many of these problems might have been avoided.

TERRY L. MCCOY

University of Florida

Police-Association and Department Leaders: The Politics of Co-Optation. By Stephen C. Halpern. (Lexington, Mass.: D.C. Heath, Lexington Books, 1974. Pp. 127. $11.00.)

If union politics in general has been left primarily to economists and historians, the study of unions within public organizations has been, until very recently, considered the bailiwick of public administrationists and personnel scholars. However, just as the rise of militancy among craft, trade, and nonskilled laborers forced economists, historians, and some political scientists to notice the impact of such political organizing in the private sector, so the last decade, with its mushrooming spate of strikes by police, fire, sanitation, and education personnel has finally jogged the empirical interests, if not the analytical consciousness, of some analysts of the public sector. Further, the literature on police, regardless of disciplinary source, is all but bereft of anything short of citing the existence of police unions or associations. Stephen Halpern's detailed case analysis of three police associations attempts to fill the limitation in the literature of political science posed by the dearth of studies of public unions. Secondly it studies police politics from the "group perspective" rather than the present trend in the literature which usually studies the police officer as an individual.

More precisely he selects the cities of Baltimore, Buffalo, and Philadelphia and isolates the relationship between the office of chief executive of the police agency and the police employee organization as the major objective of analysis. Halpern contends that this relationship encompasses the bargaining and power variables which are important to the "goals and tactics" (p. 2) of both the police commissioner and the public employee group leadership.

From the beginning the author makes the case that the formation of police organizations was not only important in mobilizing agendas to enhance employee working conditions but, as in the case of Buffalo, also came to exert significant influence over the executive direction of the department. Of particular concern to the author are two issues: police accountability through the internal review process and civilian review boards and the attempts of police employee associations to exert control over the managerial decisions of the department and thus reverse the unilateral direction of policy determination.

In all three cities, as in many cities, the issue of civilian review was an important one. However, only in Philadelphia was the creation and function of a *working* civilian review board the focus of debate. From his analysis of the issue the author draws an important point, which I believe has broad-reaching value for the study of police in any city, namely, "it is crucial to understand that the opposition of police associations to these boards reflected far more than objection to that one method of civilian participation in police affairs" (p. 87). Indeed the mere spectre of a civilian review board acts as a lightening rod for police association arguments "that police possess unique skills, training, and experience that make it impossible for civilians to evaluate knowledgeably their performance. Civilian review, they argued, denied police the professional prerogative to judge their colleagues who had been accused of misdeeds" (p. 87). Halpern supports this conclusion with evidence from police association leaders who see as one of their long-term goals heightened professionalism of police—in much the same manner that exists in the legal and medical professions. In the trenches, this means the street-level patrolmen are seeking control of their profession. They want a say in the process of reprimand and review of behavior and control over everything from the initial selection of candidates to the training, promotion, and day-to-day procedures of the police officers. The commissioner and the police officers are united over the preservation of departmental autonomy from outside review; but they are certainly in conflict over the professional demands of police organizations which culminate in a struggle for managerial control.

The second major issue in Halpern's study is that of internal review. He finds the efforts to gain increasing departmental influence and control on the part of the police organizations, their call for heightened professionalism aside, lead to threats to truly objective review by an internal inspection office. Here Halpern puts some empirically interesting analysis behind the arguments of departmental whitewash.

The third area of interest is that of "managerial incursion" by the leadership of police associations into areas which, even under the host of superior-subordinate conditions, had been composed of a paternalistic, rigidly paramilitary and unilateral relationship between the executive officers of the department and the line officers. This section represents Halpern's most substantial contribution. As he indicates earlier, his approach to police is contrary to most of the literature in as much as he concentrates on the police association—or the line of officers acting in consort rather than as individuals. Just as importantly, where much of the literature of policy deals with conflict between the police and external groups of citizens, this section of Halpern's study concentrates on the growing internal conflict within police departments as the politics of police associations makes an impact. The discussion of both militant and conventional bargaining tactics uses cases where the police associations are strong (Buffalo and Philadelphia) and relatively weak (Baltimore).

In summary, the book provides some good empirical information on a significant new political pattern in urban areas. However, while it is strong on empirical insight and provides some new areas of interest for scholars of urban bureaucracy and politics, it goes no further. Like most of the literature in this area (case study or otherwise) it does not spend enough time establishing some larger theoretical or conceptual and historical standards of explanation for these patterns in the urban condition. The Halpern study is an intelligent, yet theoretically weak, addition to our primarily empirically bound literature.

DAVID PERRY

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Women in Politics. Edited by Jane S. Jaquette. (New York: John Wiley, 1974. Pp. 367. $14.95.)

Starting with the observation that it is the social science discipline which has responded with the least enthusiasm to the impact of the modern feminist movement, Jane Jaquette sets out to fill the apparent gap by presenting under one cover a series of recent papers and articles on women and politics. The result is a very valuable addition to the small but growing body of literature with a focus on the very challenging problem area of female political participation and consequent rewards. For those teaching courses on women in contemporary political life, this anthology will be an essential source book for years to come.

The parameters of the book are set wide indeed. Divided in two parts and four different sections, the first contributions examine the individual American woman as a voter and activist. Jane Jaquette's introductory essay offers a perceptive and sometimes bemused review of the earlier literature in the field. In the comment of the author: "The pattern of female participation in American politics seems to provide the occasion for a mild tut-tut or a studied ho-hum . . ." (p. xiv). To the degree that women do not take on male political characteristics, they are seen as weak, unable to overcome outworn stereotypes, and lacking participatory elan. By contrast, Jaquette suggests, to raise the question of female stake in politics will bring different issues to the fore. Quite simply, the kinds of reasons brought forth to explain lower levels of male political participation should be applied to women as well. Are women more alienated than men from the political system? Has pluralism removed from the political arena issues of first importance to women? Her concluding comments on the male bias in political science are pointedly ironic: "What starts out as an objective study of male-female differences all too often ends up as a diatribe against female weaknesses and a paean to male virtues" (p. xxxi).

The changing patterns of voting and political participation among American women come up for a thorough examination by Marjorie Lansing. Using survey data from the University of Michigan, she finds support for the hypothesis that changes in the structure of society have led to a redefinition of political roles for American women. The major shift in the political behavior of women has resulted from the general acceptance of the legitimacy of the female vote or, as she defines it, of a participant role as appropriate for women. The second part of her paper explores various dimensions of female activism and finds increased participation of women to correlate with college experience, youth, and labor force activity.

The effects of socialization and the conflict between motherhood and participation are the topics of papers by Lynn B. Iglitzen and by Cornelia B. Flora with Naomi B. Lynn. The conclusions of their separate studies—both based on interviews with a sample of respondents from their own communities—amply confirm the key role of sexist ideology suggested first in my own *The Silenced Majority*. Audrey Siess Wells and Eleanor Cutri Smeal proceed to take up the question of women's attitudes toward women in politics, starting with a provocative analysis of bias and shallowness in earlier surveys. Their own research data point to three meaningful and distinct categories of potential supporters for women politicians.

In the section titled "Women as Political Elites," Emmy E. Werner and Louise M. Bachtold examine the personality characteristics of female state legislators and find these—not surprisingly—to be more intelligent, more assertive, more venturesome, more imaginative, and more liberal in their attitudes than women in general. More importantly, no evidence was unearthed of ego defensiveness or anxiety produced by role stress among the women politicians surveyed. Mary Cornelia Porter and Ann B. Matasar next present a review and analysis of the role and status of women in the Daley organization. Though the research design is questionable, the dismal picture painted of stereotyping and bland dismissal of women by this powerful political machine is convincing enough. Mary M. Lepper's study of women in the federal career structure finds, on the other hand, some movement of women into higher echelons. There is, she suggests, sufficient and clear administrative machinery to cope with sex discrimination in government. Yet stereotyped notions holding women unsuitable for the top positions get in the way; her factor analysis of the career structure indicates that the hiring and promotion system for women is frequently based on technical competency, while for a man it is based on peer relations and anticipated long-range development. Sexist stereotyping is also and consistently found in Supreme Court decisions in Susan Kaufman Purcell's incisive analysis, titled "Ideology and the Law." The past cavalier treatment of sex discrimination cases now appears at an end, yet she points out that it is still not clear whether a majority of the Supreme Court will declare sex a suspect classification in the future.

Three contributions are included in Section 3, "The Movement: New Alternatives?" Both Nancy McWilliams and Warren Farrell focus on consciousness raising groups as distinctive of the new feminine style in politics and view these as a positive and promising political innovation. The attempts to avoid hierarchy in feminist undertakings are noted with but a few suggestions as to its attendant problems. Ironically, in Jo Freeman's provocative critique, "The Tyranny of Structurelessness," it is just this favored mode of operation within the new feminist movement that invites a new tyranny. She finds it necessary to restate the obvious: Power is not abolished by deliberately avoiding selection of who shall exercise it; all this does is abdicate the right to demand that those who exercise power and influence be responsible for it. The diffuse power within the feminist movement insures only that the movement becomes totally ineffective in the political realm, not that any group or person is prevented from dominating it.

Part II, Comparative Perspectives, starts out with two highly interesting explorations of women's changing status and roles under communism. Barbara W. Jancar demonstrates that communist ruling groups had to break up the traditional family structures and mobilize women into the work world in order to achieve the objectives of economic development. Yet little attention was given to revolutionizing the female status; the Chinese Communist leaders appear to be the only ones who believe that a radical approach to the question is essential to the modernizing process. Women under Communism, as a consequence, have been unable to exercise any real power and are still being conditioned to identify themselves primarily in the traditional subordinate roles. Gail W. Lapidus reaches similar conclusions in her carefully designed study of the Soviet pattern of modernization and its impact on women. The Soviet experience, she suggests, calls into question the Marxist model of social change; it is the character of the development process rather than development per se that is decisive in shaping women's roles.

Among the remaining contributions we find a succinct summary of the Marxist analysis of women and capitalism, authored by Temma Kaplan, and a somewhat inconclusive case study of mobilization of women in Allende's Chile, authored by Elsa M. Chaney, suggesting the failure of the radical government in this area. Another case study from Costa Rica by Jo Ann Aviel demonstrates the important role that can be played by the school in the political socialization of women. "Memsahib, Militante, Femme Libre" is the intriguing title of an essay by Judith Van Allen which briefly describes the impact of colonialism and modernization on African women and analyzes three types of responses women are making to the dislocation produced by that impact.

Kay Boals' article, "The Politics of Cultural Liberation," in turn touches on the important and complex question of the liberation of the female self within the context of struggle against cultural domination by an outside group. The revolutionary's dilemma—to be pulled in two opposed directions toward reassertion of traditions on the one hand and toward the creation of a modern developed society, on the other—proved to be particularly painful in the realm of male-female relations in Algeria. Boals finds that a growing number of Algerian women are eager to change the traditional patterns of female seclusion and male dominance, yet are inhibited from doing so by the internal psychic ambivalence created by the desire to affirm the Algerian heritage and culture.

Mary Cornelia Porter's bibliography is the final and valuable addition to this timely anthology. Though the contributions are not even in quality and a more rigorous editing job could be wished for, there is no doubt that *Women in Politics* contains sufficient rich and provocative material to recommend it to anyone who does not insist on remaining blind to the professional and political challenge suggested by the title.

KIRSTEN AMUNDSEN

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Mayors in Action: Five Approaches to Urban Governance. By John P. Kotter and Paul R. Lawrence. (New York: John Wiley, 1974. Pp. 287. $14.50.)

As befits two Harvard professors of organizational behavior, the authors' primary focus is behavior. They want it understood that this work is interdisciplinary, "not a political science essay." Their report is the result of a two-year exploratory, interdisciplinary, comparative examination of the behavior of 20 mayors no longer in office and of their administrations during the 1960s. What do mayors do? Why do they do that? What impact does such behavior have on their cities?

Can a general purpose "model" of mayoral behavior be constructed that can describe the different ways in which mayors behave, explain why a particular behavioral pattern emerges in a specific situation, and predict the impact of a mayor's behavior on his city? Pointing out that such a general purpose model does not currently exist, the authors proceed to develop one. But to the uninitiated, the process is tortuous, and the "average" mayor (my model, not theirs) would find the reading difficult.

Social scientists, however, will discover this to be a solid work and should be particularly interested in the research methodology and design. It includes an impressive amount of field work.

After a complete examination of all literature on mayors, the authors re-examine the more serious writings to identify what are at least implicit as 10 different models of mayoral behavior, although most are not stated or written as a model of mayors' behavior as such. Examples are the Power Broker model, derived from Edward C. Banfield's *Political Influence* (New York: Free Press, 1961), the "Muddling Through" model, from David Braybrooke and Charles E. Lindblom's *A Strategy of Decision* (New York: Free Press, 1963), the Multihat Role, from Leonard I. Ruchelman, ed., *Big City Mayors* (Bloomington: Indiana University Press, 1969), etc.

A personal judgment evaluation of each of the 10 models in each of the 20 mayoral situations studied in the inquiry led the authors to conclude that while each model has a certain limited usefulness, often complementing and supplementing one another, none is entirely satisfactory. By defining six of the models as focusing on the behavior of the mayor, the other four as treating the context in which the behavior occurs (contextual variables), a new framework was developed which, in a sense, shows how the ten models "fit together." In this framework a mayor's behavior can be seen as occurring in three different but somehow interrelated processes: in one the mayor decides what to do—sets the agenda; in another he builds and maintains a network of relationships needed to pursue the agenda; in a third he accomplishes tasks that are on his agenda.

These three processes of the mayor's behavior—agenda setting, network building, and task accomplishment—occur in a context of at least four variables: the mayor's own personality, the formal structure of the city government and the mayor's role in it, the distribution of power in the community, and the city environment itself. This context affects the behavior and in turn is affected by it.

The first three chapters of the book are devoted to the authors' rationale for this new framework. The next three describe a variety of actions they found the mayors to be using for the three key processes of agenda setting, network building, and task accomplishment. A chapter then identifies five mayoral types, each with a different behavioral pattern as defined by those three key processes. The five types of mayor are called: Ceremonial, Caretaker, Personality/Individualist, Executive, and Program Entrepreneur. Of the 20 mayors studied, "approximately" four representatives were found of each type.

The model that has evolved up to this point describes how mayors behave and how short-run dynamics and constraints both affect and are affected by such behavior, concentrating on relationships between the processes and the contextual variables. It is completed with a chapter on "Pattern Dynamics," with emphasis on system dynamics in the long-run. Relationships among the four contextual variables themselves are examined and found to be such that if any two are not "aligned," the consequences eventually create problems for the mayor. If he cannot somehow eliminate the nonalignment, people generally look to the electoral process to correct the situation by replacing the mayor.

If all four contextual variables of a mayor's system are aligned simultaneously, we have a state of "coalignment," a condition which none of the 20 mayors in this study attained, although some came close. Relationship between election results and coalignment results, as depicted in an interesting chart (p. 140), appears to be fairly clear. This issue of coalignment is important and is explored further in some of the remaining chapters, using mayoral behavior is complete. In a later chapter on "Implications for Social Scientists" the authors suggest some implications of their coalignment model for further research on administration.

If mayors would take the time and effort to assimilate this book, they would discover a number of findings from the research that have practical relevance to their efforts to be effective. In a chapter, "Implications for Mayors," the authors describe seven that seem to be particularly important, although they admit that none is currently an accepted part of "conventional wisdom." This leads to the final chapter, "The Future: Some Disturbing Trends," which indicate a bleak prospect in view of current trends affecting cities and their populations. The conclusion of Kotter and Lawrence: *"Unless subsequent events change or alter these trends, our model predicts an urban crisis of unparalleled size and scope."*

PATRICK HEALY III

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Transformations of the American Party System: Political Coalitions from the New Deal to the 1970s. By Everett Carll Ladd, Jr., with Charles D. Hadley. (New York: W.W. Norton, 1975. Pp. xxv + 347. $12.50, cloth; $4.95, paper.)

Ladd and Hadley begin by reminding the reader that "when the priestess of Apollo at Delphi made ready to deliver an oracle, she positioned herself upon a special seat supported by three legs, the tripod." This book reports their panoramic "view from the tripod."

The first half of the book uses the concept of the "agenda" (which Ladd developed in his *American Political Parties*) to explain the society's shift during the 1930s away from a business-directed society and economy (business nationalism) to an emphasis on the government's responsibilities for the direction of economic life and individual welfare (governmental nationalism). The agenda of government nationalism is used to explain the movement of blacks and Jews into the Democratic party; it is used to explain the intensification during the 1930s of a nascent class cleavage in partisanship; and it is used to explain the apparent continuity of both the sectional and religious dimensions of party support.

The social group character of the party coalitions and the agenda of governmental nationalism also explain why the expansion of the middle class and the growth of suburbia did not presage a decline in the Democratic party. The expected Republican majority did not emerge, according to Ladd and Hadley, because the partisanship of the social groups (religious, racial, sectional) was only weakly predicated upon social status. Then, too, the postwar middle class was not the entrepreneurial middle class which had supported the Republicans since the Civil War, but an increasingly professional middle class with a symbiotic relationship to the liberal state and government nationalism.

Their analysis of the Southern Democracy is one of the most interesting to be published in recent years. Using older Gallup data, Ladd and Hadley document substantial southern support for the welfare programs of the New Deal during the 1930s and 1940s. The data provide a benchmark against which they chart the political movement of the South, as the Democratic party extended the application of governmental nationalism to include race relations and as the South experienced economic changes which undermined its sympathy for the policies of the national Democratic party.

The second half of *Transformations of the American Party System* uses the agenda concept to explain the withering of the New Deal system. As Ladd and Hadley describe it, the current Democratic-Republican division is the party system of an industrial society in which the major questions revolve around the distribution of material benefits. They believe that the dealignment/realignment of the parties is a result of the fact that America, sometime over the past two decades, began to evolve into a postindustrial society. The agenda of this postindustrial society, which has substantially resolved economic needs for all but a fraction of the population, is social and cultural questions; and the "leading class" which defines the agenda is the new, enlarged middle class. The principal importance of the enlarged middle class is that it has magnified the power of the intellectual stratum and made it possible for them to wield greater influence over the content of political debate than they have ever previously enjoyed.

Ladd and Hadley conclude that the future of the party system is being molded by a declining concern with industrial society welfare issues and an expansion of a self-conscious intellectual stratum which uses the communications system to perform many of the educative and political tasks previously reserved for political parties. The symbolic politics of 1972 are, in their view, a harbinger of a future in which organized activists will frequently capture presidential campaigns in order to proselytize their vision.

On the whole the first part of the book is one of the best descriptions in print of the social group basis of the New Deal and the issue agenda that provided its raison d'etre. It has only two problems. The abandonment of the Republican party by the upper status segment of the population seems much more marked in their Gallup data than in the SRC data for the 1952 to 1960 period. Whether the data are really in conflict or only reflect different definitions of what constitutes the upper class (Ladd and Hadley are not very clear about such things) is impossible to determine. Whatever the explanation for the difference, it seems certain that the declining Republicanism of the middle and upper class is not as large as Ladd and Hadley indicate in their analysis. A second problem is their attempt to show that the emergence of the Democratic majority did not depend upon the mobilization of a new group of voters in the 1920s and 1930s. The problem with the analysis is that it begins in 1940, although the process of mobilization of new Democratic voters began a couple of decades before 1940 and largely ended by that year. Also, the mobilization which created the New Deal is based on a complex interaction of party identification and turnout rates, and simply examining party identification and voting choices after 1940 will not pick up the effects. To sustain their thesis, they would have to refute the data in *The American Voter* (pp. 153–56) and *The Changing American Voter* (Chapter 5) which show that Democratic ascendence in the 1930s depended upon large numbers of latent Democrats, both young and old, voting for the first time.

Finally, the application of the postindustrial society model to explain changes in the New Deal party system seems to be a procrustean exercise. It is beyond question that there are a new set of social issues that are frequently orthogonal to the prevailing party cleavage, and that they are playing a significant role in the ongoing realignment of the parties. Unfortunately, one of the least postindustrial issues, race, is principally responsible for the altered partisanship of the South, the region which contributed about 50 percent of all of the observable change in the social basis of the parties since 1960. Also, there is the fact that at this writing (October 1976) New-Deal-style welfare issues are at the core of the current presidential election. Which is a model for the future, 1972 or 1976?

However, the value of the book does not depend upon the accuracy of the authors' predictions about the future of the party system or about the importance of postindustrial politics to the declining hold of the parties on the electorate. The book should be read because its interpretation of the social and economic infrastructure of the New Deal system is insightful, and because it provides a suggestive explanation for the social changes that are bringing that system down.

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Urban Outcomes: Schools, Streets, and Libraries. By Frank S. Levy, Arnold Meltsner, and Aaron Wildavsky. (Berkeley: University of California Press, 1974. Pp. x + 278. $12.50.)

This is the third book from the Oakland Project, following Arnold Meltzner's *Politics of City Revenue* and Jeffrey Pressman and Aaron Wildavsky's *Implementation.* Many more are on the way. *Urban Outcomes* adds to our knowledge of Oakland using a reasonably traditional case study approach, here baptized "close causation" (p. 17). It is a useful if limited study. Its focus is "urban outcomes." In the three service areas studied, these outcomes include (1) for schools: teachers' salaries, student/teacher ratios, and costs per student; (2) for streets: resurfacing rates and street construction projects; and (3) for libraries: book circulation rates. The outcomes are all analyzed by neighborhood area within Oakland.

The authors essentially present a "social audit" of the sort James Coleman has called for: tracing budgetary commitments through the service production process to the provision of teachers, streets, and books. Given the sparse literature on these processes, the book justifiably includes substantial descriptive detail for each service area. But the empirical work stops short of considering actual impacts on citizens.

The book deserves careful reading for such matters as capital expenditure decisions by the street engineers, the emphasis of the librarians on hiring highly qualified librarians (in contrast to more books), and the way that extra-local funding helped move school resources more toward the poor. The reportage is excellent. The writing is sharp and clear. We have come to expect no less from Wildavsky and company.

Mixed with the empirical work is a strong normative brew. The authors, reflecting the concern of the late 1960s with equality in service delivery, are intent on documenting distributional differences according to income and (occasionally) race. They find some, but remarkably few. Coming from Berkeley, the authors are ultra sensitive to the political (primarily distributional) aspects of each decision. The Oakland people are not. The school administrators, and especially the street engineers and librarians, generally adhere to their professional norms in resource-allocation decisions. Operationally, this means (re)building streets where traffic flows are greatest, and providing books to book borrowers in general response to their requests. School resources are allocated more directly toward equalizing resources per student. The strongest sections of the book are those where the decision rules leading to these patterns are made explicit. The professional bureaucrats are presented, reasonably convincingly, as the key decision makers. This stable picture of repetitive decision making is, not surprisingly, fully incrementalist. One imagines that, if the authors had instead selected certain more controversial decisions, more input from nonbureaucratic actors would have been apparent. But what we are shown is surely useful.

The authors are also concerned with answering "who benefits?" Two general types of criteria have been used in the literature: those originating with researchers and those originating with the persons they study. The authors are sensitive to abandoning criteria which might derive from the citizens of Oakland. They try, a little. Meltsner's earlier book included an outstanding chapter on the remarkable absence of opinions on tax policy even by politically active Oakland citizens. Here too some fragmentary material suggests that general citizens have very few clear ideas concerning specific policies in these three service areas. Current advocates of "policy maximization," "issue politics," and the like should find these sections of the Oakland reports chastening.

A second plausible answer to who benefits might come from analyzing interest groups and their inputs to the political process. A widespread view is that more affluent citizens participate more in the political process and correspondingly receive more policy outputs than the poor. This book does not provide much direct detail (although other Oakland Project reports do) to show that this is not the case in Oakland. The reader may first react with doubt, but gradually accept the thorough-going "professional" character of Oakland politics. With perhaps Chicago or New Haven in mind, the authors write, "public works elsewhere may be a pork barrel, but in honest Oakland street decisions are straight" (p. 163).

At this point, other researchers might have sought to explain the distinctive political culture of Oakland leaders and citizens which maintain the legitimate this style of decision making. But "reform government," "public regardingness," the "Irish ethic," or "non-ideological particularism" are not used here. They might have entered if the authors had sought to place Oakland on the national urban map. A systematic comparative study is unnecessary for this; comparisons with other case studies and comparative work could be used, particularly that of Robert Lineberry, Elinor Ostrom, and Harry Hatry.

Instead the authors leap to their own normative typology. It then is used to interpret their findings and suggest policy implications. Their typology is a threefold classification of policy outcomes. First is what they call simply "the more, the more"; their two defining examples indicate its vagueness: "Roads go only where the cars already are. Experienced teachers transfer to the well-to-do schools" (p. 224). Thus lumped together are policies which positively favor the more affluent as well as policies where a user charges principle is applied. This vagueness is not accidental; it derives from ignoring the inputs (here primarily fiscal) to Oakland's government from the neighborhoods whose outcomes receive such detailed attention. The authors are appropriately apologetic on this point (p. 21); Henry Aaron, John Weicher, and others have developed straightforward procedures for estimating intraurban fiscal burdens which could readily have been applied in Oakland. Perhaps they will be in the future.

Their second policy type is "compensation" whereby poor neighborhoods receive larger resource allocations per user (e.g., extra teachers) than more affluent neighborhoods.

The third type is "resultants." Here the policy is allocated with sufficient compensation to make the impacts identical across neighborhoods, e.g., identical test scores by children in all neighborhoods. The authors affirm in a tortuous concluding chapter that they themselves prefer more redistributive policies in Oakland (less No. 1 and more No. 3). But they then go on to point out how probably such policies would fail for many reasons, the most immediate being that the affluent could simply move out of Oakland.

A logical weakness of the whole argument, however, becomes more apparent at this point. Why impose these three ideal types on Oakland and then take a strong personal stand? Why not attempt to see how Oakland citizens (and leaders) view these issues? The argument (in Chapter 1) was that citizen preferences could be ignored because citizens had few opinions on the specific service issues studied. This may be true. But it does not follow that the same applies to views on these three very broad types of policies.

The authors seem inordinately critical of other research approaches, such as comparative analyses of cities. These, they suggest, "do not identify particular levers which might be used to alter policy outcomes." For example, after one finds that cities with numerous Irish Catholic citizens have large city government budgets, they ask, "if a community wants to increase its budget, should one suggest that it import Catholics from Ireland?" (p. 11)

By contrast, these authors are sufficiently convinced of their own ethical views to essentially ask citizens of Oakland to adopt their general principle of dramatically increasing redistribution, and accordingly to reorganize the entire city government. Is this policy analysis or political philosophy? Surely one alternative would be to document the ethical views of different ethnic and income groups within Oakland (blacks if not Irish) and then to point out how the city government might be reorganized to become more responsive to these different groups within Oakland. They might thus suggest better means to an accepted end; instead they suggest new ends.

Still, it is likely that *Urban Outcomes* will be read more outside than inside Oakland; political scientists with an interest in service delivery cannot afford to ignore it.

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Against the Law: The Nixon Court and Criminal Justice. By Leonard W. Levy. (New York: Harper & Row, 1974. Pp. xvi + 506. $12.95.)

If anyone reading this knows someone who religiously clings to the belief in the immaculate conception of constitutional policy making, steer him/her to Leonard Levy's book on the Nixon Court and criminal justice. For what Levy does in his meticulously wrought analysis of the 1969 through 1972 Terms of the Supreme Court is to demonstrate beyond a reasonable doubt that profound changes in criminal law have occurred since the end of the Warren Court and the advent of the Nixon Court. Levy provides abundant and persuasive qualitative evidence that the Nixon Four plus White (and sometimes Stewart) are pro-prosecution conservative activists. He shows that true to the activist tradition they have brought about a constitutional revolution to further their policy preferences.

Levy's book is aimed at a general audience and in the first chapter he goes over ground familiar to specialists in the field, e.g., a summary of the accomplishments of the Warren Court, selection politics, and a sketch of the justices with particular attention to the Nixon appointees, and a brief overview contained within a discussion of "strict construction" of the wide latitude judges have in fashioning constitutional policies. Once over the preliminaries, specialists will have a field day with the subsequent six chapters as an uninhibited Levy lets few sleeping dogmas lie (a Levyathan pun as is the title of his book) and subjects to most searching scrutiny the decisions of the Nixon Court concerning the criminal justice guarantees of the Bill of Rights. Each opinion of the Court along with concurrences and dissents is dissected in terms of its reasoning, handling of precedents and history, the persuasiveness of the arguments, and the way opposing viewpoints of colleagues are handled. In the concluding chapter Levy even brings in a primitive form of bloc analysis to provide a quantitative complement to the qualitative analysis.

No justice is spared from Levy's critical eye although the Nixon Four plus White do (deservedly) win more poor marks than the somewhat more moderate (in terms of opinion behavior) Stewart and liberal activists Douglas, Brennan, and Marshall. For example, Burger is char-broiled for his *Harris* vs. *New York* opinion as "surely one of the most scandalous, extraordinary, and inexplicable in the history of the Court" (p. 149). Burger, among his other sins in Harris, misrepresented a vital fact in the case and no one, not even the outraged dissenters, caught it. Powell gets a professorial spanking for his *Kastigar* v. *U.S.* opinion for "having contradicted history and garbled the precedents" (p. 183). White doesn't make right in the case which legitimized six-person juries, *Williams* vs. *Florida,* and Levy's indictment charges White with a "hodgepodge of bad reasoning and guesswork" (p. 267). And so on.

But Levy is not always anti-prosecution. For example, with the 1972 death penalty decisions Levy finds after a lengthy and detailed examination that the decisions are "wrong as a matter of law and history" and "wrong in reversing irrevocably the sentence of death for many who were justly convicted without caprice or unfairness" (p. 418). (But Levy fails to reconcile this latter point with his more realistic earlier evaluation of the inherent institutionalized unfairness of the criminal justice system particularly for the poor.)

Levy's overall assessment of the Nixon Court's work in the criminal justice sphere is damning. He concludes that the Nixon Court

> reinterprets precedents, distinguishes them away, blunts them, obliterates them, ignores them, and makes new law without the need of overruling or being bound by the past.... It cultivates the illusion, suitable to the image of a conservative court, of having some respect for precedents; at the same time, it narrows them until they become meaningless and moribund (p. 423).

What the justices are engaged in is "result-oriented adjudication which is a corruption of the judicial process" (p. 438) and constitutional law today is essentially the embodiment of the prosecution-oriented attitudes of the Nixon Court. This leads Levy to his ultimate denunciation: "The lawyers who today constitute the majority of the Court in most criminal-justice cases are no damn good as judges" (p. 439).

Levy's indignation, however, seems out of joint with the realistic appraisal of constitutional decision making offered in his first chapter where he recognizes that:

> inevitably, then, our constitutional law is subjective in character and to a great degree result-oriented. We may not want judges who start with the answer rather than with the problem ... but as long as mere mortals sit on the Court and must construe that majestic but muddy Constitution, we will rarely get any other kind" (p. 35).

It seems then that Levy's real gripe is that the Nixon Court majority gives the Bill of Rights a less generous reading than he does. I am skeptical that Levy would accept the results of decisions he most bitterly criticizes if only they were brilliantly argued. Neither do I buy the argument that poorly crafted opinions sap public confidence in the Court (note the increase in positive ratings in the polls during the 1970s). It would have been more consistent with much of his earlier argument had Levy in his conclusion questioned the Nixon Court's criminal justice policies qua public policy or had he stressed that he would like to see these philosopher-kings be more philosopher and less king rather than condemn result-orientation.

In brief, Levy's qualitative analysis is first rate and, by the way, supports the quantitative work that has been done on the Nixon Court. The methodology, although traditional, provides in the expert hands of this leading constitutional historian a public policy perspective that is as compelling as it is timely—as the Nixon Court revolution continues unabated.

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Federalism and Clean Waters: The 1972 Water Pollution Control Act. By Harvey Lieber in collaboration with Bruce Rosinoff. (Lexington, Mass.: Lexington Books, 1975. Pp. xiv + 288. $18.50.)

Without exaggeration, Professor Lieber describes the Federal Water Pollution Control Act Amendments (1972) as "the most ambitious and expensive pollution control requirements to be promulgated into law." But was the act also a massive, and unnecessary, federal displacement of state pollution control authority and, thereby, a repudiation of the Nixon New Federalism supposed to govern administration policy? In a welcome effort to evaluate environmental policy in terms of broad disciplinary concerns, the author places the law squarely in the context of American federalism and asks: (1) "Was the Act really necessary—were the states, the key implementers of water pollution controls, unable to unwilling to clean up their waters?" and (2) if federal action were necessary, ". . . are the provisions of the law likely to fill these needs and improve specific program inadequacies of the states?" Five states are selected for "intensive" impact study: New York, Mississippi, Texas, Washington, and Wyoming.

The author suggests that the justification for federal action is "mixed." Among the states studied, they found Wyoming was "needlessly forced to install sophisticated monitoring devices" and Texas had to "unnecessarily reshuffle" a well-developed program. About the law's general effect, the author concludes:

"...the Act will help to spur the more retarded states, have a mixed effect on those with developed programs, and have a negative effect on a few like Wyoming." The impact analysis, however, appears to be the least substantial portion of the work. The significant aspects of the work lie in a different direction. By treating the Act as "a case study in the New Federalism," the author offers a carefully detailed and insightful explanation of why the New Federalism failed to appeal to those ostensively committed to it. "Despite the rhetoric," writes the author, "the commitment by governors, Congress, and the administrative agency to a federal partnership in practice was not deep" (p. 196). The author describes how Congress, governors, the White House, and the EPA engaged in a somewhat unwitting, but highly successful, effort to frustrate the New Federalism and why the states, in particular, were persuaded to embrace an Act which largely rejects in practice the New Federalism. Another secondary, but important, theme throughout the book is how, and why, the U.S. Environmental Protection Agency came to exercise enormous autonomy in implementing the program—an excellent study in the evolution of administrative discretion. "Congress' role [in implementing the Act] seems to be reduced to that of a frustrated, sometimes angry bystander," writes Professor Lieber; the EPA, he concludes, exercises "almost total control over the [implementation] process" and enjoys the ability "to further tighten requirements over state and local governments" (pp. 123–24).

The policy impact assessments are unsatisfactory for several reasons. Although published in 1975, the empirical materials on state water quality and implementation of the Act appear to have been gathered no later than 1973—far too early for confident judgments about policy consequences. A more useful source on impact now is the multivolume Report of the National Commission on Water Quality, published in 1975. Moreover, many judgments about policy impacts contained in the book's Appendix seem purely speculative, as, for example, those relating to "1976–76 Needs" and "Effect of 1972 Amendments" in each state; evaluative criteria and data, in any case, are not readily identified. Data supporting many conclusions seem inappropriate. Thus, the author's material on the "Extent of Water Pollution Problems" in each state—a critical baseline data for assessing the "need" for federal intervention—comes from a 1960 water quality survey and takes no account of potentially important water quality alterations in the many years intervening before the Act was passed. Often, data seems irrelevant to conclusions. In a detailed study of the Texas water quality program, for instance, we are told that "the most pressing Texas water pollution problems are caused by municipalities" (p. 162); but the figure and table supporting this assertion reveal no such information. One wonders, then, upon what basis the conclusion is justified that "Texas was doing quite well without the necessity in most cases for additional federal requirements. . ." (p. 192). Such characteristic flaws gravely diminish the force of the author's argument that federal intervention in state pollution control programs was often unjustified.

In short, one should read this book for its informed and enlightening discussion of how the New Federalism miscarried and for its insights into the development of administrative discretion in water pollution management. Although the author has asked very significant questions about the impact of the Act upon the states, the questions still await more persuasive answers.

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Congress Against the President. Edited by Harvey C. Mansfield, Sr. (New York: Praeger, 1975. Pp. viii + 200. $15.00, cloth; $5.95, paper.)

Of all the manifestations of the past decade's frenetic political events, the struggles between president and Congress have been among the most intriguing and productive of profound changes. The twin lightning rods of Vietnam and Watergate have invested new meaning to Edward S. Corwin's characterization of the Constitution as "an invitation to struggle," altering (at least for the time being) the character of legislative-executive relations. Political scientists, no less than others, have had to relearn generalizations and reassess assumptions about these institutions.

Now we are busy consolidating our information and insights concerning these new developments. After all, nearly everything written before, say, 1970 about seniority, the Rules Committee, the filibuster, the budgetary process, appointments and confirmations, and the war-making and treaty powers is acutely in need of revision. Changes have been so swift that journalists rather than scholars have led in announcing them. Thus Mansfield's collection of papers is timely and useful. However, considering the APSR backlog, by the time this review appears the collection will itself need updating.

Inevitably, the collection is something of a mixed package, but it is sufficiently rich that it can be recommended for the classroom or for general enlightenment. And, happily, the contributions are highly readable.

Picking favorites among the essays is bound to be a personal matter; in my case I tended to be drawn to pieces on subjects about which I was least familiar. One of the finest articles is Allen Schick's "The Battle of the Budget," a primer on budgetary politics, the causes of intensified budgetary conflict, and the altered rules of the budgetary game. Schick has done this sort of thing before, but his explanations are so authoritative, his observations so insightful, and his writing so lucid, that even close students will profit under his tutelage. One hopes he will soon furnish us with a book on the budgetary process.

Another intriguing essay on a complex topic, Richard Pious' "Sources of Domestic Policy Initiatives," reminds us that presidents have no monopoly over policy initiation. Indeed, although many factors propel presidents toward the Initiator-in-Chief role, there are often equally compelling reasons for avoiding such a role. Pious details the sources and channels of policy initiation in both the executive branch and Congress.

Pious draws several useful distinctions in a field where distinctions ought to be made more carefully than they have been in the past. He distinguishes between first- and second-stage innovation and appears to differentiate between innovation and initiation—although this latter is not clear. As with Schick's article, Pious' contribution seems an abbreviation of a longer argument, which I look forward to reading in detail.

Other informative chapters cover such diverse topics as senatorial confirmation of appointments (Ronald C. Moe), oversight of intelligence agencies (Harry Howe Ransom), Congress and foreign policy (Edward A. Kolodziej), and congressional staffing (Harrison B. Fox and Susan W. Hammond). Mansfield's own essay sets the tone of the volume by describing the dispersion of power within Congress—a development whose implications are addressed by some of his colleagues but not by others.

As usual in such collections, the most stimulating portions are those that invite controversy. For me these included John G. Stewart's discussion of congressional leadership and Robert G. Dixon's views about separation of powers, including executive privilege.

Stewart's informative and thoughtful essay details the growth of central party organs in Congress from the vantage point of one who has observed them first-hand. He tends to identify the changes—especially caucus controls over committee appointments—with the notion of party government. "By 1975," he writes, "the visible trappings of a responsible congressional party system were in place" (p. 31).

Yet these party instruments, while admittedly stronger than a decade before, have yet to be used extensively for policy initiation or development. (Stewart's favorable view of the congressional Democrats' foray into integrated energy policy making, for example, will not be shared by everyone.) Moreover, as Stewart notes, the caucuses have ironically been resuscitated at the very time when parties appear to be withering at the grassroots, leaving most candidates and incumbents to fend for themselves. The parties' policy-making apparatus lies largely dormant.

In fact, caucus resurgence is probably transitory, linked less to "party responsibility" than to changing factional politics. Outnumbered in the House and Senate chambers, where conservative Democrats could align with Republicans to checkmate liberal initiatives, the Democrats' growing liberal wing turned to the caucus as the instrument to pry open the seniority system. Caucus activism could conceivably persist, but I tend to be less optimistic than Stewart.

Dixon's essay demonstrates that the executive force theory has not been demolished by the past decade's events. His discussion of congressional vetoes is informative. He properly distinguishes between "committee vetoes" of doubtful constitutionality and the congressional veto provisions of reorganization acts, the function of which is clearly legislative rather than implementation. Yet these are matters of degree; after all, Congress has been delegating matters to its committees at least since the early nineteenth century. A more telling objection to committee vetoes, in my judgment, is that they can enmesh committees and bureaucrats in paperwork, even precluding discretionary probes into issues of genuine concern.

More questionable is Dixon's expansive reading of the so-called "separation of powers" concept, and hence his eagerness to defend executive assertions of privilege. He is wide of the mark in asserting (p. 139) that "Congress has an instinctive thrust toward control of the Executive." And I believe his view (p. 148) that "the courts, it seems, can save the nation from an imperial President" misreads Watergate's meaning. It is wise to recall Corwin's admonition that courts can only create vacuums, while others must fill them. Otherwise, what can save us from an imperial judiciary?

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Yeas and Nays: Normal Decision-Making in the U.S. House of Representatives. By Donald R. Matthews and James A. Stimson. (New York: John Wiley, 1975. Pp. xiv + 190. $14.95.)

Yeas and Nays is an important addition to the growing body of literature on congressional decision making. The fundamental problem addressed by Matthews and Stimson is how can congressmen vote on bills in a reasonably rational fashion given the number, scope, and complexity of the issues confronting them and the insufficient time and resources available. After reviewing and rejecting a number of models of legislative decision making including voting, the constituency, precedent, and ideology, Matthews and Stimson posit cue taking as the conscious strategy that congressmen adopt to cope with their decision-making burdens. A cue is defined as "any communication—verbal or nonverbal—intended or unintended—that is employed by the cue-taker as a prescription for his vote" (p. 51) and the essence of the cue-taking model is given in the following statement:

> When a member is confronted with the necessity of casting a roll-call vote on a complex issue about which he knows very little, he searches for cues provided by trusted colleagues who—because of their formal position in the legislature or policy specialization—have more information than he does and with whom he would probably agree if he had the time and information to make an independent decision. Cue-givers need not be individuals (p. 45).

The cue-taking model is deemed by the authors to be most relevant to the normal voting situation in Congress—that is, on low information, low salience votes. The authors talk of initial cue-givers—members with expertise on the issue in question—and intermediate cue-givers—members who have evaluated the cues of the initial sources and have themselves come to a position on the issue. The initial cue-givers tend to be on the committee where a bill originated, while the intermediate sources are more partisan in character, including party leaders, state party delegations, and sociolegislative groups such as the Democratic Study Group.

The authors then test the cue-taking notion with two distinct bodies of data—a set of structured interviews conducted with a random sample of members of Congress between February and June 1969 and a roll-call based simulation for nine sessions of Congress. The interview data are used to show that members of Congress explicitly recognize the need for and use of cue-taking process. The accuracy of the simulation results gives one greater confidence that cue transmission really does occur. The fact that the results of two separate analyses on two independent data sets converge makes one extremely confident that the pervasiveness of cue taking in Congress has been convincingly demonstrated by Matthews and Stimson.

The analysis of the interview data is straightforward. There are some minor criticisms to be made of the presentation of results: some of the tables could be labeled more clearly; some information about uncodable responses might be given, especially when such responses are numerous; and some of the simpler tables might be eliminated with their information presented more economically in a sentence or two of text. There is a tendency, especially in chapter 4, to overinterpret percentage differences.

More serious objections can be raised about the operationalization of the cue-taking process via the computer simulation and the evaluation of the accuracy of the simulation, although the following criticisms do not upset the validity of Matthews and Stimson's findings. Concerned with maintaining parsimony, the authors incorporate nine cue-givers in the simulation: the president, the state party delegation, party leadership, committee chairman, ranking minority member, conservative coalition, Democratic Study Group, party majority, and House majority. The choice of these nine cue-givers reflects the extant literature on legislative decision making and the authors' own intuitions about the process. Yet other theoretically relevant cue-givers might be included. In particular, a cue-giver that combines state party delegation and committee membership might better represent the decision-making processes of members from larger delegations with members placed across a large number of committees.

The authors claim that the computer program that comprises the simulation is "itself a description of the process of congressional decision-making" (p. 114). This claim seems a bit exaggerated. To predict how a representative will vote on a particular roll call, the simulation first determines how often the member has voted with each of the nine possible cue-givers on the previous 50 roll calls taking into account the number of times the cue was available and the number of times the legislator actually voted. A rank ordering of the importance of the cue-givers for each representative is then established with predictions about how a member will vote on a bill being generated by examining in order of importance how that member's cue-givers voted. It seems more reasonable to describe the simulation as prognostic rather than process, that is, as more of a data manipulation routine than a serious effort to model actual processes. There are some process aspects built into the simulation; the learning curve is intriguing and does allow for changes over time in cue-taking behavior.

The authors are not as sensitive as they might be to the problems involved in evaluating the predictive accuracy of the simulation. To be sure, Matthews and Stimson are keenly aware that their impressive 88 percent overall prediction rate is not to be compared to zero percent success or even to a chance model with 50 percent success. Yet the authors do not admit that there are other models to which their results might be compared and that these other models enjoy predictive success comparable to their own simulation. For example, more parsimonious party-only models can achieve predictive accuracy above 80 percent and perform even better when party is trichotomized as Southern Democrat, non-Southern Democrat, and Republican. On grounds of explanatory (as opposed to predictive) power, the Matthews and Stimson simulation is to be preferred over a party-only model. The authors recognize that predictive accuracy may be inflated by such factors as the virtual unanimity of many votes. Hence. they examine accuracy by the substance of the issue, the division of the vote, the type of vote, and the timing of the vote. They also analyze accuracy for the 100 representatives that were interviewed, thereby deftly combining the interview and roll call data.

In summary, Matthews and Stimson have documented very skillfully the importance of cue-taking in the U.S. House of Representatives. Their book is well worth reading both for its substantive findings and its intelligent use of multiple data sources. The problem now is to integrate the Matthews and Stimson work with other models of legislative decision making. The authors themselves might have done more to establish linkages between their enterprise and the work of, for example, Kingdon and Clausen. Yet this is certainly not their burden alone; an assessment of what we know about legislative voting and of how our various models mesh or conflict is now in order.

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Corporate Response to Urban Crisis. By Kenneth J. Neubeck. (Lexington, Mass.: Lexington Books, D.C. Heath, 1974. Pp. 166. Price not listed.)

Sociologist Neubeck explores four questions. First, why did major corporations become involved in solving the urban crisis? Second, how were these social program expenditures reconciled with the profit and growth objectives of business firms? Third, what benefits and costs are involved in this type of corporate activity? Fourth, what are the implications of this intervention for ghettoized blacks? The analysis is based on three case studies of corporations in St. Louis which Neubeck hopes are "similar to other corporate social projects undertaken elsewhere in the nation's central cities" (p. 132). The methodology is the case study approach which is based on a "flexible, exploratory research design of an inductive character" (p. 131). The author explains that the book "embraces a highly critical perspective . . . because this is what all sociology should be about and is about" (p. xi).

Neubeck conducts three case studies of corporations in St. Louis which attempted to alleviate urban problems from 1967 and 1970. In the first case, Southwestern Bell Telephone Company, a wholly owned operating company of AT&T, focused on working with the St. Louis school system to train individuals for low level positions. The second case study focuses on Brown Shoe Company which joined with a grass roots community group to establish a plant in the inner city. The third case study describes the efforts of the Ralston Purina Company to allocate charitable trust funds to enable a black inner-city grass roots group to carry out its own plans for housing development.

Each of the three corporate activities is described in terms of its limitations. One of these seems to be that each of the corporations' responses to the urban crisis was one of "enlightened self-interest" (p. 110). Specifically, Southwestern Bell served its own needs by training students for low level positions and thereby continued the "limitations on upward mobility for those with disadvantaged backgrounds" (p. 45). Brown Shoe Company employed workers at very low wages and "the profits from the factory were drawn *out* of the black community and not reinvested in it" (p. 85). The Ralston project had the greatest success because the company took a "hands off approach [which] placed resources and decisions in the hands of ghettoized blacks" (p. 112). Based on this, Neubeck points out that this is "*one of the less reprehensible approaches to helping* that might be undertaken by a corporation" (p. 112, emphasis added).

This last statement seems to reflect Neubeck's view that corporations are engaged in urban problem solving because of a "defensive managerial strategy which was designed to intervene in and manage a socially hostile environment." Corporate "foreign aid" tended to contribute to racial polarization and continued the colonial relationship between the residents of the ghetto and traditional power wielders.

In reading this volume one must raise a number of questions. First, to what degree did the author's critical perspective influence his conclusions? Second, are these three cases of corporate activity representative of the corporate response to the urban crisis? Third, is it not useful for institutions based on the profit motive to intervene in urban problem solving, i.e., do they do more harm than good? Fourth, what would have been a "better" way of corporate intervention? Fifth, did the corporate activity have a different impact on the individuals involved than on the community as a whole? Finally, what are "structural requirements of capitalist enterprise" which must be changed to make the corporate response more viable? These questions deserve further exploration.

WOLFGANG PINDUR

Old Dominion University

Financing the New Federalism: Revenue Sharing, Conditional Grants, and Taxation. Edited by Wallace E. Oates. (Baltimore: Johns Hopkins Press, 1975. Pp. xiv + 160. $12.00, cloth; $3.95, paper.)

This edited volume is the fifth in the Resources for the Future Governance of Metropolitan Regions series and contains five stimulating essays prepared for an October 1974 conference. The contributors concentrate on developing the major political and economic themes of the New Federalism. Each essay is well prepared, stimulating, and quite readable, but the collection is loosely tied together and the reader soon becomes aware of five individually prepared contributions and not a neatly organized group effort. This observation is meant to be descriptive and not negative because each of the contributions is extremely useful to the researcher interested in the theoretical and empirical aspects of New Federalism. Before returning to this point, a brief synopsis of the contributions is in order.

As Professor Oates states in his introductory comments, "It is . . . of great importance that we seek to understand what we have done, and are doing, in the course of modifying the structure of intergovernmental fiscal relations in the United States. This is the objective of the studies in this collection" (p. 6). Perhaps the most interesting approach to this question is the one utilized by Oates in his concluding essay dealing with the relationship between income elasticity of the tax structure and the growth of public spending. After developing a series of regression equations, Oates concludes that "for state and city governments, we found that over the decade 1960–1970 the income elasticity of the tax structure did indeed display a positive association with the growth in public expenditure" (p. 156). Oates, as he had hinted throughout this analysis, then goes on to point out the tentative nature of the findings because of time constraints with the data, endogenous variables, political considerations which could be influencing the analysis, and the fact that he did not cover the normative questions involved. Thus Oates is well aware of the factors limiting his analysis and is sensitive to the need to be cautious about them.

This is also the case with the contributions of Robert P. Inman and Martin McGuire. Inman examines the question of the "likely effects of matching, base-equalizing, or lump sum aid on local expenditures and taxes" (p. 89). Inman then develops policy models for the various types of federal aid and applies the equilibrium grants-in-aid policy model to the reform of school financing and concludes the chapter with a less than optimistic evaluation of the utility of the policy model approach. McGuire's chapter deals with the development of ". . . empirically testable models for describing the impact of the grants-in-aid system in price and income terms" (p. 116). Developing a series of econometric models, McGuire concludes by discussing the preliminary, but potentially useful nature of his research. As Oates and Inman do, McGuire points out the model's inability to accommodate a variety of empirical realities—most of these are directly tied to the political arena and public choice questions.

The chapters by Robert D. Reischauer and Jeffrey L. Pressman are much more sensitive to the political realities dominating intergovernmental fiscal relationships. Reischauer, borrowing heavily on his knowledge of the Brookings study of general revenue sharing, concludes that "whether state and local governments' behavior will be significantly affected by the revenue sharing program will become apparent only after several years" (p. 87). Using a variety of analytical techniques, Reischauer speculates about the probable impact of general revenue sharing on a variety of fiscal phenomena. Pressman, after analyzing a variety of the political implications involved with New Federalism programs, concludes that "... the struggle between proponents of centralization and decentralization, national power and local power, will continue to be a central one in the public arena" (p. 39). Pressman's analysis includes a wide array of the programs associated with New Federalism and his evaluation of possible political effects and impacts is quite well done. Thus we are back to the point raised at the beginning of the review.

These essays are provocative, but speculative. Each requires empirical results not yet available when they were prepared. In addition, the models and equations often ignore major political factors (such as local budgetary processes and their politics, the political power of the "haves" and "have-nots," etc.) with the result that their general applicability and explanatory value may be quite limited. Despite these reservations, the contributors carefully develop their arguments and a consideration of their underlying assumptions is most revealing. Certainly the policy maker interested in the impact of New Federalism or a specific program will not find answers here, however the researcher interested in a source of important hypotheses to be investigated will find the veritable "pot-of-gold." The problem is to develop the data needed to investigate the hypotheses—a task which is always difficult. As a researcher long involved in evaluation research dealing with the New Federalism, one can only hope that the ideas and theoretical points raised in this collection have been thoroughly investigated by these authors or others in the past few years. In fact, Oates' basic assumption about the modification of the structure of intergovernmental fiscal relationships in the United States might even be subject to serious questioning and at the very least ought to be empirically verified. If the volume stimulates this type of research then it will be a major success. The contributions to it already are.

DAVID A. CAPUTO

Purdue University

Voter's Choice: Varieties of American Electoral Behavior. By Gerald Pomper. (New York: Dodd, Mead, & Co., 1975. Pp. ix + 249. $10.00.)

A search for the responsive voter—the individual whose electoral decision reflects policy content—describes the main theme around which Gerald Pomper has built his secondary analysis of the "voter's choice." This book extends many of the basic concepts and findings reported earlier in "From Confusion to Clarity" (*American Political Science Review*, 66 [June 1972], 415–28), which, as Chapter 8, forms the heart of this volume. The work also displays the impact that V. O. Key, through his ideas about the responsible voter, has had on contemporary thinking about electoral behavior. Indeed, a secondary theme of the book is the role of parties and candidates in presenting vital issues to voters for their judgment.

The "responsive" voter is the individual citizen who can and does utilize, as a basis for decision making, policy differences that exist between the candidates. Pomper employs the now familiar V. O. Key argument that if candidates were to address policy questions during the campaign, the voters would be more likely to apply issue-laden criteria in their electoral selection. The study provides no direct evidence on how candidates and the parties have presented issues to the public over time; it, rather, utilizes national survey data from the University of Michigan Survey Research Center and Center for Political Studies national election surveys between 1952 and 1972 to determine if voters were more responsive to issues and party differences in some elections than in others. The analysis therefore sheds indirect light upon those political circumstances that either augment or attenuate the impact of issues on the electoral choice.

The antithesis of the responsive citizen, the "dependent" voter, is described as the individual who selects a particular political candidate because of more enduring determinants of the vote, such as social characteristics or traditional party loyalty. Presumably, dependent voters pay little attention to political events or issues of public policy, and instead base their vote decision on cues from their social group, economic class, race, or preferred party. According to Pomper, what has emerged from the electoral studies of the fifties is a portrait of the voter as dependent, while the contemporary voter is more accurately described as responsive.

The first half of the book is aimed at rejecting the portrait of the dependent voter. The importance of party identification, social class, sex, age, race, and education as determinants of the vote decision in recent elections is questioned. Ignoring congressional voting, Pomper argues that party loyalty and traditional demographic distinctions no longer explain the electoral choice. Party identification has not only declined with the recent growth in the proportion of Independents, but it is also less strongly related to vote choice than in earlier years. The newly emerging Independents, unlike those of an earlier historical period, are found to be no less factually informed about electoral politics than partisan identifiers. Yet, in a fashion incongruous with the portrait of the responsive voter, Independents still participate in electoral politics at a rate well below that of partisans.

A noteworthy omission from the analysis aimed at determining if the vote choice is dependent on social background characteristics is concern with union membership, religion, ethnicity, or the urban-to-rural composition of the individual's immediate environment. The criteria used in selecting demographic variables for the analysis is not clarified, and one can only assume that age and sex are included because they represent recent interest in the youth and women's movements. It is interesting to learn that women now participate in voting at a rate equivalent to that found for men—except in the South where regional culture still suppresses female participation—and that young people are more likely than older people to be ideological liberals, regardless of their party identification. But the important point for this book is that these demographic characteristics are not related substantially to the vote choice. However, age and sex have rarely been seen as major explanations of the vote and, while interesting, this part of the analysis is somewhat of a departure from the main theme.

The most relevant discussion of the dependent voter model comes in an analysis of social class, a demographic factor traditionally assumed to affect the vote. To authenticate the dependent voter portrait, the empirical evidence would have to reveal a direct and permanent influence of social class on the vote choice. Indications of enduring and consistent class differences in the vote are, however, not evident in *Voter's Choice*. Pomper finds that the vote has been episodically related to class with the correlation strongest for those elections in which questions of social class were salient, thereby facilitating class voting. He thus concludes that vote choice is not so much a reflection of fixed background characteristics as it is a given reaction to the circumstances, candidates, and issues of the times.

Having rejected the theory that voters simply cast a ballot on the basis of party tradition or as a conditioned reflex of social differences, the second half of the book examines the portrait of the responsive voter—that citizen whose political actions are explicable but not permanently predictable. The search for evidence of the responsive voter begins with an assessment of material that should be quite familiar to students of electoral behavior—party and candidate images.

The responsive voter can effectively express policy preferences only if some candidate or party will represent these concerns and promise to translate them into policy decisions if elected. To be "responsive," therefore, the voter must be aware of policy differences between the parties, must discern the direction of party differences, and must respond on the basis of these perceptions when making electoral choices.

The empirical evidence shows a consistent pattern of growth in the perception of policy-related party differences during the period from 1956 to 1972. This change occurred almost equally across various age cohorts and at all levels of education, thereby suggesting political events as a likely cause. This increased awareness of partisan differences resulted in the clear perception of the Democratic party as the liberal party—a perception relatively independent of the voter's own partisanship or personal ideology. Yet, despite the increased awareness of partisan differences, Pomper argues that the effect of party on the vote choice has eroded and been replaced in importance by the voter's own issue preferences and those of the candidates.

Numerous other writers have examined the changing role of issues and candidate personalities as explanations of the electoral decision, but *Voter's Choice* provides, through its use of causal modeling, a more concise method of illustrating the changes that have occurred. While the issue measures used in the causal analysis are not identical for the three time points compared (1956, 1968, 1972), the results clearly support the argument that issue and candidate contributions to the vote choice increased while partisanship decreased. The portrait is thus complete, the contemporary citizen in reaching a vote choice is more responsive to current issues and candidates than dependent on long standing party allegiance or socioeconomic characteristics.

In his conclusion Pomper calls for strong and responsible parties that take clearly distinct positions on the issues. Responsible parties would presumably provide policy content to the meaning of party loyalty and again strengthen the relationship between party identification and the vote—ironically a trait of the dependent voter.

Despite many admonitions about hos the nature of the times and historical events may affect the reality of any portrait of the American voter, much of the discussion seems time specific. It is clear that shifts in political behavior have occurred during the past decade, and this book capably spells out some of these changes. Yet the fact that there was not more change in the face of such dramatic events as the civil rights movement, urban riots, the Vietnam war, and Watergate attests to the substantial inertia in certain attitudes and behavior patterns. Only the passage of time will allow us to test the endurance of the changes brought by the events of the past decade. *Voter's Choice,* while perhaps too complicated for the lay person or undergraduate because of the sophisticated analysis and intricate writing style, should be essential reading for political scientists who will want to compare the electoral impact of future political events with those of the past.

ARTHUR H. MILLER

The Liberalization of American Protestantism: A Case Study in Complex Organizations. By Henry J. Pratt. (Detroit: Wayne State University Press, 1972. Pp. 303. $15.95.)

This sophisticated analysis of the National Council of Churches focuses on the forces that helped produce changes in policies and actions from its inception as the Federal Council in 1908 to 1970. Professor Pratt teaches political science at Wayne State. He received his doctorate in 1962 from Columbia University. This is pertinent for he served for a short time as research assistant to Reinhold Niebuhr while he was at Columbia. Therefore, this is not Pratt's first work in the politics and institutionalism of religion; also he served under one of religion's most sophisticated mentors.

Initially, his analysis is historical, a procedure which enables him to isolate three primary stages of development. From 1908 to 1933, the primary concerns in the council are social issues and problems, as one might expect during the climactic closing decades of the Social Gospel. At this time, "member churches were prepared to go along with the council's . . . outspoken stance on controversial issues of the day, though not all of them were equally supportive and some actually balked" (p. 262).

From 1933 to 1963 there prevailed, according to Pratt, a general state of confusion in the council, with no clear policy guidelines, an uncertain organizational definition, and a floundering institutional identity. The New Deal took away from the council many of the "radical" social declarations, and the rise of aggressive power in Germany diluted the message of the pacifists in the churches and the council. This complex period of trembling, if not fear, was capped by the hysteria of McCarthy and Velde and the attack from the right-wing churches whose people honestly supposed that the National Council was riddled with communists.

From 1963 to the recent past, the secular situation was again more favorable for the council; again they were needed and useful at the local and national levels. This was most notable in the arena of civil rights, and many staff specialists were added in this and other areas.

Pratt does not confine his analysis to the good and bad fortunes of the council as it responds or reacts to external secular and religious forces. Internal study indicates how the organized bureaucracy changes in response to needs. Speaking generally, the staff of the council was recruited almost exclusively from the ranks of the pastoral clergy. Beginning in the 1950s, however, experts were appointed in such areas as civil rights, migrant labor, poverty, housing, voter education, black economic development, and community organization.

This is a useful book from the historical standpoint alone, covering many issues that cannot even be mentioned here, among them for example, analysis of the minority of liberal churches and their impressive power which belies their numbers, liberalism versus conservatism as reflected at the level of national organization, local councils of churches, the council and the black power movement, and the continuing search for Protestant identity. But Pratt seeks to achieve more than this by trying to lay a foundation for a "theory of interest group activism."

Having analyzed the National Council, the author does a sociohistorical analysis of the action frames of the AF of L, the NAACP, and the ACLU. He finds a striking similarity in institutional development and behavior in all four organizations. In each of the institutions, he finds a similar evolutionary pattern: an "early stage of social concern" which lasts about 20 to 30 years; this is followed by a second "stage of crisis and doubt about strategy"; and the change in strategy comes "about not in response to the reasoned objections of the critics, but to the 'logic of events' and a dramatic adverse alteration of external and internal conditions" (p. 268). Pratt is referring here primarily to power and ideological battles both within the organizations and from rival organizations. The third stage is characterized as one of "reconsolidation and reform group alliances."

Concluding his study in 1970, Pratt was benignly optimistic about the active social concern, the "liberalism," of the National Council. "One would not anticipate any drastic changes in predisposition as long as NCC liberalism is supported by its more actively involved member bodies. True, there are numerous conservative churchmen in these same denominations; but their opposition has not succeeded in reordering priorities. Their impotency is partially attributable to the skill of council leaders in countering criticism and isolating opponents . . ." (p. 266). True, "drastic changes," whatever they might be, may not occur, but profound fluctuations in policy and procedure must be anticipated. The council is now experiencing one of those fluctuations, again from both internal and external forces. For example, between 1968 and 1975, the number of women and the number of minorities has sharply risen, from 16 to 30 percent and 8 to 25 percent, respectively. But this has not radicalized the council. On the contrary, there has been a serious drop in funding from the member denominations, due primarily to internal troubles among these groups and a drift from social concern. Within the council itself, as noted by Pratt in the later years of his study, there is an increasing awareness of the complexity of contemporary social problems. Simple declarations of religious conscience are recognized as ineffective and unnecessarily inflammatory. In any case, the council is once again in a period of reassessment and retrenchment, seeking for "deeper biblical and theological rootage." But the council continues to be maintained and staffed by predominantly liberal social forces, so the uncertain trumpet may be heard again when the time is fulfilled.

PAUL HARRISON

Pennsylvania State University

The New Federalism. By Michael D. Reagan. (New York: Oxford University Press, 1972. Pp. 175. $1.95.)

Reviewing a book on intergovernmental relations five years after it is published can be an exercise in futility. After all, several events since 1972 have had significant effects on the federal system: the total amount of federal aid to state and local governments increased by over 75 percent between FY 1972 and FY 1977; general revenue sharing was implemented; Congress approved block grants for manpower training, community development, and social services, further enlarging the noncategorical sector of the federal assistance system; state and federal courts tackled the issue of equity in the financing of public schools and the delivery of public services; and Watergate, the recession, and the presidential election campaign generated anti-Washington sentiment and raised doubts about the federal government's capacity to solve domestic problems. These recent developments, among others, underscore the fact that intergovernmental relations are in a constant state of flux. The dynamic nature of the subject matter can quickly transform studies of the present contours and future directions of the federal system into works of history. Fortunately, Professor Reagan's book is an exception.

The New Federalism has for the most part met the test of time. Much of the book is as relevant now as when it was published. The author's perspective is pragmatic, political, and philosophical. At the outset he notes that "old style" federalism is dead. The "new federalism" is intergovernmental relations, which have blurred the constitutional division of authority and functions between the federal government and the states. "Sharing," "independence," and "partnership" characterize the new federalism. While these themes are not new and have been developed more extensively in the writings of Morton Grodzins and Daniel Elazar, Professor Reagan takes a different position on their significance—the federal government is, and should be, the senior partner. Its authority derives from the constitution and judicial interpretations of key clauses; its power from domination of the income tax field and the growing dependency of states and localities on federal funds; and its influence from the skilled and sympathetic professionals who run the executive and legislative branches.

Federal grants-in-aid are considered the "cutting edge" of intergovernmental relations. The chapter on this subject is one of the most insightful treatments of the politics of the grant system in print. It contains a thoughtful analysis of the types and purposes of grants and a skillful probing of the rhetoric and realities of such issues as national decentralization, states' rights, federal domination, and "no strings" grants. However, relatively little attention is given to the problems resulting from the categorical grant explosion during the 1960s and the administrative responses to them.

The chapter on "Revenue Sharing—Panacea or Cop-out?" challenges much of what has become conventional wisdom about the merits of this instrument. Readers will find particularly interesting the author's observation that instead of restoring vitality to state and local governments it would "in the long run, put another nail in the coffin of federalism" (p. 104). The arguments supporting and opposing general revenue sharing are well developed and, in view of the issues raised during the Congress' debate over renewal of the State and Local Fiscal Assistance Act, are still quite pertinent.

A disappointing feature of the book is the author's acceptance without serious questioning of several of the traditional indictments of state governments. While it is noted that wide variations exist in state performance across the country, for the most part states as a whole are viewed with distrust if not contempt: "State governments are structurally inadequate and politically weak even when not actually corrupt" (p. 111). This position is certainly not new to political scientists. What is surprising is that the discussion of the "proven incapacities" of the states is based more on political philosophy than on empirical evidence. Since during the last decade several states made significant strides to strengthen the governor's powers, reorganize the executive branch, revamp the legislature, restructure the fiscal system, and expand services, to ignore these developments detracts from an otherwise realistic picture of the workings of the intergovernmental system.

Although in a general sense the title of this book refers to intergovernmental relations, the author also considers the term "new federalism" in the specific context of the Nixon administration's domestic policy. Not surprisingly, this is viewed as being "simply romantic rhetoric, a facade behind which the national government is to abrogate its domestic role, to reduce its presence merely to that of an onlooker . . ." (p. 163). The author suggests alternatives to general revenue sharing, including tax credits and expanded federal financing of public assistance and education.

The final chapter charts some future directions for the federal system. The general thrust of these prescriptions—labeled (regrettably) "permissive federalism" would be to make explicit and further enhance the federal government's dominant role in intergovernmental relations. The basic fact of life underscored by this phrase is that "there is a sharing of power and authority between the national and state governments, but that the state's share rests upon the permission and permissiveness of the national government" (p. 163).

Professor Reagan's approach is provocative, his argumentation is persuasive. Regardless of whether one accepts the author's point of view and the supporting evidence, *The New Federalism* highlights several areas where additional research and policy analysis could help dispell some of the myths surrounding intergovernmental relations.

CARL W. STENBERG

Advisory Commission on Intergovernmental Relations

Policy-Making in the Federal Executive Branch. By Randall B. Ripley and Grace A. Franklin. (New York: Free Press, 1975. Pp. 209. $11.95.)

This volume is part of a larger project on policy making in progress at the Mershon Center of the Ohio State University. Ripley and Franklin describe the book as an interim report on the status of that research project. They suggest that the eight chapters in the volume, a number of which were written by project associates, can stand on their own but that the common framework, language, and methodology justify a single book. Whether this is true or not is less important than that the work is an heuristic undertaking designed to aid in theory building.

The framework of the study is a synthesis of three approaches to the study of public policy making that have played a significant role in political science scholarship in recent years. The three include: the comparative study of American states characterized by the work of Dawson and Robinson, Hofferbert, Dye, Sharkansky, and others; analytical efforts growing out of the typology developed by Lowi and refined later by Salisbury; and the study of decision making using a systems approach.

In organizing their work, Ripley and Franklin and their associates have divided the policy-making process into four parts: the external environment, the internal environment, policy actions, and policy results. These terms are roughly comparable to the concept "inputs, conversion process, outputs, and outcomes" used by systems theorists. The authors have operationalized these phenomena by employing variables that are relatively easily quantified, such as party strength, economic and social conditions, and public opinion as external environmental factors and agency structure, agency personnel characteristics as defined by rank and status, and similar measures as internal environmental factors. Unfortunately many other environmental variables that might be equally useful for analytical purposes, but for which quantitative data are not available, are excluded. Although this exclusion is a source of some disappointment, it is difficult to quarrel with the authors in their effort to obtain measures suitable for statistical testing. In dealing with policy actions and results, Ripley, Franklin, et al. have used quantitative measures such as appropriations and expenditures. Again, they were aware of the limits of such variables as descriptions of outputs and outcomes. As a result, Chapter 5, prepared by Steven Shull, is devoted to examining what the authors refer to as functional actions using nonexpenditure measures. The results were inconclusive, holding out only limited promise for future research.

The most valuable contribution of this work is that it provides further empirical testing of the incremental theory of policy making. As stated by Ripley in his introduction:

> The primary hunch underlying the research effort to be reported in the subsequent chapters is that policy actions, especially when measured by dollars, are not determined simply by previous policy actions. External and internal environmental factors are believed to have an important role in determining the level and kind of change in agency policy actions (p. 17).

In short the authors believe that the incremental explanation is too simplistic a way of looking at the policy-making process.

The authors have identified nine major hypotheses regarding the policy-making system. These relate to matters of agency structure, partisan division, interest group and opinion coalitions, and economic and social conditions, both general and specific to agency decision making. The study emphasizes appropriations and expenditures as measures of policy actions. Five separate studies were made using as few as eight agency sites and as many as 40. The studies were longitudinal, ranging in time from 1960 to 1971 in two instances, from 1952 to 1971 in two others, and from 1946 to 1971 in another. The authors sacrificed the benefits of in-depth analysis for the ability to do systematic and sophisticated statistical analysis. Statistical techniques employed in testing included historical trend analysis, block variable path analysis, and subsample tests for interactive effects. Bivariate as well as multivariate analyses were used throughout the work.

The findings of the individual studies, although frequently inconclusive, are intriguing and support Ripley's hunch on incrementalism. Chapter 2 reports William Holmes' finding that change in social conditions of a general nature and change in conditions on issues related specifically to agencies studied affect both policy statements by agencies and their policy actions.

William Moreland's chapter explores the relationship between dimensions of agency maturity and budgetary policy actions in order to construct an alternative to the incrementalist explanation of budget outcomes. His findings demonstrate that, in addition to previous allocations, agency managerial capacity, agency size, and administrative experience of agency executives are all positively linked to outcomes.

The chapter that follows, authored by Lance Le Loup, identifies systematic sources of change in agency appropriations. It criticizes the incrementalist perspective on budgeting for emphasizing stability of appropriations and for defining change as random because this precludes examination of regular determinance of change. The findings show that percentage change in appropriations is determined primarily by percentage change at the agency request stage of the process rather than in the final appropriation outcome—thus indicating the relatively greater importance of the executive branch in the budget process than of Congress.

In the last two substantive chapters, Ripley and his associates link a number of independent variables such as agency maturity, partisan strength, economic conditions, and previous budgetary policy actions. Their effort results in what they call a dynamic change model in which previous budget policy actions represent only one element.

Although the authors are modest in their claims, this volume has made a significant contribution to the critique of the incrementalist model. They acknowledge that incrementalism should not be eliminated in creating a more workable model:

> We are not disputing the claims that the rationality of persons dealing with complex phoenomena is limited, and they therefore deal with "successive limited comparisons" in thinking about and acting on reality. Nor, despite Gist's caveat, are we disputing the claims that budget-makers in particular think in terms of a base situation and the increments of change in it. Our data do not permit us to comment empirically on these claims and, at the level of hunch, we think they are probably in good measure true. They are certainly sensible claims, which explains in part the pervasiveness of those parts of the incrementalist perspective (p. 175).

The authors have rendered further damage to the utility of what many scholars regard to be an oversimplified model of decision making. More important, they have taken the first steps in developing their own conceptual scheme for future theory building. It remains to be seen whether the new model resulting from this enterprise will provide us with a workable theory of policy making.

ROBERT S. FRIEDMAN

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The Dying of the Light: A Searching Look at America Today. By Arnold A. Rogow. (New York: G.P. Putnam's Sons, 1975. Pp. 384. $10.00.)

With some felicity, much pathos, and an irretrievable sense of loss, Arnold Rogow seeks to mobilize our errant consciences against the decline of American civilization. We are, collectively, poor candidates for this mission, for our superegos—defined as "the conscience and sensibility of the Western mind" (p. 14)—have, according to Rogow, declined almost to the point of no return. We Americans have lost the supreme value we once placed on human life and dignity, faith in reason, capacity for self-denial, respect for others. But in the twilight before the dark, with Dylan Thomas, Rogow bids us "rage, rage against the dying of the light." And he shows us how.

There is a theological quality to the work which makes the opening chapter on the Puritans, whom Rogow greatly resembles, peculiarly appropriate. They, like their successors, Fromm, Roszak, Slater, Mumford, Arendt, Jules Henry, and now Rogow, had a special knowledge of an internal state of grace which mankind imperiled with his sinful ways. Today, it is the refusal to accept a "domestic revolution of falling expectations" (p. 14) that is the source of the trouble. And the central problems, in addition to the decline of the superego mentioned above, are: "continuing loss of affect (or the capacity to feel and to respond to others), a further expansion of permissive sexual culture [about which Rogow is most ambivalent]. . . , decline of the family, diminishing ethnic and cultural diversity (and the eventual dominance in the world of a universal or 'pop' culture of which the leading characteristics are American), increasing conflict between hedonistic consumption and resource scarcity, and a movement away from traditional and personal freedoms" (p. 13).

While these are trouble enough, they represent only those occurring to the author as he wrote his preface. As he writes the book, the focus turns to the graceless state of the economy, with its continuing inequalities and injustices, the desecration of the city, rising crime and violence, the cruelties of racial oppression and the degradation of the blacks by a repressive society, corruption of the media, and the lonely, bored life of the suburban housewife, the fear of communism (because it abandons the father principle), the corruption and unresponsiveness of government, and, of course, the trashy sex life of the times. The indictments are partially true; Rogow has the evidence which he lays before us without mercy—or much interpretation.

Why do we not do something about this? We do not move our indifferent selves to rekindle "the light that once blazed so brightly" in America because of the decline of conscience and sensibility, but more importantly because it is in the nature of reformers to be *ambivalent* about their own reforms. We feel guilty about our own violation of the conventional corrupting norms; thus, we are only half-hearted about reform, while the "forces of the Right" act from conviction (pp. 308–09).

Perhaps it was the purpose of this eloquent narrative account of pain and worthlessness to restore to us the conviction eroded by the corrosions of ambivalence. For this purpose, rage is useful, moral appeal relevant. But for the purposes of understanding, not as moralists, but as social scientists, or as social humanists, the work leaves us to create our own comprehension out of the materials of indignation. We have seen enough neo-Marxists straining to pour the materials of history in a leaky vessel not to ask for more, but there *is* an economic structure that affects the culture of which Rogow now almost despairs. The demand for Freudian causal interpretations of our decline, after Marcuse, Rieff, and Norman O. Brown, may be temporarily satiated; but even here, where Rogow has considerable expertise, the systematic working out of a theory of instinct and its vicissitudes is missing. Indeed, there is no system at all, not even the kind of partial system which Daniel Bell, or Jacques Ellul, or Hannah Arendt offers. The work is not grounded in anything but observation, much of it through careful clipping of the *New York Times.*

Modernity is a Rorschach. As Rogow says, "The supreme difficulty facing any interpreter of life in America is that almost any statement he wishes to make about the United States he can show to be at least half true" (p. 11). We know why the Right is so passionate in its views: it defends its privileges, expresses its authoritarian personality nurtured in patriarchies, "experiences anxiety in the face of uncertainty" (G. D. Wilson, *The Psychology of Conservatism,* 1973, p. 259), responds defensively to ambiguities, and so forth. But Rogow's impulse to rage against the dying of the light, under this condition of half truth, needs explication. Herewith a modest list: (1) It represents a reaction formation against the death wish, or perhaps a projection: "Society is dying, I am not." (2) It represents the revenge of the frustrated reformer: "I cannot change things; therefore it must be that people are irremediable." (3) It represents a search for grace or absolution or avoidance of complicity: "I warned them to mend their ways." (4) It represents the struggle against Rogovian ambivalence, a reaffirmation in the face of doubt. (5) "This high I.Q. whimpering on a cosmic scale occurs whenever an external source of values fails to work" (A. Maslow, *Toward a Psychology of Being,* 1968, p. 16). When a child of the New Deal finds his faith in reformist liberalism shattered, he rages against the dying of the light. (6) Without moral passion, the tepid liberal loses his will. And (7) because the light *is* dying.

ROBERT E. LANE

Yale University

Achieving Effective Desegregation. By Al Smith, Anthony Downs, and M. Leanne Lachman. (Lexington, Mass.: Lexington Books, D. C. Heath, 1973. Pp. 128. $8.50.)

Achieving Effective Desegregation is a practical guide for the school administrator who faces the task of desegregating a school district, wishes to avoid disruptions in the life of the school and community, and aims to achieve a quality education for all students. The authors' primary thesis is that committed leadership on the part of the superintendent and school board, who emphasize the "inevitability" of desegregation, take the initiative in developing their own desegregation plan, and move steadily toward developing an effective plan, will decrease the probability of disruption. They then proceed to present a detailed outline of the process of achieving effective desegregation starting with the moment the decision is made to desegregate the schools through the opening day and the entire first year of desegregation. The process is divided into three phases: planning, initial implementation, and positive development.

The authors use information from the literature on desegregation, from a field survey of ten districts, from essays by experts in the field, and from interviews with knowledgeable persons. An immense amount of useful information has been distilled into a carefully cross-indexed manual. Techniques cover a full range of concerns. Administrative measures include a discussion of improving representation in decision making, suggestions for promoting community support, a program to reduce staff uncertainty, and an approach to revising the role of the principal. The authors cover new classroom management techniques, new curriculum materials, approaches to improving teacher sensitivity to diversity, and they offer ideas on new supervisory arrangements to improve teacher performance. A section on community relations contains useful proposals for reducing community uncertainty about desegregation issues through a "rumor center," through an "open door" policy in the school, and through developing a School Board Communication Policy. They present advice on methods for gaining community participation in implementing the desegregation policy and obtaining the cooperation of other community agencies.

The authors emphasize the importance of including students in the planning, and they counsel the reader on the importance of forming democratic school structures which includes students of all ethnic groups. They suggest ways in which the curriculum and the guidance services can be used to improve student behavior, adjustment, and achievement while making a strong case for the maintenance of discipline within the school. The section on techniques for achieving effective desegregation also concludes with recommendations for diversifying the contents of the curriculum and changing classroom organization and scheduling to promote small-group work.

The reader needs to observe two cautions in reading the book. While many of the proposals made by the authors have been tried successfully in desegregating districts, other proposals are untried suggestions made by the authors on the basis of their observations and experience. The reader needs to be alert to separate the two types of counsel. The very timely nature of the material in the book creates the problem of obsolescence. For example, lists of consultants and resource agencies already need updating, and the suggestions for reading and for curricular materials will become less useful as time passes since they will not be current with the rapidly changing educational scene.

The administrator faced with the task of desegregating a school district will find *Achieving Effective Desegregation* a helpful guide in anticipating problems and preparing for critical junctures after assessing alternative courses of action. It is the best treatise currently available for persons seeking practical information on a wide variety of educational problems faced by educators who are seeking to achieve the goal of effective desegregation.

JANE R. MERCER

University of California, Riverside

The Sources of American Student Activism. By James L. Wood. (Lexington, Mass.: Lexington Books, D. C. Heath, 1974. Pp. xviii + 187. $15.00.)

As the American student movement fades into the background, further assessments about its nature and impact continue to appear. Why did it happen, who comprised its rank and file, what was the place of ideology, how did it reflect lineage and generational tensions, and why did it wane, are among the leading questions. Scholars have tended to fall into two camps when addressing the student movement. Some view it primarily from the standpoint of a social movement, with emphasis on ideology, organization, and social dynamics. Others are more inclined to take a socialization perspective, with emphasis on the precursors of involvement, motivational structures, and personal consequences.

In this transformed Ph.D. thesis Wood emphasizes the socialization perspective but he is not unmindful of the social movement aspect. Appropriately enough, the research setting is the birthplace of the student movement, the University of California at Berkeley. The data base consists of interviews in 1968 with a random sample of undergraduate and graduate students (N = 482, response rate = 89 percent) gathered by Robert H. Sommers and one set of parental questionnaire data gathered by Sommers and another gathered simultaneously by Jeanne Block (combined response rate about 60 percent).

The most successful parts of the book are theoretical in nature. After briefly surveying the nature of the student movement and the various interpretations of it, Wood sets out to develop a middle range theory. This theory combines the radical socialization approach with the family conflict approach. Rather than seeing these as mutually contradictory, as is the usual stance, he argues that they are complementary. The essence of his position is that

> many activists act out radical political values that are derived from their parents' radical political or culturally unconventional values; yet the activists come into conflict with their parents because the activists feel the parents have not lived up to their own political or cultural values (p. xvi).

Although the accompanying analysis lends at best moderate support for the position, it is at least as defensible (and in my view more so) than the monistic theories typically advanced. Wood sticks conscientiously to exploring various explanations throughout the book; this steadfastness is perhaps its greatest strength.

As with many dissertations turned first book this one pays excessive tribute to authority figures and belabors methodological niceties already within the grasp of most readers. On the last point, for example, much attention is paid to the "elaboration schema" designed to test and explicate two-variable relationships as though it were something new to the social sciences. This tediousness reaches its zenith with the adaptation and inclusion of two tables from a secondary source to make a point about contingency relationships (pp. 88–89).

Given the author's methodological self-consciousness it is all the more surprising to note questionable decisions and interpretations. Case numbers are often perilously low and might sometimes account for seemingly anomalous findings. Using parental reports to characterize power and affective relationships between parent and growing child is a weak technique at best. To use student self-reports on their relations with parents as the criterion for determining whether activism had increased or decreased lineage harmony is equally suspect, as is the assertion (based on synchronic data) that student radicalism leads to parental radicalism.

An example of a different sort of temporal problem occurs when the author tries to analyze the social characteristics of activists over time. Working only with the 1968 student survey, Wood attempts to show the changing composition of the 1964 Free Speech Movement, the 1966 Student Strike on campus, and the 1967 Oakland Induction Center demonstration. Whether the various "findings" actually correspond to reality is beside the point, because the data base will simply not allow one to address the question. The turnover in the student population was enormous across these four years. Illustratively, of the 492 respondents in 1968, 29 percent were on campus in 1964–65, 59 percent in 1966, and 91 percent in 1967. Trend interpretations could be rendered only if the relevant time-specific populations had been sampled.

The error is compounded by statements about the absolute amount of activism over the four years. The conclusion is reached that activism decreased among virtually all social groupings. But that clearly cannot be deduced from these data. Is it any wonder that the author is drawn to the contradiction between his Berkeley findings versus those from replicated cross-national surveys. Even if he were right about the broadening social base and overall decline at Berkeley, he would be right for the wrong reasons.

The book is also curiously deficient in one area where it should be especially strong. Since the early 1960s there has been a veritable flood of literature regarding political socialization. Because much of what the author discusses draws upon pre-adult experiences and sources of political orientations one would have expected at least some utilization of the theoretical and substantive outpouring in the area. Yet that is not the case, and the book is the weaker for it.

On the other hand, and much to his credit, Wood does not take the extant literature on student activism as the received wisdom. He very correctly points to several deficiencies of some of the landmark writings and attempts, within the limits of the data, to overcome these deficiencies. If for no other reason, such efforts would stamp the book as a useful contribution to the literature on the student movement. Whether one wants to pay the extravagant price which the publisher has attached to this slender volume is another question.

M. KENT JENNINGS

University of Michigan

Governing American Schools: Political Interactions in Local School Districts. By L. Harmon Zeigler and M. Kent Jennings with G. Wayne Peak. (North Scituate, Mass.: Duxbury Press, 1974. Pp. 269. $12.95.)

In some respects this book by L. Harmon Zeigler and M. Kent Jennings with the assistance of G. Wayne Peak is neither important nor interesting. The format and theoretical underpinnings are taken from elsewhere—a point freely suggested by the authors—while the methodology has been used frequently before. Indeed, the authors even resorted to using a sample designed for different purposes and used before by one of them in a previous study. Once the apparatus and techniques for large-scale national surveys has been perfected, as certainly it has at Michigan's Survey Research Center, the cynic might dismiss a study such as this as just another "easy" method of expanding one's list of publications. In addition, books reporting the findings of such surveys are tedious reading at best. Rare indeed is the author—one is reminded here of the late V. O. Key, Jr.—who can bring such data to life.

These limitations notwithstanding, there are other respects in which this book is groundbreaking. The study of the politics of schools is not new by any means, but most attention is recent and could benefit from the data base now generated by Zeigler and Jennings. It may not be glamorous, but Theodore Lowi's contention that taxonomy should precede ontogeny or phylogeny—a notion long established in the so-called hard sciences—makes eminent good sense in the study of governing our nation's schools. After all, there are about 16,000 units of government, with approximately 90,000 elected officials, controlling the expenditure of some 50 billion dollars, or more than 20 percent of state/local expenses.

The reluctance of political scientists to study school politics—except as a part of larger social issues—stems from many factors. Zeigler and Jennings suggest that it results from political scientists having been "so persuaded by reformers' arguments to keep schools separate from politics that they have virtually ignored educational government as an appropriate object of study" (p. 3). Perhaps most important in the present study's attempt to open this vast area to further investigation is their framing of the dichotomy between the reformers' dream of nonpolitical schools and democratic theory. If one accepts democratic notions of elected representatives translating majority preferences into public policies then schools don't come off very well. Indeed, a recent Gallup Poll (1975) commissioned by the National School Boards Association found 63 percent of those polled unable to name a single thing their school board had done in the past year. With such little interest, Zeigler and Jennings' findings that incumbents are virtually assured of reelection if they so desire, that boards tend to name their successors, and that boards bear only slight resemblance in social characteristics to the population they represent, should come as no surprise. Moreover, few members are interested in furthering their political careers, thus removing the constraints-of-ambition theory.

But schools are different from most other public bodies in that they are also service organizations. Thus one can take the view, which reformers certainly did, that with politics out of the school business schools could be run by those who know what education is all about: that is, by the experts, the educational managers, or more specifically, the superintendent. *Governing American Schools* documents the shift in direction of policy making that has occurred under this operative assumption. Superintendents make policy and boards serve as legitimizing agencies, justifying and selling these policies to the community.

Zeigler and Jennings found the most competition for school posts and greater citizen input in those districts that were unreformed—that is, where board members were elected from districts or wards or where the choice remained partisan. Recent attempts in Los Angeles to revert to election from districts, suggests that those who favor more citizen input into school decisions had intuitively grasped what Zeigler and Jennings are documenting. Although maintaining objectivity in reporting these findings, I think the authors would prefer more politics and less expertise in the running of our nation's schools.

Other findings in this study should prove of heuristic as well as practical value. For example, practitioners might wish to peruse the findings of board-superintendent interaction; scholars might enjoy exploring the fact that right-wing groups do not appear to be as involved in school politics as left-wing groups. Concerning the latter, one might pick up a hint given by the authors, which is developed more fully by critics of schools in general—that one need not organize to influence something that one agrees with. The point being, Zeigler and Jennings have provided a wealth of data and suggestions from which the study of school governance might emerge as an important component of political scientists' research.

ROBERT S. ROSS

California State University, Chico

Comparative Politics

Economic Growth and the Public Sector in Malaya and Singapore, 1948–1960. By Lee Soo Ann. (New York: Oxford University Press, 1974. Pp. 192. $20.00.)

Rising interest in the comparative study of public policy has focused attention on the paucity of data and case studies on public policy for Third World countries. A small segment of that void is filled by this case study of Malaya and Singapore during the period from 1948 to 1960. In this book, the author explores the relationship between public fiscal policies and economic growth. He begins without stating any theoretical propositions or enunciating any specific developmental strategy, but he does state his preference for an open economy in which foreign investment and enterprise are encouraged, but with government playing a positive and guiding role to convert trade and private investment into a "catalyst for sustained growth" (p. 4). The book proceeds on the assumption that government expenditures and investments were critically significant factors in sustaining the fairly impressive economic growth of Malaya and Singapore during the period under examination. By his analysis of public expenditures, revenues, trade policies, and shifting budget priorities, the author seeks to identify those public policies that were effective in creating conditions for balanced economic growth.

While the book presents a wealth of information on government revenues, investments, expenditures, and fiscal policies, the author is unable to come to grips with the central problem of isolating the effects of public policies from "environmental factors" beyond government control, such as world market conditons, the availability of private investment capital, and the accessibility of foreign assistance. It is of some significance to note that the years covered by this study almost exactly encompass the period of the Communist guerrilla insurgency in Malaya. This was also a period when the Korean War and the stockpiling policies of the United States government drove up the prices of rubber and tin, the two largest trading commodities produced in Malaya. Because of export duties on these products, government revenues rose dramatically during this period. In numerous tables and in the text, the fiscal configurations of all these factors are described, but there is little analysis of the impact of specific policies on overall economic performance. Instead, the author presents piecemeal observations on factors that he identifies as probably contributing toward stimulating various aspects of the economy. He suggests that government should have diversified its tax base and engaged in more domestic borrowing. He also concludes that a large share of the unexpected government surpluses accumulated during this period came from consistent underestimation of revenue and overestimation of expenditures. These budget surpluses helped to pay for the heavy costs of counter insurgency operations, but they were also able to be diverted to infrastructure investments, for social services and for the costly rubber replanting scheme, all of which, in the long run, stimulated productivity and economic growth. While a number of other economic policies of government are mentioned with approval, it seems rather paradoxical that one of the most important aspects of successful economic policy was derived from conservative errors in estimating government revenues and expenditures.

While political scientists may not find the economic analysis particularly relevant to their interests, the large amount of data collected on revenues and expenditures for Malaya and Singapore reveal the overall patterns of government priorities. Consequently, this book could be utilized as an important secondary source for public policy analysis of Malaya and Singapore. The book gives only slight attention to the processes of economic planning and public policy formation, and completely ignores the operations of the Economic Planning Unit which was established within the Malayan prime minister's department to assist the cabinet in the preparation of economic development plans. Some of the reports of this agency could have been put to good use in this study, but may not have been available to the author at the time of his research.

In the years since 1960, many important changes have occurred in the economies and public policies of Malaya and Singapore. The formation of the wider Federation of Malaysia in 1963 brought problems of integrating the economies of the member states. Singapore's exit from the federation two years later prompted that state to engage in a crash program of expanded industrial production and services geared for customers and trade on the world market. International oil companies began a massive development of off-shore oil resources. In response to the traumatic racial riots of 1967, Malaysia embarked on an ambitious and controversial New Economic Policy designed to effect a dramatic racial redistribution of wealth, ownership of industry, and occupational patterns in all sectors of the economy. For both Malaysia and Singapore, political and social considerations received far higher priority in economic policies and development planning. This volume would have been more valuable had it been extended to cover these more recent developments. In its present form, limited to the years from 1948 to 1960, the book should have been published at least a decade earlier, and even then it would have attracted the attention of a small and specialized reading public.

GORDON P. MEANS

McMaster University

An African Dilemma: University Students, Development, and Politics in Ghana, Tanzania, and Uganda. By Joel D. Barkan. (Nairobi: Oxford University Press, 1975. Pp. vii + 259. $15.75, cloth; $9.00, paper.)

An African Dilemma presents an overview of university students in Ghana, Tanzania, and Uganda, emphasizing how they conceive of their present and future roles in society. The study is based on three surveys in 1966 and 1967 involving self-administered questionnaires filled out by 1,900 students at Makere University College in Uganda, the University College, Dar es Salaam, and the University of Ghana, Legon. After a review of basic student values and career intentions, the book examines perspectives on national integration and economic development, political participation, political socialization, and perceptions of incumbent elites. In a provocative final chapter, Barkan considers the implications of his findings for development and political change. Appendices include a section on methodology and the text of the questionnaire.

Barkan's data indicate that social background is not a major determinant of student values and role orientations. Moreover, he argues that the educational system tends to "homogenize" social distinctions such as ethnic origins. Students expect to become part of an upper-middle-class technocratic elite working in state-supported activities, primarily education, the professions, and the civil service. Success is equated with personal and/or communal security, making a contribution to the development of the country, and doing one's job well. Elite status, per se, is not something students strive for. Nor do the students appear to have grandiose ideas about their career possibilities: desired and expected occupations coincide.

Students consider their countries' most pressing problems to be developing the economy and promoting social integration (". . . defined as the progressive elimination of class and ethnic cleavages through an increase of the proportion of a society's population which interacts within its national institutions" p. 85). However, few students choose careers in agriculture, the primary economic sector in need of development. Likewise they are somewhat indifferent to the existence of elite/mass gaps, rationalizing that a technocratic elite makes a significant contribution to the developmental process. Barkan suggests, as did Coleman some years ago, that such indifference is a potentially dysfunctional result of the educational system and may be the cause of more serious class conflict.

Although students intend to "work hard at their jobs" to further development, they are unsure about what economic or political model their nations should follow. Most opt for a mixed economy and favor a technocratic political order that Barkan considers has authoritarian overtones reminiscent of pre-independence administrations. Few students seem to question the existing political system except as it affects their material interests. Compared with students in other Third and Fourth World countries, their political participation is only marginal. They seem unlikely to challenge existing elites even though they describe some of them in pejorative terms. For upwardly mobile students, quietude is both rational and a survival technique.

Although this concise and cogent book gives us a fuller view of students than is found in the many case studies based on single African countries, Barkan seems ambivalent in his assessment of what his findings mean for the future. He sees the students contributing reliability but not creativity to development, change, and stability, and he rightly questions whether this is sufficient. In one passage Barkan states: ". . . it is clear that existing procedures for granting status and material security in Africa, and the values communicated by the educational system must be changed if the prospects of development are to be raised" (p. 197). But how is this to be done and by whom? Political order and stability may be correct virtues leading the way as Barkan suggests, but every tyrant as well as democrat argues that. The dilemma is not where students fit in but what they fit into and toward what ends.

Anyone who has attempted survey research in Africa will appreciate the effort behind this study. Although I have questions about Barkan's decision to present a participation scale based only on respondents whose answers met Guttman criteria, it is still an informative and well-written book. I believe it belongs on the shelf of anyone concerned with students, development, and the role of education in society.

DAVID J. FINLAY

University of Oregon

Patterns of Political Development: Japan, India, Israel. By Roger W. Benjamin, with Alan Arian, Richard N. Blue, and Stephen Coleman. (New York: David McKay, 1972. Pp. xi + 192. $9.95, cloth; $4.95, paper.)

A variety of possible relationships between the three main variables is discussed briefly in the introduction and then data from Japan, India, and Israel are examined with these hypotheses in mind. Generally, literacy and levels of urbanization and industrialization are related to associational activity, which in turn is related to participation operationalized as voter turnout, party competition, support for minor party candidates, and so on. Since each country is unique in some respect and since different kinds of data are available from each, so the variables are constructed in slightly different ways for each country. For example, districts are compared in India and prefectures in Japan, but five different settlement types spread over the country are compared in Israel. Not forcing data into a preconceived pattern is a commonsense solution to widely discussed problems of data collection which does not seem to detract from the results, since similar patterns are still detected in the three countries. By extensive use of graphs, one of the book's strongest points, Benjamin has managed to restore the visual component to the word "patterns" so that even the statistically untutored can understand the relationships evidenced by the data.

One way in which the graphs are particularly effective is in showing that most relationships between the variables are not linear. For example, a finding that supports the suppositions of other theoreticians is that the proportion of people voting tends to decline after some critical amount of urbanization is reached, although the two variables are positively associated at lower values of urbanization. As the authors note, the nonlinearity of relationships would be still more obvious if data were available over a period of time.

There are a few difficulties in this as in any book. The introduction contains the by-now obligatory discussion of problems of conceptualizing and constructing indicators for modernization, along with a brief discussion of the "natural science trend" of hypothesis building and testing in the social sciences. I am sure it increases the scholarliness of the book to refer to previous literature on the subject, but the section offers no new interpretations or solutions to the difficult problems of defining modernization or building hypotheses about it. The conceptual fuzziness of this section contrasts poorly with the clarity of the data manipulations that follow and detracts from Benjamin's implicit argument that pragmatic solutions to the problems are most helpful at this point in our understanding of modernization. In addition, since it is patterns we are looking for, it would be more useful to have the summaries at the end of the case studies more directly parallel. The chapter on Israel, for example, has a wonderful page of summary graphs but the other two studies have helpful verbal recapitulation; both would be helpful in all three chapters. Also each case requires a second reading in order to follow exactly what form the hypothesis is taking and how it will be tested for that country.

Despite these few drawbacks, I would recommend this book to anyone trying to teach realistic yet creative research design, to anyone interested in modernization, or to anyone concerned with a common sense approach to the problems of scientifically interesting patterns in data. In short, almost all political scientists will profit from the graphs and the sensitive approach to aggregate data employed here.

SUSAN G. HADDEN

Clark College

Legislative Systems in Developing Countries. Edited by G. R. Boynton and Chong Lim Kim. (Durham, N.C.: Duke University Press, 1975. Pp. 286. $11.85.)

Despite the growth of a substantial body of literature in the past decade, much remains to be learned about the development and functioning of legislatures in various political systems. This is perhaps especially true for "developing" systems where empirical research is oftentimes difficult and fraught with a host of theoretical and methodological pitfalls. The first book-length publication of the Consortium for Legislative Studies (general editor: Malcolm E. Jewell), *Legislative Systems in Developing Countires,* represents the collective effort of several scholars to further knowledge of legislatures and legislative behavior in a number of Third World polities.

Structurally, the book is divided into three sections entitled "Legislatures and Goal Setting Function," "Legislatures and Conflict Management," and "Legislatures and Integrative Function," respectively. Under these headings are placed a series of seven substantive papers and two general theoretical essays. Legislatures studied in detail include those of Japan, Thailand, Colombia, South Vietnam, Kenya, the Philippines, and Korea. Additionally, one of the "theory" papers discusses at some length the historical development of legislatures in Iran, Turkey, and Afghanistan. As is typical for a book of this type, the entire set of papers is preceded by an introductory chapter by the editors.

The basic fact that legislatures in most political systems—including all of those studied in the several papers in the Boynton-Kim volume—are "weak" in the sense of playing at best a "minimal" role (the term is Mezey's) in what the editors call the "goal setting" function is recognized by the editors as well as each of the contributors. Recognition of the sharply truncated role that legislatures have in policy-making processes leads the researchers to explore alternative functions performed by these bodies. Hence, the previously mentioned focus on "conflict management" and "integration." The shift in attention to these functions leads many of the authors, when studying the behavior of individual legislators, to explore such phenomena as representational role orientations and constituency service behavior. The assumption underlying examinations of the attitudinal and behavioral linkages between legislators and their constituents would seem to be that interactions between legislators and constituents may constitute an important device by which political support is generated. Intuitively, such an assumption seems quite plausible.

Two caveats, however, are in order. First, neither the authors nor the editors provide the empirical evidence necessary to sustain the assumption that legislators' attitudes and behavior influence support levels significantly. Indeed, only one paper (a study of the Kenyan legislature by R. F. Hopkins) presents any data whatsoever on *citizen perceptions* of the legislature. In brief, as Wahlke and others have recently noted, the extent to which legislatures as institutions and legislators as individual political actors influence political support levels remains an important task for future research, with only a handful of inquiries on this topic being currently available.

Second, and relatedly, none of the several contributors nor the editors seem sufficiently cognizant of the distinction between support for the legislature and support for the larger political system. If, for example, legislators, through their representational and constituency service behavior contribute primarily to the development of a sense of diffuse support for the political system *as a whole,* then, paradoxically, the behavior of legislators in systems where legislatures are currently "minimal" may be contributing (albeit perhaps unknown to the legislators) to the maintenance of the status quo. Alternatively, it may be that legislators' performance of representational and constituency service activities contribute to the generation of support for both the legislature, as a perceptually distinct political structure, and the larger political system. Theoretical expectations regarding the relevance of the behavior of individual legislators for understanding the structure and functioning of legislatures as institutions will be greatly strengthened when information on these matters is available.

The comments above should not be taken to imply that the papers in the Boynton-Kim volume have serious conceptual deficiencies. On the contrary, in most instances, the authors demonstrate considerable conceptual sophistication. Particularly noteworthy is a commendable sensitivity to historical and cultural factors influencing legislatures and legislative behavior in non-Western settings. Most of the authors strike a judicious balance between studying the legislature as an institution rooted in the historical and cultural circumstances of a particular polity and investigating the attitudes and behaviors of individual legislators. Additionally, attempts are frequently made to assess the broader relevance of micro-level findings.

Regarding the behavioral research, all substantive papers are based on substantial bodies of survey data. For the most part, analyses of these data are performed in a straightforward and competent fashion. There are minor exceptions. Styskal, for example, in his paper on the Philippines, incorrectly assumes that "beta weights" can be squared to yield the percentage of variance explained (p. 243). Although distressing when encountered, such methodological errors are happily rare and do not invalidate the general thrust of the arguments being made.

In sum, the several papers in this volume make a definite addition to our knowledge of legislatures and legislative behavior in non-Western countries. One could perhaps summarize the quality of work done by saying that the contributions are "solid" rather than "outstanding." As the first in a projected series of books on comparative legislative research, the generally high standard of the papers in this initial publication suggests that more valuable work can be expected from the Consortium for Legislative Studies in the future.

HAROLD D. CLARK

University of Windsor

Lunda under Belgian Rule: The Politics of Ethnicity. By Edouard Bustin. (Cambridge, Mass.: Harvard University Press, 1975. Pp. xx + 303. $11.00.)

The Lunda state, in southwestern Shaba (alias Katanga), Zaire, is one of the most prestigious political configurations of Central Africa. Historically, this derives from the central role played by the Lunda state from 1600 on in the dynamics of state formation in the savanna regions of what are today Zambia, Zaire, and Angola. This impact was felt partly through its own expansion, partly through the diffusion of its forms, and partly through the periodic secession of rebel fragments who then replicated many of its structures. With the independence era, occasional speculation about a reconstituted "Lunda Empire," and the incandescent, ultimately tragic career of the sometime Prime Minister and erstwhile secessionist leader Moise Tshombe, of Lunda royal connections, combined to renew the aura of the kingdom. Even today, the Lunda paramount rulers are among a handful of chiefs whom the centralized Mobutist state has been unable to totally eclipse or dominate.

Although in the universe of Zairian precolonial societies it is by no means a typical example, Lunda is nonetheless an arresting case, which merits the meticulous attention it has received from Professor Bustin in this admirable book. The central focus of his inquiry, as suggested by the first part of the title, is the interaction between the Lunda institutions and Belgian colonial administration (though Lunda domains spilled over into Zambia and Angola, the seat of the kingdom was in Zaire). However, he completes the study with intriguing chapters analyzing the formation of the Lunda state and its nineteenth century crisis on the eve of colonial conquest, and the fate of Lunda during the turbulent first decade of independence. This perspective is more distinctive than it might at first appear; political scientists who have examined African historical states have tended to do so in interaction with nationalist movements and political parties (Lemarchand, Apter, Whitaker), while anthropologists have focused upon the African society somewhat abstracted from the colonial overlayer. Now that centralized, authoritarian regimes seem in the ascendant, the insights derived from the Bustin approach become quite pertinent. Parties no longer mediate between administration and those historical states that survive; the bureaucracy seeks devices to bind these local structures to its purposes.

Any system as distinctive as Lunda is bound to have attracted the analytical notice of administrators and anthropologists (Biebuyck, Crine, Duysters, Hoover), and Bustin has their work to draw upon. However, the major underpinning for his inquiry was in the administrative archives. These have been quite thoroughly exploited, in field work undertaken in 1967, 1968, and 1972. He also did some interviewing of Lunda notables, and Methodist missionaries and Belgian administrators who have labored in the area. Some may regret that more recourse was not made to interviewing, exploiting in party the analysis of the arcane craft of oral tradition that historians have developed. Yet the archives themselves are a quite rich source, and the solidity of the documentation is beyond dispute. Bustin's impressive immersion in the extensive historical and anthropological research which has appeared in recent years provides the equipment for creative utilization of the archival material. In its multidisciplinary perspective and breadth of vision, the present work seems light years away from Bustin's first, heavily institutional book, *La decentralisation administrative et l'évolution des structures politiques en Afrique orientale britannique.*

Bustin skillfully relates the enduring principles of Lunda political organization, positional succession and perpetual kinship, to the organizing concepts of Belgian administration, preservation of the colonial order, labor recruitment, and cash agriculture, lightly overlaid by the influences of "indirect rule." Above all he demonstrates how much scope there was for the impact of a particular administrator, for the dependent manipulation of the colonial system by power contenders within the Lunda orbit, for major fluctuations of policy over time. Global characterizations of Belgian colonial policy tend to obscure the range of variation on these important dimensions.

The subtitle for the book is unfortunate, as Bustin is not centrally concerned with ethnicity per se. He does offer a brief and interesting analysis of the basis of Lunda identity, which is above all political and linked to the kingdom and not linguistic. However, this is not related to the ethnicity literature generally, nor do we receive an adequate treatment of the bases of solidarity among the most important "relevant others" for Lunda: the Cokwe, an historically related cultural system, whose hostile symbiosis with the Lunda state created many of the most difficult issues for central administration, colonial and postcolonial.

Overall, Bustin merits our praise and gratitude for this carefully documented study, which is a valuable contribution to our understanding of political change in Central Africa.

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Lectures in China, 1919–1920, *by John Dewey*. Edited by Robert W. Clopton and Tsiun-Chen Ou. (Honolulu: The University Press of Hawaii, 1973. Pp. 337. $12.00.)

Improbable though it seems today, there was a time, some half a century ago, when the ideas of John Dewey appeared more likely than the theories of Marx or Lenin—not to mention the then unknown Mao Tse-tung—enduringly to influence the destiny of China. In the late 1910s and early 1920s, Dewey had a considerable following among "progressive" Chinese intellectuals, including several of the most esteemed leaders of the iconoclastic "New Culture Movement" who had themselves been his students at Columbia. More to the point, Dewey and his wife arrived in China in May 1919—coincidentally at the beginning of a period of feverish academic and political upheaval—for a lecture tour which lasted, finally, more than two years. During this time, Dewey travelled to almost every important intellectual center in the country, usually accompanied by one or another of his eminent Chinese disciples, to expound in person his philosophy of education and his social and political views. It was, as many observed hopefully at the time, an unprecedented intellectual occasion and opportunity.

Lectures in China, 1919–1920, is a memento of that encounter, so auspicious in its time, now so anachronistic. The book comprises only two of the several series of lectures which Dewey delivered in the course of his stay (some 30 out of a total of perhaps 150 formal presentations), one on "Social and Political Philosophy," the other entitled "A Philosophy of Education." Both were delivered in the winter of 1919–1920 at Peking National University, China's premier institution of higher education and the indisputed center of intellectual radicalism. As with all Dewey's China lectures, these were given in English, translated paragraph-by-paragraph from the platform by one of Dewey's Chinese hosts, transcribed in Chinese, and in this form published in the periodical press and in collections, some of which went through many printings.

Unlike Dewey's lectures at Tokyo Imperial University some months earlier, which were later published as *Reconstruction in Philosophy,* the English texts of his lectures in China, or the notes from which he spoke, did not survive. What he had to say to his Chinese audiences can be gleaned only from the now scattered Chinese versions or, laboriously, from retranslations such as these. Dewey scholars are not likely to find here anything new or startling, though Dewey's awareness that he was breaking new ground for most of his listeners, and the fact that several of his interpreters were themselves men of wit and eloquence, give these lectures a cogency and directness rarely found in his more formal writings. The real usefulness of *Lectures in China,* however, is to the historian interested in the transformation of Chinese thought in the early decades of the twentieth century. Dewey was an eminently commonsensical advocate of a certain strain of American liberal values: a sustained tradition of intellectual self-criticism; political civility and moderation; free access to, and unhampered dissemination of, information; socially responsible individualism. We have in these lectures a fair approximation (what translation can be more?) of opinions that circulated widely among Chinese intellectuals and had a significant, albeit transient, impact on the manner in which they assessed their situation and their options. The fact that Dewey was so often frankly critical of the traditions he sought to explain may well be a factor in their ultimate failure to act upon the values he expounded.

Professors Clopton and Ou preface the translations with a minimally helpful introductory essay that sketches the context of events within which Dewey's China visit was conducted and traces the rise and decline of his influence. Or perhaps one should say, of his popularity, since the question of influence is intentionally evaded. The introduction was apparently written several years before *Lectures in China* was finally published, and therefore does not benefit from much recent scholarship which has substantially enhanced our understanding of the period and of several of its leading personalities. More useful is an appendix which lists a number of additional lectures (some with brief abstracts of their content) which, though not printed in this volume, have been translated and are available in reprographic copy.

JEROME B. GRIEDER

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In the Spirit of the Red Banteng: Indonesian Communists Between Moscow and Peking, 1959–1965. By Antonie C. A. Dake. (The Hague: Mouton, 1973. Pp. vii-xvi + 479. Guilders 48.)

Indonesian Communism Under Sukarno. Ideology and Politics, 1959–1965. By Rex Mortimer. (Ithaca, N.Y.: Cornell University Press, 1974. Pp. 464. $15.00.)

Among the most remarkable developments in post-World War II Asian history, was the almost meteoric rise, out of total disarray and discredit, of the Indonesian Communist Party (PKI) in the period from the early 1950s until September 30, 1965. On that date a new debacle overtook the party, as a result of its involvement along with dissident Indonesian military in an abortive coup attempt. The era of the PKI's present ignominy dating from this 1965 coup, has by now lasted almost as long as the time span of its rapid and spectacular rise to power in the closing years of Sukarno's presidency, and this points up the fact that in all of its more than 55-year-old history (the PKI is the oldest Communist party in Asia) the PKI's legitimacy, let alone its expansion and influence, have been the exception.

Both studies here under review address themselves to what could without exaggeration be called the PKI's first "golden age" thus far, seeking to analyze the party's role both in terms of Indonesia's domestic and foreign policies and in relation to the Sino-Soviet dispute. Dake's book, by far the superior of the two, has, however, a more predominantly international focus, and—while by no means neglecting the party's critical domestic relationships toward the Indonesian Army, the other political parties, Muslim regional dissidents, and so on—measures the PKI's influence primarily by the major turning points in Indonesian foreign policy, e.g., the effects of Khruschev's Asian policies, the West Irian question, the Malaysian confrontation, Moscow's resultant coolness, and the partnership with the People's Republic of China. Dake's research has been exhaustive on each of these issues and is altogether persuasive that the party's position, e.g., with respect to the Malaysian question, became influential if not normative for the government as well.

Both Dake and Mortimer, although the latter to a greater degree, accentuate what might perhaps be termed the PKI's nativistic or "neotraditional" tactics and appeal, i.e., the particularly close correlation between these tactics and indigenous, non-Western political styles and social values, re-enforced by the changing character of Indonesian nationalism after the struggle for independence had been won in 1949. The essence of this PKI "nativist" policy had already been described years ago, but it is useful to have it detailed again. There is little doubt, as Mortimer demonstrates, that the PKI, seeking to become integrated in the complex balancing process of political institutions, and while never discarding its function as a "modernizer" of the socio-economy, made considerable effort to adjust itself to the religious syncretism of the so-called *abangan* life style and political culture in Java, just as, in its foreign outlook, it seemed to drift to the activist Asian nativism projected by Peking's self-conceived role in the Third World.

Perhaps the most useful section of Dake's book is just this Indonesian Communist (and eventually officially Indonesian) orientation on People's China, and Dake's factual accounting of the Chinese readiness to assist in the arming of the PKI and its allies presumably in the context of preparation for seizure of power, is to be especially recommended to those commentators who, despite all evidence to the contrary, persist in ignoring or minimizing Peking's involvement in the 1965 coup. Dake's analysis admirably integrates the government's and the party's relationship with Peking—the latter in preparation for the coup—with the dynamics of the PKI's campaign of domestic radicalization.

One aspect of this mobilization for an eventual seizure of power, as described by Dake, however, has made his book particularly controversial. This is the role of Sukarno in the preparation of the 1965 coup. It is Dake's contention that it was Sukarno—weary of the opposition of key army commanders who were opposing his policies of close alliance with Peking, and who were aligning themselves against the general radicalizing impetus in domestic affairs (what Sukarno once called his nation's "revolutionary gymnastics"), in which the PKI played such an important role—who suggested and encouraged Lt. Col. Untung, a key officer of his own presidential guard, to "take action" against the "disloyal" army generals. Some PKI leaders, among them party chairman D. N. Aidit, who, Dake seems to suggest, had been making their own coup preparations, became aware of Sukarno's wishes and associated themselves with the schemes of Untung and other military dissidents, proceeding to the denouement of the abortive coup and their own eventual catastrophes. While some authoritative analysts of the 1965 coup are agreed that Sukarno probably had some and perhaps a good deal of information about the plans of Untung, Aidit, and their associates, convincing proof that Sukarno, even indirectly, was the coup's instigator has simply been lacking. The evidence of Sukarno's aide de camp, Bambang Widjanarko, on which Dake's argument of Sukarno's initiating role appears to rest so heavily, and which, with an introduction by Dake, was published separately a year after Dake's own book, raises the most serious questions as to its legitimacy and credibility (as I indicated in *Pacific Affairs,* Summer, 1975, pp. 285–87).

This lapse (surprising in a study by one, like Dake, trained as a jurist) is, however, a rarity in these pages. Where Dake's account—as for example in the sections on the PKI's and the Sukarno regime's policies toward China—can be compared with related studies by others, *In the Spirit of the Red Banteng* shows itself to be an invaluable source.

Though giving a comprehensive picture of such matters as the interplay of the PKI's and Sukarno's ideologies, the party's relationship to strata changes in Indonesian rural society, the effect of domestic economic deterioration in the Sukarno years, and the catalytic effect of the Irian and Malaysian confrontation campaigns, Mortimer's study, compared to Dake's, is generally more interpretive, attempting to distill the essences of the PKI's strategy and its programs as the power struggle unfolded within the Sukarno regime. As such, Mortimer only rarely breaks new factual ground. Hence to those already familiar with the course of PKI history and tactics, his book's value will probably depend on the quality assigned to a number of Mortimer's basic value judgments about the party's operations and presumed intentions.

Unfortunately these judgments seem to depend heavily on knocking down straw men. For example, even in the preface, Mortimer seeks to break down a number of what he calls "stereotypes of Indonesian Communism," which, he asserts, arose from "a tendency on the part of many Western academics writing about Indonesia" to "denigrate" the PKI's role in Indonesian national life. One such stereotype, according to Mortimer, is to hold the view that the millions of Indonesians who at one time supported the PKI did so "only" in order to articulate their reaction to a painful process of "social change." This particular stereotype, says Mortimer, does not do justice to "any rationally conceived interests" on the part of the supporters of the PKI, nor "to the character" of the PKI itself. The present reviewer is not aware of "many Western academics" concerned with Indonesia who, in fact, would deny that those who supported the PKI did so without considering "any" of their "rationally conceived" interests." Indeed, this reviewer doubts the premise, and he would question whether in Indonesia, or anywhere else in the world, to be aware of one's subjection to difficult "social change" (not further defined) can be confidently separated from a "rational" understanding of one's interests.

Another stereotype alleged by Mortimer, is or was the fear "in the West" that a Communist Indonesia would have marked the beginning of an era of Chinese "domination" of Southeast Asia. The PKI's "nationalism" and policy independence, according to Mortimer, would have prevented this. It should first of all be noted that the ever closer relationship between certain Indonesian and Chinese policies in the 1963 to 1965 period, and the role played by the essentially Mao-oriented PKI in that relationship, was a matter of concern not just "in the West," but, e.g., in Malaysia, Singapore, and the Philippines as well. Secondly, it should be observed in respect of the PKI's "nationalism" and its "independence" acting as a barrier to alleged Chinese "domination," that it was precisely during a period when party pronouncements laid maximum stress on the PKI's "national" and independent role that the Sino-Indonesian partnership, and the pro-Maoist orientation of Indonesian communism was most apparent. After reading Mortimer on this point, Dake's account of the Sino-Indonesian rapprochement is a particularly welcome corrective.

Mortimer's own conceptual "stereotype" of the PKI is perhaps revealed in such observations as that it is "at least arguable" that the PKI "represented the most hopeful" prospect for economic growth and social betterment in Indonesia, and that the present Suharto era of "ruthlessness of men in power" and the "misery and powerlessness" of the masses today, should be compared with the *hypothetical* future of an Indonesia dominated by the PKI as revealed in the party's "ideology and actions." Given this kind of perspective, it is perhaps not surprising that Mortimer's study ends with his "preferred interpretation" of the 1965 coup in which PKI leaders appear as having been "manipulated" rather than having been among the manipulators. Of a piece, in this connection, is Mortimer's categorical assertion (in the context of a two-sentence treatment of the Chinese role) that a Chinese connection with the coup "was not established," despite reports to the contrary. This statement incidentally appears on the same page (p. 423) as Mortimer's one and only reference to Dake's book, which would have seemed to merit considerable attention considering its extensively detailed analysis of the problem of the coup's possible Chinese connection. However, instead of dealing with the question of this Chinese connection, which might perhaps have been expected of one concerned to demolish the alleged stereotype of China's dominant role in Southeast Asia, Mortimer's sole specific reference to Dake deals with the unreliability of the Widjanarko testimony, though Mortimer vaguely refers also to other "conclusions" of Dake which he is "unable to accept." Touches like these should continually prompt a reader of Mortimer's pages to considerable caution.

JUSTUS M. VAN DER KROEF

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Den Herskende Klasse (The Ruling Class). By Jörgen S. Dich. (Borgen, 1973. Pp. 244. D.Kr. 45.)

The Scandinavian welfare state is frequently hailed by foreign admirers as a model for emulation by other Western nations. Here, it appears, is found that most elusive blend of freedom, security, and economic well-being associated with a highly developed economy. Scandinavians themselves do, by and large, share this positive appraisal of their situation; they are a patriotic lot, and generally given to the view that theirs is, indeed, a good life.

Nonetheless, recent years have seen mounting criticism from both the right and left side of the political spectrum, and charges of either too much or too little socialization. Maverick parties, such as Anders Lange's party in Norway, have cropped up on the Right, while on the Left, dissident voices demand more wholehearted commitment to socialist transformation of society.

Not the least interesting aspect of this polemical and provocative book by the Danish Professor Jörgen S. Dich, are the hardhitting punches delivered both to the Right and to the Left as he mounts a scathing attack on the form the Danish welfare state has assumed. As a Marxist, Dich proposes to undertake a "Marxist analysis of class contradictions in our day" (p. 133), and submits that a new ruling class has developed, consisting of the functionaires providing the services of the welfare state, particularly the personnel in the health, education, and welfare sector of the economy. The "servant class" has become the ruling class, and the social policy it designs reflects, as the social policy of any era will, its own class interests.

According to Professor Dich, the almost cancerous growth of the public service sector since 1960 amounts to social exploitation, for the expansion of social services has reached a point where their utility is extremely diminished, and often negative. No cost-benefit analysis of the ruling class's services is ever undertaken. On the contrary, new jobs are constantly created in response to new "needs" discovered by experts. The cost is borne by the common worker through taxation, and the beneficiaries of this social exploitation are the middle class functionaries and their offspring. The smooth functioning of the system is aided by the fact that the ideology of the ruling class has become the social ideology. It is an ideology which conforms to strong popular instincts, rooted as it is in humanist compassion, the desire for health, and fear of death, as well as in a middle class contempt for manual work. Thus, "the working class has let itself be fooled to pay for the middle class ambitions, based in its contempt for manual labor" (p. 108).

The butt of Professor Dich's greatest wrath is the educational system, from preschool to university level. Pointing to the low proportion of working class youth among the university students, he sees the institutions of higher learning as the playgrounds of middle and upper class children.

> The status symbol of the middle class is their children's career. Those whose children become workers, lose face. In no other area is it more clearly demonstrated that we live in a class society. Since there are increasingly more areas where one can get a higher education free, and since the middle class has grown simultaneously, we are confronting an educational explosion where no one thinks of what the common man has to pay for the social ambitions of the middle class (p. 101).

One of the first priorities for system reform must therefore be in the educational sector, and Dich proposes reintroduction of self-financing of higher education as a means to reduce recruitment to the ranks of academicians. Limited enrollment, with high school grades determining university acceptance, an increased student-teacher ratio ("Whether there are lectures for 30 or 300 is pedagogically indifferent. In both cases it is just one way traffic" p. 149), and reduced salaries for academicians ("those for whom the work is important will thus be attracted to the field" p. 156), are some of his proposals for pruning this sector.

While Professor Dich in his anger may have overstated his case, his book has nonetheless sharpened the attention of Scandinavians to the pressing problem of a proliferating and inefficient bureaucracy, and the possibility of the reduction of the citizen-body to the status of minors, as their lives are governed in more and more detail by experts. The value of the book lies in its marshalling of evidence that the Scandinavian welfare state may have reached the saturation point both economically and psychologically. It deserves to be read by all who profess interest in Scandinavian politics, and as it poses problems that extend beyond Scandinavia's boundaries, it may be read with benefit by those whose interest is in the welfare state as such. It deserves translation.

INGUNN NORDERVAL MEANS

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The Political Economy of Africa. Edited by Richard Harris. (New York: Halsted Press, 1975. Pp. ix + 270. $15.95, cloth; $6.95, paper.)

With a broad-ranging introductory chapter and five others focusing on specific African states (Ghana, Nigeria, Zambia, South Africa, and a chapter comparing Kenya and Tanzania), this volume examines relationships between economic and political factors—a key nexus being that between the African state and alien economic power. Most of the contributors are concerned with the state's role in promoting, tolerating, or attempting to overcome (1) the economy's typically heavy dependence on imports for both consumer and producer goods and on sales in foreign markets of only a few primary-product exports to finance its import bill; and (2) the usually dominant role played by foreign private enterprise in mining, manufacturing, and other sectors. Zambia, for example, has relied on one industry, copper mining, for over 90 percent of its export earnings and about 60 percent of government revenues.

The chapters on Zambia, Kenya, and South Africa give a particularly useful analysis of the penetration of alien enterprise during the colonial era (in South Africa, the continuing quasi-colonial period) and demonstrate the white-controlled state's promotional activities vis-à-vis European settlers and investors and its regressive impact on African farming and commerce. Legassick's economic interpretation of South Africa's *apartheid* depicts a system derived from the demands of white farmers for protection from African competition for land and markets and of mining companies for abnormally cheap labor. The state's response had two basic facets: to restrict African agriculture severely (to 13 percent of the total land) and to limit permanent African settlement in cities. This forced blacks to become migrant laborers in the white sector and, along with rigid residential segregation, prevented them from organizing effectively against white-controlled economic and police power.

In assessing the state/alien-enterprise nexus during the postindependence period, the book's major theme is that foreign economic interests continue to exercise strong influence; hence, the relationship is "neo-colonial." This plausible and important (but scarcely new) hypothesis deserves the difficult effort of empirical testing through comparative decision-making studies. The volume under review does not take that needed approach, but in its largely speculative analyses it does provide some promising subhypotheses. One (developed in the chapters on Nigeria and Kenya) is that alien influence is exercised through local political elites who find their pocketbooks and/or career-survival interests served by a status quo of smoothly functioning established foreign enterprises and by continued new direct investment which yields them "commissions," employment opportunities for their supporters, tax revenues to fund social services, and other benefits. Another (from the Ghana chapter) is that foreign-owned extractive enterprises use their current and potential export-earning capacity to bargain successfully for favored treatment—especially during balance of payments difficulties for the host country.

A third, perhaps unintended subhypothesis is that foreign private capital's influence has not been decisive, if even present, in postindependence conflicts over regime or government change. The four chapters on independent black-ruled states offer no evidence of significant investor involvement—direct or through their home governments or alleged local allies—in the two coups and intervening election in Ghana, the prolonged regime instability in Nigeria, or the threats to the survival of one-party governments in Kenya, Tanzania, and Zambia. As these same chapters indicate, most of the conflicts have been due to ethnic and/or regional rivalries, with little economic policy differentiation to tempt foreign investors to risk intervention on one side or the other.

A fourth interesting subhypothesis is that foreign enterprise has importantly influenced postindependence politics through its unwilling role as a "pork barrel." Under pressure to provide tangible benefits from independence, political elites in Kenya and Zambia, among other countries, could satisfy many followers by simply redistributing land, shops, salaried jobs, and other existing economic opportunities from aliens to nationals. Where large foreign-merchant and landowning groups did not exist, elites had the much more demanding task of engineering new opportunities.

The book's first four chapters argue that African states would benefit from ending or severely restricting foreign private investment and from reorienting their economies from producing for export to meeting internal needs. We are told that Africa must "disengage . . . from international capitalism" (p. 40) and that Tanzania should work toward "developing its own productive capacity and limiting its involvement in the international economy" (p. 185). However, given its limited market and raw materials endowment can the typical African economy realistically be expected to produce much of its manufactured producer and consumer needs? And can it afford to neglect its export sector while groping toward a feasible degree of self-reliance? Ghana under Nkrumah attempted an "economic independence" strategy but clearly flopped at it. Import-substitution enterprises established by the government in manufacturing and farming tended to be unproductive because of poor design and/or wasteful management. Neglect of export industries contributed to stagnation in foreign exchange earnings and the country's near international bankruptcy at the time of the 1966 coup against Nkrumah (see "L'expérience du Ghana," *L'afrique de l'indépendance politique à l'indépendance économique* [Paris: Maspéro, 1975]).

Tanzania has also embarked on a strategy of "self-reliance." Since 1967 the state has taken over virtually all significant foreign enterprises and has attempted to increase production for domestic demand by, among other means, massive resettling of peasants onto cooperative farms. However, the Tanzania chapter in this book does not tell us whether these policies have been successful. Are the nationalized enterprises producing surpluses for reinvestment, or are they, through mismanagement, eating up state revenues? Have the cooperative farms significantly increased production, or have outputs dropped because of peasant opposition to forced resettlement?

To be persuasive, the policy recommendations of this volume need to be supported by more analysis of actual outcomes: for economic growth and income distribution, for political support generated and lost. The subject—which strategy to follow in coping with economic dependence—is important enough for in-depth, comparative studies. This volume makes a helpful start in posing issues and generating hypotheses.

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Trade Union Foreign Policy: A Study of British and American Trade Union Activities in Jamaica. By Jeffrey Harrod. (Garden City, N.Y.: Doubleday, 1973. Pp. 485. $3.95, paper.)

In recent years, wide attention has been given to the foreign activities of America's gigantic multinational corporations. Contrarily, very little has been written concerning the international role played by our powerful trade unions. Jeffrey Harrod's study partially fills this void by focusing attention on certain foreign activities of the United Steelworkers of America. As an illustrative case study, the author gives special consideration to union activities in the Caribbean nation of Jamaica between 1938 and 1967. The American trade union activities in Jamaica are compared and contrasted with those of the major British union, the Trades Union Congress, during the same period.

Over half of Harrod's book consists of "background" chapters which the author considers necessary for understanding his specific Caribbean case study. These include: a theoretical setting; a general study of the international activities of trade unions; an analysis of the functions and structures of American and British trade unions with special emphasis on their policies toward less developed countries; and sections on the geographical, historical, economic, cultural, political, and trade union background of Jamaica.

The effects of the world-wide economic depression of the 1930s were particularly disastrous in Jamaica. Out of conditions of unusual economic adversity emerged the first mass political parties, the first broadly based trade unions, and the first charismatic local leaders. By the time the first legislative elections were held under universal suffrage in 1944, two vigorous political parties were able to present candidates for voter consideration. One, the People's National Party headed by Rhodes scholar Norman Manley, was closely associated with the Trades Union Congress of Jamaica. The second, the Jamaica Labour Party headed by popular Alexander Bustamante, was indistinguishable in organization and membership from the Bustamante Industrial Trade Union. For over 30 years, these two parties and their affiliated trade unions have provided Jamaica with competitive political and trade union systems. Though close ties between political parties and trade unions are not unusual in democratic nations, competition between two such dual organizations is an unusual characteristic of the Jamaican scene.

In order properly to understand political and labor conditions in Jamaica, Harrod made an extensive investigation of the cultural background of the local people. He found that about 80 percent of the population belonged to what he termed an "evolved culture," which was based on the commonly shared ancestoral experience of slavery. The other 20 percent, he found, belonged to an "imitative culture," based on attempts to copy ideas, styles, and institutions from Britain. Alexander Bustamante, founder and leader of Jamaica's first mass trade union, appealed mainly to the "evolved culture" group. He provided a highly personalized type of leadership for his organizations. Norman Manley's party and affiliated trade union, contrarily, appealed mainly to the "initative culture" group, and copied both policies and organizational concepts directly from British practice. When first British, and then American, unions became interested in Jamaica, they naturally found themselves most comfortable dealing with the kind of organizations with which they were already familiar. For this reason, the Jamaican unions associated with the minority "imitative culture" received the almost exclusive interest and attention of the foreign unions.

The major cause for American trade union interest in Jamaica came as an outgrowth of the discovery of extensive bauxite deposits on the island in the early 1940s. Within a decade of discovery, large American and Canadian corporations were extensively involved in the exploitation of the deposits, thought to be the most extensive in the world. Jamaica was soon the major source of raw material for the North American aluminum smelting industry. Since most American and Canadian aluminum workers are members of the United Steelworkers of America, this union took an active interest in union conditions in the Jamaican bauxite industry. In order to protect the source of needed raw materials, the American union wanted a stable trade union situation in Jamaica. And through American eyes, such stability would only be possible if the Jamaican trade union associated with the "imitative culture" was successful. Thus American involvement was one-sided, and involved either ignoring, or in some cases outright opposition to, Bustamante's union and party organizations.

Though the foreign unions involved in Jamaica often used high-sounding rhetoric to defend their actions, such as favoring union democracy, opposing Communism, and supporting improvement in working conditions, Harrod's investigations found "self-interest" the motivating force for their activities. Instead of the more typical North American adversary relationship of unions and management, he found that the foreign unions closely cooperated with the foreign aluminum corporations in their respective Jamaican activities. Both shared the common goals of wanting a safe investment climate in Jamaica and desiring unimpeded access to the bauxite ore. If the case study presented by Jeffrey Harrod is typical, one may conclude that motives of pure humanitarianism are no more likely to be found in the foreign activities of trade unions than in the foreign activities of multinational corporations.

CHARLES KUNSMAN, JR.

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What Government Does. Edited by Matthew Holden, Jr., and Dennis L. Dresang. (Beverly Hills, Calif.: Sage Publications, 1975. Pp. 320. $17.50, cloth. $7.50, paper.)

Political science, according to the introduction in *What Government Does,* has turned away from its early interest in public policy to study other questions. Believing public policy to be an essential aspect of political science, the editors offer this volume of 12 previously unpublished papers to remedy the discipline's inattention to public policy. The volume's major shortcoming is its lack of structure—the only common theme the editors admit is that all selections deal with "what government does." Given the objective of emphasizing the study of public policy in general rather than a more narrow theoretical or empirical objective, the reader is left awash in a potpourri of studies, unsure why all of them are important. Despite the editors' disclaimer and the tremendous variety in substance, method, framework, time period, and national context, some common threads do emerge from the book. The selections may be categorized by their emphasis on the determinants of public policy, the impact of public policy, the prescription of public policy, the impact of public policy, and the methodological issues in policy studies.

The best contributions are those that analyze the determinants of public policy. Robert Russell's paper on international monetary reform is clearly the class of the volume. Russell outlines three theories of international politics—power politics, class struggle, and transnational elites—and through exhaustive analysis makes and tests dozens of hypotheses suggested by the three theories about international monetary reform. In the only example of the Dye-Sharkansky-Hofferbert school of state policy analysis, David Brady and Richard Murray examine the relationship between malapportionment and public policy. Unlike most other studies they find that changes in malapportionment do affect social welfare policies. The other three selections concerning the determinants of public policy are less sophisticated. Thomas Wolanin and Lawrence Gladieux contend political culture affects U.S. higher education policy, a conclusion marred only by their operational definition of culture as past policy decisions. Lawrence Pierce argues Nixon's decision to use wage and price controls was affected more by political than economic concerns, and Edmund Keller presents an interesting case study of mass participation determining educational policy in Kenya.

Only one paper has its primary stress on the impact of public policy outputs on policy outcomes, Hugh Heclo's study of social welfare policies in England and Sweden. Heclo's analysis reveals, as most students of public policy suspect, that well-designed public policy may not have the impact intended. Jerome Stephen's prescriptive essay on the rights of medical research subjects is the only direct attempt to influence public policy. Stephens uses a journalistic style to show how medical research subjects are inadequately protected. The low point of the volume is two descriptive selections. Craig Worthington attempts to present a framework for analyzing governmental arts policies but ends with two unsatisfying case studies. George Simwinga's chapter on Zambian copper nationalization is based on other analyses with little contribution by the author.

The methodological selections are informative but not major breakthroughs in the study of public policy. Dean Mann rightly criticizes Theodore Lowi's policy typology (distributive, redistributive, regulatory, and constituent) for not being mutually exclusive. Jack Brand underscores the obvious, that social indicators have serious validity problems. Ending the volume with Anthony King's analysis of the role that political scientists can play in policy analysis is especially troublesome. King competently argues that political scientists can play only a minor role in evaluating public policy, a peculiar position to end a book hoping to encourage political scientists to examine public policy.

Because the book's contents are so diverse, it is guaranteed some uses. The volume provides a snapshot of public policy research in the early 1970s; the short time span, however, limits the quality of the articles and renders the volume unsuitable as a class reader. Students interested in the substantive concerns of individual chapters should read those chapters, but only the most rabid student of public policy will read the entire volume.

KENNETH JOHN MEIER

Rice University

The United Kingdom in 1980: The Hudson Report. By The Hudson Institute Europe. (New York: Halsted Press, 1974. Pp. 126. Price not listed.)

This little book represents the Hudson Institute's diagnosis of, and prescription for, what's wrong with Britain (despite the title the United Kingdom dimension is largely absent). The main theme is that Britain is suffering from a serious and unique form of economic sickness. "It is a failure to grow industrially and economically at the same rate at which other neighbouring countries are growing" (p. 3). The result is an "unstable and socially divided nation, economically depressed" (p. 6), with a standard of living and political conditions inferior to other European states. The suggestion is that by 1980 Britain's position will resemble the slow death of seventeenth-century Spain. Three principal arguments are employed to support this theme. First, some specific weaknesses of the economy are identified and examined. These range from low investment rates leading to technical retardation to poor foreign trade performance, a constantly deteriorating balance of payments, and a declining standard of living. Secondly, some popular "myths of salvation"–the Atlantic alliance, European Community membership, and North Sea Oil–are discussed and rejected on the grounds that they represent "a series of major evasions of reality" (p. 70). Thirdly, two policy areas–education (particularly higher education) and the management of regional resources–are singled out for special treatment to illustrate the general point that successive British governments have wasted major assets.

What is to be done to remedy this mess? The Hudson Institute suggests that "Britain once and for all break with its Victorian past, and renew in these closing years of the late twentieth century the process of social and psychological evolution that has been in abeyance for nearly a century" (p. 113). More briefly "a shift in Britain's national style" is required. In policy terms this involves, first of all, a six-year national development plan managed by a planning commission composed of "Britain's best economists and administrators and engineers, all serving the nation in a self-conscious spirit of ambition (for the country) and enterprise" (p. 115). In addition, the Institute suggests a National Investment and Development Corporation to ensure that Britain recaptures "the technological heights" (p. 116), a new national administrative college, control of city investment abroad, and, finally, the abolition of the hereditary peerage–"no doubt trivial in its practical effect" yet "weighed with symbolic importance" (p. 123).

Evaluating all this is a difficult exercise. On the one hand the report, first published in late 1974, was written primarily to stimulate public debate. As a result it has to be viewed more as a piece of quality journalism than a weighty academic study. On the other hand, the Hudson Institute clearly regards itself as a worthy organization with views which should be taken seriously by academics. If this is the game then the exercise as a whole can be criticized on three counts. First, the analysis is characterized by a kind of inverted Almond and Verba-ry. In the past, foreign commentators on British politics and society were too ready to emphasize the good points in the system. The Hudson Institute goes to the other extreme. Other countries, particularly France (and even Italy), it sees as operating in a fundamentally different and superior league to Britain. Few would deny that there is some sort of crisis in Britain. Whether other countries will avoid similar tribulations in the future is open to debate. Britain, not France, may be the developmental model for Western Europe. The Institute ignores this problem. Secondly, there is a credibility gap between the perceived nature of the crisis and the solutions offered. To fight a tiger of a crisis the Institute has put up a few mice. Moreover, the reform proposals are marked by a constant desire to separate economic management from politics, something which, in the present British context, is next to impossible. Finally, it may be that in emphasizing the economic nature of the problem the Institute has mistaken a symptom for the crisis itself. The argument that the British sickness is largely political is never seriously considered. However, it is generally accepted that a variety of possible solutions to our economic problems exist–what is lacking is the political will and capacity to carry them out. The absence of a serious political dimension to the report's analysis is its major weakness. Fortunately other, and less expensive, studies are beginning to fill this gap.

JIM BULPITT

University of Warwick, England

Metropolitan Growth: Public Policy for South and Southeast Asia. Edited by Leo Jakobson and Ved Prakash. (New York: Halsted Press, 1974. Pp. 301. $17.50.)

This volume is the second book by the same editors in the South and Southeast Asian Urban Studies Series. Both volumes are designed to be policy oriented, with this later volume concentrating on management and planning for urban growth. In the short introduction by the editors, the theme of the first essay by Arch Dotson and Henry Teune is endorsed for the book as a whole, supposedly providing a basis for integrating the diverse contributions to the volume. The Dotson and Teune paper, "On the Consequences of Urbanization: Contributions to Administrative Capacity and Development," employs statistical analysis of aggregate data to support the authors' thesis that urbanization contributes to development, and that higher levels of urbanization produce higher levels of development on a wide range of indicators, such as administrative capacity, health, education, industrialization, and communications, among others.

If this is, indeed, the main theme of the book, one would expect the remainder of the contributions to propose policies designed to promote urbanization and a pattern of development modelled after the more advanced industrial countries of the West. While a few contributors make tacit acknowledgment that urbanization may contribute to national development, greater attention is given to counter themes. Several essays, including the final contribution by the editors, emphasize the necessity of retarding the pace of urbanization, accommodating nondeveloped sectors of the economy and preserving the symbiotic relationship between modern and traditional social structures and economic systems.

The contrast between the initial theme and the subsequent themes in this volume points up a critical issue never directly faced by any contributor, but nonetheless lying at the center of the volume. Is the South and Southeast Asian city substantially different from cities elsewhere in the world, such that it requires unique policies and plans to solve its problems? Most of the contributors appear to be answering this question in the affirmative. In the last six papers, which concentrate on Pakistani and Indian experiences with urban planning, the failures of Western-oriented planning and urban development schemes are highlighted. Time and again the message seems to be that policy must fit resources and be adapted to administrative and government capabilities. Because the South and Southeast Asian city, with a few exceptions, has massive overcrowding, overtaxed services, slums and bustees, a dreadfully inadequate tax and revenue base, low administrative capability, and a complex mix of traditional and modern cultures and economic systems, the process of urbanization poses problems which are insoluble by reference to Western models. We learn that slums must be preserved, but made marginally livable, that paralyzed urban services must be incrementally improved, and that priorities should focus on minimal criteria for urban services and infrastructure investments. In short, objectives and priorities must be scaled down, planning should be sensitive to cultural factors, and should be cumulative and flexible, but also long-range and coordinated.

The dilemmas of the cities of South and Southeast Asia are highlighted by the fact that the most modern city in the area is not chosen as an appropriate model for other cities to emulate. There are two contributions on Singapore. The first, by Richard L. Meier, discusses Singapore and Bangkok as "pacemaker" cities for the areas they serve, and presents, in an impressionistic fashion, some evaluations on the positive influences of multinational corporations and the modern business community, along with views on the importance of English as a medium for the transmission of aspects of modern technological civilization. In the second paper on Singapore Peter A. Bush explores the problems of legitimacy and national allegiance in that multiethnic society. Neither paper attempts to generalize for the urban planners of the less developed cities of Southern Asia.

The major contributions of the book are those which recount and analyze the urban planning experiences and strategies of the major cities of South Asia. Among those deserving special mention are the articles by Khalid Shibli on "Metropolitan Planning in Karachi" and by Arthur T. Row, "Metropolitan Problems and Prospects: A Study of Calcutta," along with the final three papers written by the editors. Rather sophisticated and abstract heuristic models are utilized in several of the papers, including one devoted to a complex calculus for evaluation of alternative criteria for investment in water supplies, and two excellent papers which discuss the complex issues associated with formulating appropriate housing and urban development policies. Typological models of the urban economy, the social system, and the budget resource allocation system are utilized to illustrate the complex interrelationship of factors which must be taken into account in urban planning.

Unfortunately, the theory and models utilized in the volume are not always related to the practical choice situations of the Asian urban planner. While there is frequent mention of administrative capabilities as a factor to be considered, corruption is ignored as a major impediment to urban planning strategies. Furthermore, nearly all contributors seem to ignore politics and the process of public participation in plan formation and implementation, apparently assuming that such considerations are irrelevant. For most states in Southern Asia, urban planning must first come to terms with both corruption and politics.

This book will, no doubt, be read with interest by expatriate urban planners who have assisted the major cities of the area with their urban planning problems. However, one cannot help but wonder whether there is an audience for this volume among the indigenous urban civil servants of the medium-sized cities of the region which suffer from unplanned urban growth and have no such expatriate advisory staff. It is more likely that this volume will be utilized as a source book for those urban studies programs in North America which seek to become more cosmopolitan in scope. For such purposes, it is an excellent collection and a most welcome addition to the expanding collection of works devoted to urban problems in Southern Asia.

GORDON P. MEANS

McMaster University

The Devious Dalang: Sukarno and the So-Called Untung-Putsch. Eyewitness report by Bambang S. Widjanarko. Translated and edited by Rahadi Karni and Antonie Dake. (The Hague: Interdoc Publishing House, 1974. Pp. 211. Guilders 17.)

Any documentary material that could shed new light on the controversial question of the true authorship of the October 1, 1965 "coup" in Indonesia, which led to the downfall of President Sukarno, the physical destruction of the huge Indonesian Communist Party, and the installation of the Suharto military regime, is *prima facie* of real importance. This book claims to provide the "verbatim testimony" of one Colonel Bambang S. Widjanarko, a longtime adjutant to Sukarno, in response to questions posed by the Central Interrogation Team of the Operational Command for the Restoration of Security and Order on the subject of the "coup" and its antecedents. An Indonesian-language text is given, along with an English translation and brief introductions by Dake and Karni.

The colonel's testimony had been referred to earlier by Dake in his book *In the Spirit of the Red Banteng* (The Hague: Mouton, 1973), and formed part of the basis for his theory that the real author of the "coup" was Sukarno himself in a complex intrigue to get rid of anticommunist generals who stubbornly blocked the president's leftward course in domestic and foreign affiars. Both Dake's thesis and the evidence for it came under sharp criticism, and the publication under review here is clearly intended to convince the skeptics. Unfortunately, it is likely to have just the opposite effect.

In the first place, there is no way to determine whether the Indonesian text is authentic or has been tampered with by the Indonesian military (who have a long tradition in this regard). The testimony was given *in camera* and to this day has not been published in Indonesia. Though the colonel did testify at two show-trials of "coup" participants, and the transcripts of both have long been available to scholars, the editors have not seen fit to include Widjanarko's public testimony for comparison and cross-checking. (Furthermore, their identification of the defendants at these two trials as Air Marshal Omar Dhani and General Pranoto is disquieting, since the latter has never been tried!)

Secondly, Dake asserts that the colonel was "for many years close to President Sukarno as no-one else," to stress the central importance of his testimony. As any serious student of Indonesian politics knows, this is plain nonsense. The present text itself deflates such claims. About 88 of the 120 questions put to the colonel deal with matters that he would have known about first-hand if he had really been Sukarno's intimate confidant; yet more than half of his answers are based on hearsay. His testimony as a whole gives a characteristic picture of a minor palace functionary hanging about outside closed doors hoping to pick up scraps of high-level gossip. (In addition, less than two-thirds of all his answers have anything at all to do with the "coup" or its background!)

Thirdly, the interrogations were carried out in October-November 1970, *over five years* after the coup. The dating (and the testimony's rapid arrival in Dake's hands) suggests that its production relates less to the events of 1965 than to the internal military politics of 1970, possibly to efforts by the declining Nasution faction to hit back at the winning Shuarto group by intimating a cover-up of Sukarno's true role in the "coup."

But aside from the equivocal status of the text, the largely heresay nature of the evidence it presents, and the political context in which it was prepared, there are also two major technical drawbacks to this book. In the first place, there are no footnotes of any kind to guide the reader through the maze of personalities, places, organizations, and policies mentioned. Only the handful of scholars specializing in the intricacies of Indonesian palace politics under the late "Guided Democracy" can be expected to make head or tail of the information presented or to judge its accuracy and relevance. So many names of persons and organizations are misspelled or simply incorrect historically, that one's confidence in the editors' competence in this field is undermined.

The second drawback is the poor quality of the translation. In some instances the errors may be due to carelessness or ignorance of contemporary colloquial speech in Indonesia. But in other cases, one gets the distinct feeling that the mistranslations are designed to buttress claims that Sukarno authored the "coup" or to put the military interrogators in a good light. Space permits me to offer only one example of each: (1) Sukarno is repeatedly said to have urged people to *menjingkirkan* the anticommunist generals. This word normally means "set aside," "remove from office," but is always translated here as "eliminate" (murder implied). (2) Virtually every question the interrogators pose is introduced in the translation with a gentlemanly "please"—a word completely absent from the brusque Indonesian original. In sum, it is hard to recommend this book except to confirmed "coup" buffs.

BENEDICT R. ANDERSON

Cornell University

Educational Policy-Making: A Study of Interest Groups and Parliament. By Maurice Kogan. (Hamden, Conn.: Linnet Books, 1975. Pp. 262. $15.00.)

Education has excited perhaps more academic interest than any other area of public policy, and Professor Kogan has added another useful study to the existing literature. This study examines educational policy making for England and Wales between 1960 and 1974. The approach is essentially historical and institutional, although some attention is given to sociological and political science concepts as a means of understanding the historical data.

One of the most interesting aspects of this book is the description and analysis of educational policies and objectives. The 35 policies identified by the author were classified as educational, social, institutional, or economic. In addition, the main events in the formulation of the policy and the actors involved were also catalogued. Although the policies discussed rarely fell neatly into one category or another, this classification does help one understand the changes and pressures for change that have occurred in education. It is particularly interesting to note the continuity of several issues in education—teacher supply planning, comprehensivization, and institutional responses to unionization—which are of current importance. In fact, even two years after the end of the author's time period, the only major issue which would have to be added to his list would be the institutional response to expenditure cuts.

The listing of policies and objectives provides an account of the issues in education, while four chapters on interest groups and one chapter on Parliament provide an understanding of the actors involved in making policy. The listing and description of interest groups is especially thorough, with a better analysis of the role of local authority associations in education than is found elsewhere. Likewise, the discussion of teachers' groups goes substantially beyond the mention of the role of the NUT which is usually encountered. Finally, the discussion of the number of new or tangential groups involved in education policy making is a valuable addition to the mapping of actors in education.

Despite the importance of the script (the issues) and the *dramatis personae* to the play, they do not in themselves constitute the play. There is still a need for action, and we unfortunately learn little about the action of policy making from Professor Kogan's book. We do have some insight into the processes of policy making and the interplay of actors from two brief case studies: one on the universities and one on comprehensive schools. However, even these appear oriented more toward laying the groundwork for analysis rather than actually examining how policy has been made. This is perhaps especially disappointing given the numerous interviews conducted by the author, his unique knowledge of the subject matter, and the quality of his previous studies of educational policy making. One can only hope that this will be the prelude to a more complete and exhaustive study of the interactions of the several groups and institutions in the formulation of policy.

Perhaps most disappointing in this regard is the failure to deal more directly with the role of the Department of Education and Science (DES) in making policy. Admittedly, there are difficulties in penetrating a department of British government, but some recent studies—most notably that of Heclo and Wildavsky—have shown that it can be done. While Professor Kogan has defined his study as one of interest groups and Parliament, this rather arbitrary restriction seems to exclude much of what is important in modern policy making. The restriction seems even more arbitrary as it is pointed out that several of the interest groups react more to DES policy that initiate policy on their own. Even those which seek to initiate policy would appear to direct more of their efforts toward DES than toward Parliament. Thus, it seems that the exclusion of DES from more direct and intensive analysis limits the usefulness of this work in understanding the formulation of educational policy in England.

While this is a valuable and thorough study of the role of educational interest groups and of the issues which have been important in education during the postwar period in Britain, its usefulness as a description of educational policy making is limited by the concentration of the groups involved rather than their actions, and by the exclusion of perhaps the crucial actor in the policy process.

B. GUY PETERS

University of Delaware

Community and Change in European Society: France, Germany, and Italy since 1870. By Martin Kolinsky. (New York: St. Martin's Press, 1974. Pp. 234. $16.95.)

This is a brief but intelligent overview of political and social development in France, Germany, and Italy since 1870. The book is divided into three parts. Part I deals with the impact of industrialization in these countries in the years between 1870 and the outbreak of World War I—with "the formation of the modern structures and political cultures of Italy, Germany, and France." The years between the World Wars are covered in Part II, which is concerned primarily with the rise of Fascism in Italy and Germany, with the fascist reorganization of Italian and German society, and with the contrasting (although parallel in some fashion) situation in France. Part III looks at the three countries since 1945 and at the development of the European communities.

Kolinsky, who is a member of the Department of Politics at the University of Birmingham (England), did his doctoral thesis at L.S.E. on French social structure, and this volume illustrates his continuing interest in the relationship between social change and political development. In each period, Kolinsky focuses on the social bases of politics in the three countries; in particular, on the relationship between major social groups—labor and business—and the political process.

This basic approach, rather than an overarching theoretical framework, provides the book's unity. The author does not attempt to open new theoretical frontiers; he touches instead, upon the work of a number of well-known social scientists: Barrington Moore, Ralf Dahrendorf, William Kornhauser, and Talcott Parsons. He introduces basic concepts developed by these authors and attempts to illustrate their use in analyzing social and political development. There is no original research in the book; Kolinsky relies entirely upon secondary sources.

But this should not be taken as a criticism. Criticism should be related to intent, and the author has clearly not designed this book to be a major theoretical contribution, nor does he suggest that it rests upon original research. Thus, it neither is, nor does it pretend to be, a work like the Tillies' recent study of *The Rebellious Century, 1830–1930* or Charles Maier's superb volume on *Recasting Bourgeois Europe,* both of which also appeared in 1975.

Kolinsky's book instead is a highly useful text which would serve well for background reading in a Western European government and politics course. In this light, rather than seen as a major research contribution, it is to be commended as a clearly written history which introduces certain basic social science concepts within an overall framework stressing the interaction of society and politics. For students who take courses in Western European government, particularly—but alas not only—at the introductory level, with little or no background in contemporary European history, this would be a valuable additional text.

Even at this level, the book is not ideal. There is a distinct lack of comparative analysis among the three countries—except when Kolinsky focuses on the social bases of Fascism. In all, given the sweep of events between 1870 and 1970, even in these countries, one might feel that the author devotes too much of the book to his analysis of Fascism. Finally, for what was obviously intended as an introductory text, the bibliography is oddly brief. The book would be more useful if the author had devoted more attention to this.

But my major criticism is not directed at Kolinsky. He has written a book which, if not perfect, still could meet an increasingly important need. As I have attempted to document in a brief volume on *Western European Studies in the United States,* the Western European content of undergraduate education in American colleges and universities is diminishing rapidly. For good reasons or for ill, most of the requirements which formerly structured undergraduate programs and have now been eliminated on many campuses were courses with Western European content: "world (i.e., Western) history and civilization," required language courses, and so on. The international content of U.S. undergraduate education unfortunately has not, it appears, increased, but it has changed in character from Western European to "World." I don't mean to argue with this trend, or to suggest it should be reversed. What it means, however, is that those of us with interests in Western Europe can no longer *take it for granted* that students will acquire a basic background on Western European history and culture just through the normal process of their undergraduate education. Western Europe is increasingly an exotic and esoteric world area to most students, in terms of hard facts and basic academic familiarity.

Kolinsky's book can help fill this need. It could serve as a background text on modern European history in many of our courses for students who have not yet been exposed to these materials. It *might*, if it weren't a $16.95 hardback. This is not a volume that research scholars will purchase for their libraries. It isn't designed to be. But it is one that might be used widely in college courses—if it didn't cost almost $17.00.

STEPHEN BLANK

The Conference Board, Philadelphia

Montaillou, Village Occitan de 1294 à 1324. By Emmanuel LeRoy Ladurie. (Paris, Gallimard [Bibliothèque des Histoires], 1975. Pp. 642.)

It's an ill wind that blows no good. Having picked up LeRoy Ladurie's monograph during a trip to Paris, the chances of reading it were slim until convalescence from pneumonia enforced the necessary leisure. Hopefully, others will not require such compulsion to read *Montaillou, Village Occitan.*

My conclusion is best stated at the outset: this is a *very* important book. Paradoxically, it is valuable for an understanding of the Western tradition precisely because it is devoted to a single village. Although focused on 30 years (1294 to 1324), this study radically alters one's view of the entire medieval period.

For political scientists who are concerned with socioeconomic development, as well as for theorists who wish to judge social theories ranging from Marx to Aristotle, *Montaillou* is fascinating and rewarding. If you read French, you will find all 625 pages well worth the time; if not, keep an eye out for a translation.

Why this strong endorsement? What is the general significance of Montaillou, a village of only 200 to 250 inhabitants on the French slopes of the Pyrenees? And how can an ethnological "village study" be done on a community over 650 years in the past?

The answers are closely related to the efforts of the Church to suppress the Albigensian heresy in Languedoc. The Bishop of Pamiers—an energetic prelate named Jacques Fournier, later to become Pope Benoit XII—received permission to conduct his own Inquisition from 1318 to 1326; a precise prosecutor and "maniac for detail," his scribes faithfully recorded his careful cross-examination of hundreds of suspects and witnesses. In his diocese. Montaillou was clearly the community most seriously infected with Catharism: in 1308, all its inhabitants had been temporarily incarcerated by the Inquisition of Carcassonne; of the 114 people brought before Bishop Fournier's Inquisition between 1318 and 1326, no less than 25 came from this tiny village.

The copious notes of the Bishop's *Register* provide LeRoy Ladurie's source. From his analysis, we learn routine and intimate details: what Montaillonais ate, which gestures and swear words they used, how their houses were built, who seduced whom, and what people did before Western technology gave us the flush toilet. . . . We also see the social structure of the village, based on the household (*domus* in Latin, *ostis* in Occitan)—a unit which was simultaneously the physical house and the nuclear family, often augmented by one or both sets of grandparents and one or two servants.

The first of the lessons of *Montaillou*—and it is probably long overdue—is that the classic image of medieval life did not apply everywhere. In this corner of the Pyrenees, Lord and manor, Chateau and Seigneur were not oppressive nor even central to daily life. Second, and closely related, is the irrelevance of simplistic Marxist categories of social class, ideology, or mode of production, which have all too often dominated discussions of Western history. Third, and no less important, LeRoy Ladurie shows that Montaillou cannot be described in terms of Tönnies' *Gemeinschaft* or "organic community," despite the tendency to apply this concept to all nonmarket or premodern societies.

Religious issues, of course, play a central role in early fourteenth-century Montaillou: the two main axes of life seem to be the *domus* (on earth) and salvation (in heaven). The appeal of Catharism derived primarily from concern with the soul and its fate after death—though the heresy was reinforced by opposition to taxes imposed by the Church. But in Montaillou, the cleavage between Albigensians and orthodox Christians was more than theological since it generally coincided with the division of the village into two main "cliques" or alliances of households.

It is here that students of comparative political processes will find *Montaillou* most fascinating. The book treats patron-client relations—with, as patron, a village curé who adopted heretical views and seduced the widow of the local seigneur, family alliances by marriage or friendship, vendettas, and even an outright assassination. Those who have studied the dynamics of traditional societies, whether in the field or in the anthropological literature, will find themselves on familiar ground when entering medieval Montaillou with LeRoy Ladurie as guide.

Many further delights await the reader. The personalities of the village are sketched in rich detail, notably the libidinous curé, Pierre Clerque, and a delightful shepherd named Pierre Maury. The relations of men and women, the content of local folklore, and the contrast of village life with that of the shepherds in transhumance: there is more than we are accustomed to find in a single volume.

Each reader will, of course, respond to different parts of such a complex ethnographic report. This reviewer found the evidence of a persisting "mediterranean" culture particularly striking, not least because it apparently confirms Aristotle's economic theory (see p. 617).

Indeed, when enjoying LeRoy Ladurie's monumental work, one *almost* forgets the origins of our information about Montaillou. Many of the villagers were condemned to wear yellow crosses as a sign of their heresy; some perished at the stake; more were imprisoned, and their property seized, and homes destroyed.

It may well be that, as the saying goes, "it's an ill wind . . ."; but I cannot help feeling a profound bond of sympathy for the men and women whose lives we can perceive only because Bishop Fournier set out to eradicate the Albigensian heresy in Montaillou.

ROGER D. MASTERS

Dartmouth College

Power and Privilege: Influence and Decision-Making in Pakistan. By Robert LaPorte, Jr. (Berkeley, Calif.: University of California Press, 1975. Pp. xii + 225. $13.75.)

This small book of essentially 183 pages deals with political power in Pakistan from 1947 to 1973. It is based on a review of the pertinent literature and on interviews with approximately 80 American nationals who have had experience in Pakistan. The interviews solicited responses to 78 questions from persons presumed to be knowledgeable about the politics or economics of Pakistan and especially about elite groups, influentials, influence structure, and decision making. Fifty-seven of the 80 interviewees were U.S. officials and 23 were private sector and university or foundation officials.

The result is rather hard going for persons unfamiliar with Pakistan; the author seems to assume some previous background—necessarily so given the time span covered and the length of the book. Yet there is not much in the book that the Pakistan specialist will not already know. However, the book is useful for those readers who fall between these two groups. Although no individual or institution in Pakistan is treated in depth, the reader obtains a perspective on the influential groups in the country and the shifts that have taken place in the last decade.

The first two of the book's eight chapters deal with concepts, research problems, and a general history of Pakistan prior to 1971. The next four chapters focus on elites and decision making during specific time periods. The Pre-Ayub period (1947–1958) is described basically as bureaucratic rule behind a parliamentary facade. Ayub's "modernizing" rule is viewed primarily as an attempt to maintain civil and military support and to a lesser extent support of nongovernment influentials such as important industrialists. The Yahya period is characterized as a junta rule so isolated from political reality that civil war and the separation of East Pakistan was hastened.

The most interesting and longest chapter opens with a good summation of the situation as of December 1971 and continues into 1973. These years saw a decline in the influence of the Civil Service of Pakistan (CSP). Formerly the most powerful and prestigious segment of Pakistan's civil service system, it has been disestablished as a distinct and separate class. Other groups also lost influence: the higher military officers and the leading industrial families in particular. The middle classes rose in influence as they participated in national and provincial politics. Two groups—the peasants and industrial labor—failed to make appreciable gains as Bhutto did not deliver on his electioneering promises. The landless peasant "is still waiting for his twelve acres" (p. 139).

Students supported Bhutto but do not seem to have gained influence by doing so.

LaPorte's seventh chapter is quite different. Here he uses his interview results to consider the influence of the United States on decision making in Pakistan—concluding that there has been an influence "but not a dominance" (p. 147). This chapter is primarily an attempt to tap the opinions of Americans who have worked in significant posts in Pakistan. However, Appendix B indicates, among other things, that these Americans had surprisingly little contact with labor unions, students, landlords, and peasants (p. 201). One gets the impression that the author was interviewing American bureaucrats whose contacts—almost exclusively—were with Pakistani bureaucrats in urban areas.

This must have been a frustrating book to write. Most of the written materials on Pakistan predated 1971. The years of 1970 to 1973 were years of rapid change, and at this point it is extremely difficult to determine which changes were basic and important and which temporary. The author may have attempted the impossible; he must be commended for his courage. His subject is an important one; given all the methodological difficulties—which he admits to in a forthright fashion—he has done very well. There is something of interest here for any social scientist who has a real concern for Pakistan.

HENRY F. GOODNOW

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Psychological Challenges to Modernization: Emotional Factors in International Development. By Albert Lauterbach. (Amsterdam and New York: Elsevier Scientific Publishing Co., 1974. Pp. 190. $10.75.)

The core of this book is a challenge to our traditional views of development and ways of looking at what is going on in the modernizing nations. Lauterbach argues that a developing nation is not simply remaking itself (or being remade) in the image of nineteenth-century Britain, or the U.S.A., or Russia, or even China. In fact most of our familiar and cherished categories for talking about economic systems (such as private versus public ownership) are held to be inapplicable and irrelevant to such a nation. That nation's social movements will have little relation to Western movements with similar names. The attitudes of its businessmen about the family and time will be different. Both the perception of uncertainty or risk and the actual factors that make for uncertainty and risk will be different from what we are used to. For all these reasons, Lauterbach argues, we need to change our conceptual apparatus. Modernization is a distinct sociocultural system; it is not a transition from "feudalism" to "capitalism," or from "capitalism" to "socialism." Modernization is not just something that happens to the economy, for it also implies "change in cultural attitudes, administrative procedures, and power relations among social groups" (p. 160). Because Americans have a mental block against understanding themselves, it is argued, they are peculiarly unable to understand a pluralistic world except by imposing their own conceptual categories. From these misconceptions have come the suspicion, resentment, and outright failure associated with foreign "aid."

Stated in these terms, Lauterbach's thesis is not unreasonable, although it is not exactly new either. Given the author's considerable professional experience in the modernizing world, the reader would hope that in this book these ideas would receive vivid and rigorous documentation or exciting theoretical illumination. Unfortunately, I don't think that they do. Lauterbach's alternative definition of modernization is one of those notions that keep slipping away into the conceptual distance: "It must be defined in terms of exploring *their* [a people's] *own* socio-economic potential, both human and material, in line with their prevailing value system and attitudes. . ." (p. 143). I am not able to think of *any* activity in any country I know that could be excluded from such a rubric. Support for the general thesis comes largely from anecdotes that are too brief and abstract to be useful as case histories, and from bits and pieces of psychological theory. I find it incredible that a book of this title could be written in 1974 without mentioning the names (or ideas, or findings) of Hagen, Eisenstadt, Inkeles, Lerner, McClelland, Parsons, and Pye, among others. Perhaps due to this lack, the book abounds with assertions and recommendations that are vague, simplistic, and dubious: "Fascism and communism are different in their origins but similar in their results" (p. 10); "The psychological effects of a woman's work on her children are of considerable importance" (p. 118); "The modernizing nations should not assume that the wealthier ones *owe* them economic, social, or other improvements; and the wealthier powers should not become impatient if the recipients either hesitate or are unable to imitate successfully their experiences" (p. 147).

Lauterbach's own concept of choice in the analysis of modernization—freedom versus totalitarianism—leads to the most troublesome assertion: "Nobody has yet presented a totalitarian system in a modernizing country (or in an industrialized nation, for that matter) that has been more efficient and honest, especially in the long run, than are the admittedly slow procedures of many free societies" (p. 141). That last "many" makes it difficult to decide what this statement could mean; but in any case no evidence is presented to support it. Surely the comparison of post-1950 China with pre-1975 India cannot be made in such simple terms.

Chapter 6, "Society, Mental Health, and Developmental Stages," stands apart from the rest of the book. The author lays out structural and dynamic economic factors that are likely to have effects on psychological well-being, but the focus is mainly on developed countries. Here the discussion is provocative, and some of the hypotheses about debilitating effects of unemployment and economic down-turn have been confirmed by Brenner's recent research (*Mental Illness and the Economy*, 1973).

Anthropologists and sensitive world travelers have always known that each nation is unique and subtly different from each other nation; that in some ways cultures are all of a piece, while the concepts of economics, politics, and the other social sciences were dimensions that were abstracted and isolated from the rich particular settings of life. Ever since the bloom went off the rose of Point Four in the 1950s, sensitive economists and students of international development have sought illumination from anthropology, sociology, and psychology. Not all social scientists have responded with a commitment to look beyond their own national borders; and most politicians have ignored the whole effort. Yet there *have* been substantial fruits of this interdisciplinary ferment in the works of those mentioned above and others. What this book seems to show is how far we have yet to go toward a rigorous and coherent diffusion of this research, let alone to an understanding of social change and modernization.

DAVID G. WINTER

Wesleyan University

Personal Freedom and the Law in Tanzania: A Study of Socialist State Administration. By Robert Martin. (Nairobi: Oxford University Press, 1974. Pp. xvii + 224. $7.25.)

Robert Martin's *Personal Freedom and the Law in Tanzania* is a book of such poor intellectual quality that it discredits the scholarly reputation of Oxford University Press. Any prospective reader should be cautioned from the outset that it is not, in fact, a book in the standard sense. *Personal Freedom* can best be likened to an anthology since it consists overwhelmingly of a series of extended excerpts from political speeches, official statutes, court cases, government documents, radical texts, and secondary materials on East African politics. Martin's own contribution, which consists essentially of a series of brief "notes" at the beginning or end of each chapter, is, by his own acknowledgment, only supplemental and is intended to furnish a general frame of reference within which the various excerpted materials can be understood. The result of this unusual writing device is that the author's ideas are nowhere fully developed or elaborated and must be pieced together from his intermittent commentary.

Martin's decision to entitle his book *Personal Freedom and the Law in Tanzania* is also misleading since the book is not about personal freedom in Tanzania, but about its absence. The contents demonstrate, if anything, that Tanzanian law consistently sacrifices personal rights and freedoms to the executive power of the state. Indeed, Martin's purpose is to legitimize that policy by rejecting the liberal-democratic tradition which esteems personal liberties as inappropriate to the developmental needs of a poor socialist state. The concept of personal freedom, Martin asserts, was concocted in Western capitalist societies where it has served historically to protect the economic interests of the propertied classes. This has resulted, so the argument runs, in great socio-economic inequalities and it is absurd, in Martin's view, even to speak of personal freedom so long as social inequality exists.

Martin repeatedly juxtaposes the concept of individual or personal rights against the notion of rights for a greater collective entity known variously as "the masses" or "the people." The purpose of a socialist state, as he sees it, is "to guarantee the economic, political and cultural rights of the mass of the people" (see *Note*, p. 5). In order to achieve this purpose, the socialist state must equip its executive branch with unrestricted latitude to repress individuals and groups. Martin's textual commentary shows not the slightest qualm or misgiving about the elaboration of a doctrine which, if taken at face value by Tanzania's leaders, would encourage the grossest forms of political abuse in the name of collective justice. So far as this anthologist is concerned, any preoccupation with the protection of the individual against the state (such, one may suppose, as that in this review) is merely evidence of a bourgeois mentality.

The illogic in this position is so glaring that to identify it is merely to call attention to the obvious. Even if one were to grant Martin's simplistic view of the role of law in the West, it does not follow that there is no place for a concern with protection of the individual under socialism. Socialist states are no less capable of grotesque acts of political repression than capitalist ones. Yet Martin mindlessly catalogues Tanzania's truly awesome legalization of executive power and the absence of court restraints on that power as evidence that collective freedom is being upheld (see pp. 82–132). It is to the credit of the Tanzanian executive branch, and a mute official comment on the seriousness with which Martin's ideas are taken, that the government of Tanzania has, for the most part, proven reluctant to utilize the vast arbitrary powers at its disposal.

Martin's book does have one utility. It should serve as an object lesson to radical scholars engaged in the analysis of socialist societies. A critical perspective toward capitalism does not entail the abandonment of political judgment when noncapitalist societies are at issue. The heart of radical analysis is a general skepticism toward state power and its use; an analysis which obsequiously seeks to legitimize the unbridled use of state power by socialist leaders misses the entire point of the radical tradition.

MICHAEL F. LOFCHIE

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Resolutions and Decisions of the Communist Party of the Soviet Union. General Editor Robert H. McNeal. 4 Volumes. Volume I: *The Russian Social Democratic Labour Party: 1898–1917.* Edited by Ralph Carter Elwood. Pp. 306. Volume II: *The Early Soviet Period: 1917–1929.* Edited by Richard Gregor. Pp. 382. Volume III: *The Stalin Years: 1929–1953.* Edited by Robert H. McNeal. Pp. 280. Volume IV: *The Khrushchev Years: 1953–1964.* Edited by Grey Hodnett. Pp. 328. (Toronto, Canada: University of Toronto Press, 1974. Pp. 1296, inclusive. $75.00, set of four.)

The basis of this major new reference work is the authoritarian character of the Leninist Communist party. Decisions adopted in the name of the party's Central Committee (as well as decisions adopted by periodic congresses or conferences) are binding on the whole party. Whoever is able to speak with the authority of the Central Committee—whether that authority is derived from the Central Committee's plenary meeting, its political bureau, or its secretariat—can command the obedience of the entire party. When confronted with this truly charismatic authority—the ultimate source of value—even so formidable a figure as Leon Trotsky felt compelled to disavow his own reasoned judgment. Moreover, since the party commands the Soviet state, the goal of politics in the USSR, as in all Communist states, is to speak in the name of the Central Committee.

The editors, established scholars at Canadian universities, have collected the most important pronouncements of the Communist party of the Soviet Union, most of them readily available in various Russian language collections published in Moscow. The party's history falls naturally into four periods: from its beginning to the Revolution; the early Soviet period; the Stalin years; the post-Stalin period to the ouster of Khrushchev. One volume has been devoted to each period, with a single editor responsible for introducing the volume and providing the necessary headnotes. Since the individual editors were given considerable latitude, each volume has its own character. This has its advantages, but firmer guidance by the general editor might have eliminated distracting idiosyncracies, particularly in the somewhat inflated second volume. By supplementing the sources previously available in English for the study of Soviet domestic policy and politics (such as the collected works of Lenin and Stalin), the present collection will be helpful to political scientists generally, and particularly to students of comparative Communism who have specialized on regimes other than the Soviet one. Of course, very large gaps remain. There is no published collection in English of state documents for the entire Soviet period and many key articles and speeches also have not been translated, although these deficiencies are less serious for the last three decades because of the availability of the *Current Digest of the Soviet Press.* The present collection of party documents ends with the designation of Leonid Brezhnev as first secretary in 1964; a fifth volume is planned for the Brezhnev period.

The translations on the whole are well done, and the selection of documents is sensible, granting the element of arbitrariness that is inherent in the task. A few of the editors' decisions, however, are troublesome. One-third of the volume on the post-Stalin period is devoted to the party's 1961 program, even though it is readily available in English in numerous Soviet and foreign editions. Partly as a result, such major developments as Khrushchev's decisive victory over Georgi Malenkov at the Central Committee meeting in January 1955 and the inconclusive but revealing struggle between planners and economic administrators at the session in December 1956 are not represented. The apparatus provided is modest. The indexes, which appear separately for each volume, have not been coordinated and are not very helpful (for example, *faction*—one of the key terms of Soviet politics—appears in the index of only one of the four volumes). On the other hand, there is a uniform and highly useful appendix displaying membership in the Central Committee's executive bodies (political bureau/presidium, organization bureau, and secretariat) since their beginnings in 1917.

The editors' introductions and headnotes provide helpful background materials. The serious reader should consult as well the standard works on Soviet history by W. H. Chamberlin, E. H. Carr, Leonard Schapiro, and others. Nevertheless, because of tight Soviet secrecy about higher politics, readers may find that authorities not infrequently disagree, even on crucial questions. The interpretive judgments of the editors of this collection for the most part reflect what consensus exists among specialists. There are exceptions, however. Most observers, I believe, would question the view that the convocation of the Nineteenth Party Congress in 1952 was brought about by "a coalition of Stalin's lieutenants" as "part of a growing rift between Stalin and his Politburo" (III:17). At times, an editor's concise account conveys a misleading impression of how Soviet politics is conducted. From the headnote on the October 1964 plenum of the Central Committee, for example, it might appear that the process by which Brezhnev replaced Khrushchev resembles the one by which unsuccessful prime ministers are replaced by their party colleagues in a parliamentary system (IV:316). (Khrushchev's victory over his enemies in June 1957, however, is thought probably to have required the aid of the army and political police—IV:92.)

The editors are competent guides, but nonspecialists should be warned that in interpreting these documents they are operating on treacherous terrain. Nevertheless, political scientists who do not read Russian should consult this collection, which might help them understand better the obscure politics of the expanding second world.

MYRON RUSH

Cornell University

Consociational Democracy: Political Accommodation in Segmented Societies. Edited by Kenneth D. McRae. (Toronto, Canada: McClelland and Stewart Limited, 1974. Pp. 311. $4.95, paper.)

Professor McRae has undertaken the task of observing political systems that emphasize the continuous adjustment of sharply diversified and articulated social, political, and economic interests. The volume is divided into four parts: (1) a description of various approaches to "consociational democracy," including a brief explication of the multiple terminologies that have emerged from this literature; (2) a more indepth analysis of the theoretical perspectives of "consociationalism" that includes, among others, previously published studies by Val R. Lorwin, Arend Lijphart, and Gerhard Lehmbruch; (3) applications and illustrations of "consociationalism" in the Netherlands, Austria, Belgium, and Switzerland; (4) an illustration of various aspects of "consociationalism" in Canada concluding with an epilogue.

McRae's introductory statement seeks to present a broad yet fairly integrated framework for the analysis of social and political behavior in states that have substantial cleavages in social structure arising from socioeconomic, ideological, and religious differences. His review of the consociational literature presents no new insights but is an excellent survey of the basic viewpoints that emerge from the existing literature. While McRae argues that his concern "... is not with smaller European democracies in general, but with certain patterns of social structure and political behavior that have been exemplified most sharply in the Netherlands, Belgium, Austria, and Switzerland" (p. 3), the only other application of consociationalism he provides that does not deal with these four states is the section dealing with Canada. One would like to have seen an additional section of the volume dealing with applications and analysis of states that did not emerge from a Western European social and political tradition. While McRae may be correct in his belief that the smaller European segmented societies may offer valuable insights and prescriptions for other states facing similar difficulties, the evidence has yet to be presented. This limitation aside, McRae is to be congratulated for developing a theoretical framework for the volume that knits the various essays into a meaningful whole.

The theoretical essays by Lorwin, Lijphart, Lehmbruch, Steiner, and Daadler each examine and illustrate the various dimensions of consociationalism: (1) Lorwin analyzes the patterns of social structure in the society and concludes that consociationalism "works" according to the degree of segmentation found in the structure; (2) Lijphart and Lehmbruch take the perspective of elite interactions and contend that the success of consociationalism emerges from patterns of elite behavior; (3) Daadler explores the argument that consociationalism results from historical political traditions (i.e., political culture). Each of these essays, particularly the ones by Lorwin and Lijphart, are well known, and represent provocative social science analysis.

The major portion of this volume is a series of case materials. The treatment of each state is somewhat different and includes a range of presentations from simple description to more sophisticated analysis of party and voting data. McRae's introduction to these case materials is brief, and as a result the reader who is unfamiliar with these "classic examples" of consociationalism might find additional readings necessary before any meaningful evaluation of the case materials can be made.

On the whole, McRae has assembled an interesting and at times provocative selection of essays on some of the most important issues confronting many developing and developed states. The volume is a useful vehicle in introducing important theoretical and comparative issues in today's integration literature. A volume such as this has a valuable place in almost any graduate division course on comparative theory and analysis. Its modest goals and the excellence with which it reaches them is unusual for an edited volume of previously published research.

ALBERT F. ELDRIDGE

Duke University

Mapai in Israel: Political Organization and Government in a New Society. By Peter Y. Medding. (New York: Cambridge University Press, 1972. Pp. 326. $16.50.)

Unlike most other party systems in Western democracies, the Israeli party system is neither a two-party nor a multiparty system; it is what Maurice Duverger calls a dominant party system. A party is dominant when it is larger than any other party and outdistances its rivals over a certain period of time.

The problem to which Medding addresses himself in this book is not how Mapai became the dominant party in Israel but rather how it has maintained this position since the establishment of the state. Duverger attributes the domination of a party primarily to cultural and ideological factors. "A party is dominant," he explains, "when it is identified with an epoch; when its doctrines, ideas, methods, its style, so to speak, coincide with those of an epoch.... Domination is a question of influence rather than of strength; it is also linked with belief. A dominant party is that which public opinion believes to be dominant" (*Political Parties,* 1954, p. 308). This is not Medding's approach to the problem. He rarely mentions party ideology or its identity with the value system of society, but rather attributes Mapai's dominance to its success in establishing close ties with a large number of economic, social, and ethnic organizations.

The main part of the book describes and analyzes how the party, by using its control over the government and the Histadrut (a peculiarly Israeli institution, controlling most of the country's trade unions and agricultural settlements and owning more than 10 percent of the country's industries), satisfies the material and ideological needs of these organizations in exchange for their continuous support during elections. Bargaining between the party leaders and the different organizations, so Medding says, is primarily conducted within the formal and informal party bodies in which the organizations are represented.

However, I would like to question one of Medding's conclusions: his insistence that the bargaining going on continuously among the different interest groups within the party bodies results in what the author calls "a consensual power relation." The different groups within the party, he claims, reach a consensus which consequently becomes party policy implemented by the party leaders in the government and the Histadrut. Medding believes that this process disproves Robert Michels' famous theory of party organizations which claimed that the power of decision making in political parties was left to a handful of leaders ruling the party as an oligarchy.

Medding devotes a lengthy chapter to the operation of the party machine, its control of the party organization, and its success in denying its rivals both control and influence. The machine tries to satisfy the maximum number of interest groups within the party and outside it; it acts as a broker between the leaders in control of the government and the various organized interests in society. But can this process be described as consensual? Who is actually in control? On the basis of Medding's evidence it would seem that the power to make decisions is in the hands of the central leaders representing the party in the government and of the heads of the party machine. However, he maintains, there is a division of labor between the two groups: some spheres are reserved for representatives of the party in the government while others are left to the heads of the party machine.

Medding's analysis sheds new light on the political process in Israel and provides a convincing description and explanation of the ways in which Mapai has maintained its dominant position in the Israeli political structure. It also describes the modes of operation of a dominant party and the relations between such a party and the interest groups which operate both inside and outside its organization. In my opinion, Medding's study of Israel's dominant party is important to an understanding of Israeli politics and is both well argued and amply documented.

YONATHAN SHAPIRO

Tel-Aviv University

The North Korean Communist Leadership, 1945–1965: A Study of Factionalism and Political Consolidation. By Koon Woo Nam. (University, Alabama: University of Alabama Press, 1974. Pp. 214. $7.50.)

At the beginning of the present decade, U.S. "China watchers" were in reality largely "China listeners" committed to close examination of media transcripts and interviews with defectors. This difficulty has since been tempered by increasing access to the mainland for direct observation and meetings, which has led to better, although certainly still carefully limited, information. Unfortunately, students of North Korea in the mid-1970s are still "Korea listeners."

The present volume, which briefly traces the shifting coalitions within the Korean Workers' Party (KWP) from 1945 to 1966, unavoidably meets the "listener" criteria. Some ten pages are devoted to the pre-1945 origins of the party, and a similar space is given to the 1966–1970 period; the bulk of the volume deals with 1948–1956. Professor Nam's study concerns itself almost completely with a small political elite fiercely competing for power. Moreover, its brevity forces the sacrifice of a detailed analysis of complexities. As an easily-read chronology and name list it serves well, but because it takes little account of both social and economic factors, the picture it presents often overlooks the subtleties. This approach is different from other succinct and important works such as *Communist Politics in North Korea* by Ilpyong J. Kim

The 100 pages which form the body of the book sketch the struggles among the leadership groups that had been separately formed in Korea, Manchuria, China, and Siberia. Professor Nam cogently reasons that many of the causes for intraleadership cleavages can be found in long-standing factional friendships and political orientations. These three main clusters were made up of those returning from—and supported by—the two adjacent countries' Communist parties (Kim Il Sung's group was a subdivision of the "Soviet faction," and those who had stayed in Korea during the harsh Japanese occupation were the "domestic faction").

The story of Kim's rise to domination in the party with Soviet support is well chronicled. The Korean Civil War encouraged some opposition to his rule, while the presence of the Chinese People's Volunteers gave hope to the China-returnees (the "Yenan faction"). The limitations of Soviet material support to Korea during the war might have encouraged opposition to the Soviet faction. In fact, a leader of that faction was purged in 1951, but he also had been an adversary of Kim's. China did not bolster Kim's opponents within the party, perhaps in part because Kim had enjoyed fraternal relations with the Chinese Communist party in the 1930s. In sum, the book points out that it would not be satisfactory to say only that a Korean leader was supported by either Peking or Moscow. Kim Il Sung's successes were largely built upon the state's encouragement of pride in its nationalism.

The leader of the domestic faction, whose base of support had been in the South, allegedly opposed a conclusion of the war before the South had been taken. He was removed from authority in early 1953; the truce followed. It should be added that both of these intra-KWP conflicts are cautiously and masterfully dealt with in the comprehensive and choice major study by Robert A. Scalapino and Chong-Sik Lee, *Communism in Korea.*

Kim emerged from the war as a nationalist hero and continued to strengthen his position in the KWP. This included an intensified rejection of the adoption of either a Chinese or a Soviet "model." Kim declared in December 1955:

> The important thing is to master revolutionary truth. Marxist-Leninist truth, and to apply it to the actual conditions of Korea. We cannot have an imperative principle of doing [everything] just as the Soviet Union does. Although certain people say that the Soviet way is best or that the Chinese way is best, have we not now

> reached the point we can construct our own way? (p. 106)

Khrushchev's February 1956 de-Stalinization speech, in concert with Kim's tightening control over the party, prompted the Yenan and Soviet factions to join together in order to oust Kim from power while he was abroad in mid-1956. Kim returned and reversed his rivals' plan. Since then, Professor Nam relates, the leadership has been solely Kim's.

It should be noted that the study lacks any theoretical perspective (e.g., what is "leadership"?). The book draws upon an impressive bibliography in providing a well-written sketch of the political leadership. Yet beginning here, a student of North Korea would want to read further about the complexities of both the system and its social-economic structure, as well as some parallels to roughly similar situations.

ROBERT R. SIMMONS

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Violence and Crime in India: A Quantitative Study. By Baldev Raj Nayar. (Delhi: Macmillan Co. of India, 1975. Pp. 150. Rs 40.00.)

Many Westerners think of India as a nonviolent nation, following in the pacifist tradition established by Gandhi. As pleasant as this image is, it is, unfortunately, false. Although India certainly is not an unusually violent nation, it also is not an unusually nonviolent society. Indeed, some forms of violence and crime, such as those types associated with Rummel's turmoil and revolutionary dimensions, occur quite frequently in India. It is important, therefore, that the comparative analysis of the political and economic bases for crime and violence include rigorous studies of these events in India. Baldev Raj Nayar has made an able step toward providing students of crime and violence with insights into the appropriateness of their empirical and theoretical generalizations for India.

Professor Nayar has tested two hypotheses, derived from the literature concerned with relative deprivation. The first states that long-term trends in crime are inversely related to both national economic and political capabilities, while the second hypothesis argues that deviations from this inverse association are a function of countervailing tendencies arising either from political or economic capabilities. Overwhelming empirical support is found for these hypotheses. One must, however, be very cautious in inferring any causal relationships from the evidence provided in *Violence and Crime in India.* Professor Nayar states that "violence and crime are not things that happen at random but, as the record of the post independence period in India compellingly demonstrates, are causally related to system performance in politics and economics" (p. 72). The problem with this conclusion is that crimes are committed by individuals, and those individual criminals may or may not have personally experienced any effect from shifting national capabilities. The ecological nature of the data analysis does not permit inferences about the causes of individual criminal activities. The fact that in the aggregate these activities are correlated with gross shifts in economic and political capabilities in the nation cannot be taken to mean that individual experiences with shifting economic and political capabilities produce the same association. Having said that, and therefore having raised serious doubts about the causal inferences drawn from Professor Nayar's analyses, there is still much in this book that makes it an important contribution to the study of Indian politics.

Professor Nayar has convincingly demonstrated that most important categories of crime in India have followed clear and empirically interesting national trends. This result is important because it provides some basis for concluding that the Indian polity is sufficiently integrated so that national policies do, at the aggregate level, have uniform effects throughout India. The reader is left convinced that those who would argue, as D. H. Bayley has, that "very rarely indeed can evidence be found to support a contention that among most states at any given instant an all India factor has been operative" (p. 133) are wrong. The evidence for this conclusion, and its implications for policy analysis in India, are sufficient reason by themselves for reading *Violence and Crime in India.*

Beyond the discovery of clear national trends, Professor Nayar has somewhat disaggregated the crime data and revealed interesting state and regional factors associated with various types of crime. The most interesting and politically significant of these findings is the substantially greater frequency of politically meaningful crimes, such as riots, in the eastern states of India. The presence of group crimes, such as riots and dacoity, *suggests* the possibility of motivations other than greed or momentary passion causing violent behavior. To the extent that these crimes committed by groups are more highly coordinated and socially or politically motivated, the discovery of regional components to these types of crime raises important questions about differences in social awareness, political integration, and channels of opposition in different parts of India. Professor Nayar's study of crime and violence in India provides an important and informative first step toward investigating those questions.

BRUCE BUENO DE MESQUITA

University of Rochester

Parlamentarische Opposition: Ein Internationaler Vergleich. Edited by Heinrich Oberreuter. (Hamburg: Hoffman & Campe, 1975. Pp. 293. DM. 32.)

Popular mistrust of political parties in Western democracies has increased in recent years. Yet shifts in attitudes have not significantly changed the pattern of party systems in which parties in opposition are competing with those in power for voter support. This study of parties in opposition may be compared in scope to that edited by Robert A. Dahl, *Political Oppositions in Western Democracies,* published in 1966. Once again only the Western democracies are selected for analysis in order to pinpoint the role opposition parties play in them. If no important political developments had taken place in the eight countries (Great Britain, United States, France, Italy, Austria, Sweden, Norway, and West Germany) covered in both volumes then there would be little reason for the Oberreuter study. But the editor, on the staff of the political science institute at the University of Munich, was cognizant of the political changes that had taken place in the United States and Western Europe in the last decade. What we have essentially is a useful up-date of the Dahl volume, with the inclusion of Switzerland and Denmark which Dahl did not cover, but the exclusion of Belgium and the Netherlands which he did cover. All chapters, except one, represent original pieces written for this volume.

In a brief introduction, Oberreuter deals with the tolerance, legitimization, and institutionalization of political oppositions in democratic polities with strongly entrenched welfare systems. He notes that, since the opposition in each political system has its own individual profile, a comparative theoretical approach is difficult, especially if the institutional variable, sharply different from one country to another, is taken into consideration. He is critical of Dahl's comparison of hegemonic or mixed political systems with polyarchies (in *Regimes and Oppositions,* 1973) because in the first two systems the people cannot criticize or loyally oppose their regime fully. Hence, he argues that the term "opposition" must be restricted to primarily cooperative and competitive opposition among political parties in democratic regimes if it is to have any operational meaning. By narrowing his definition he must exclude, for instance, revolutionary groups intent on destroying the constitutional system. He admits that even his narrow concept of opposition-produces definitional difficulties given the various types of governmental organizations, the structure of parties, and the effects of different electoral systems in the democratic polities. Thus, the contributors to the volume received the task of working up their own model of opposition in the country or, in one instance the countries, which they studied. Since Oberreuter fails to sum up similarities and differences which emerge in the country chapters, a new theoretical construct or conclusion is missing in this volume.

Yet there is a good deal of payoff, especially on recent developments, in the individual contributions. Symbolically, the chapter on Great Britain by Nevil Johnson leads off the parade of countries. In a country still proud to grant the loyal opposition maximum freedom to criticize Her Majesty's government, Johnson notes changes on the horizon. A "democratization" of the concept of opposition has taken place. In a number of recent elections, the voters, rather than Parliament, have exerted control over the government. To influence the voters, the opposition party, unable to change government policy in Parliament, must constantly present nondogmatic alternative solutions in order to become a potential creditable government party. But often the alternatives are based on tactical rather than substantive policy differences—true in other countries as well.

George Romoser deals with "political opposition between consensus and instability" in the United States. He emphasizes the delicate balance that various administrations have had to maintain with Congress regardless of party majorities. While radical politics has ebbed, decentralized community-based groups clamoring for social change have added a new dimension to the American political scene.

Roy Macridis dissects the plethora of oppositions in France (in a reprint from *Political Opposition and Dissent,* Barbara McLennan, editor). While the chapter is dated—the socialist-communist united front was not yet in existence—the author did predict a reduction in the number of parties leading to a more simple division in Parliament and the electorate between the government and the opposition.

Silvano Tosi's chapter on Italy assumes a topical importance. He foresaw the recent governmental crises and the role the Communist party has played as a loyal opposition party ready to share in the national decision-making process. He notes that the old classical dialectic between government and Parliament has surfaced again, even though the parliamentary control function has suffered. Should a Christian Democratic-Communist government be formed some day, a powerful opposition would cease to exist.

Switzerland represents and Austria represented a coalition government model without significant party opposition. Yet in Switzerland, according to Jurg Steiner, the possibilities for parties in power being critical of specific pieces of legislation at the federal and cantonal levels are numerous. For economic interest groups, small opposition parties, and the average citizen, the initiative and the referendum provide modest means to criticize the government.

In Austria, according to Manfried Welan, economic associations also formed one source of opposition, until the breakup of the government coalition of the two major parties in 1966. While the parties did not bury their differences within the coalition, the role of Parliament atrophied. Even since 1966, the major opposition party has assailed the government with little effectiveness; this is not too surprising since about 80 percent of legislation is approved by both parties—a typical phenomenon of an emergent consensus on a host of government policies.

Nils Sternquist notes that in Denmark, Sweden, and Norway the position of the Social Democrats and other established parties has been weakened in recent years. Unable to gain an electoral majority, the Social Democrats had to form coalitions with either more radical Left parties or with bourgeois parties whenever necessary. As a consequence, programmatic and ideological differences have become more invisible, although new schisms between central and peripheral authorities, and between urban and rural areas have emerged.

Three authors (Dieter Grosser, Manfred Friedrich, and the editor) deal with the Federal Republic of Germany. They concur that the role of the opposition differs from that of Great Britain. Heirs to the Weimar system in which the opposition was split between parties loyal and disloyal to the constitution, the loyal opposition party in Bonn must confront an attitudinal mistrust of its so-called negative role and must confront a powerful executive and bureaucracy. As with many other opposition parties described in this volume, the arena of competition is a parliament in which the governing parties have the upper hand, and in which the opposition party or parties shift their tactics from competition to cooperation.

Despite the cited shortcomings of this study, it offers a valuable survey of opposition parties struggling to gain power in countries with an entrenched welfare state system. While all parties pay lip service to the welfare system, tactical and ideological schisms between them have not evaporated; they make for some lively political contests.

GERARD BRAUNTHAL

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Zambia: Security and Conflict. By Jan Pettman. (New York: St. Martin's Press, 1974. Pp. 284. $15.95.)

In January of 1976, in the midst of persistent war in neighboring Angola among competing liberation movements supported by external powers, President Kaunda of Zambia instituted a series of emergency measures designed, he announced, to preserve the security and perhaps the very existence of the Zambian state. Though surely anguished by the turn of events, Jan Pettman must take some satisfaction from the accuracy of her prefatory statement that "Zambia's national existence is precarious" (p. 7). Here is a book which seems to provide students of African politics significant insight into the current situation. Unfortunately, however, Pettman's preference for description rather than analysis reduces the value of her work in enabling observers to sort out why the emergency measures were taken, how those measures relate to the cluster of challenges Kaunda faces, and what resolutions of the apparent crisis are likely, or even possible.

Pettman's goal is the description of Zambia's insecurity. In separate chapters on Zambia's colonial heritage, polity, society, economy, and external relationships, she charts the best-known constellations in Zambia's political universe. Concerned with control, she focuses on challenges to Zambia's leadership and party. Her conclusion is that in the period since Zambia's independence in 1964 "threats to Zambia's security did not lessen . . . and Zambia's capacity to cope with them did not increase" (p. 235). The organization of her inquiry does not lead her to explore why.

The major contribution of this book is its survey of personalities and events. Drawn from a wide array of sources, her information is for the most part accurate, though occasional striking errors (like her claim, p. 131, that primary education in Zambia is now "almost universal") both mar the survey and reduce confidence in other reported information. Having relied heavily on what prominent Zambians have said, she rarely differentiates between claims made for specific political purposes and accurate assessments of events. As a result, there are substantial inconsistencies in her report (for example, her assertion, p. 115, that "the Zambian government is committed to a policy of rapid social and economic change," and her conclusion 20 pages later than "the majority of Zambians in rural areas were completely unaffected by development"). As well, she includes observations from other authors, with little effort to assess them critically or to sort out their contradictions. Her efforts at conceptual clarity are further undermined by her proclivity for ambiguous categories like "top civil servants," "leading members of the government," "personal advisers," and so on.

The major weakness of this book lies in Pettman's preference for description over analysis and in her failure to search out the links among the different dimensions she describes. Zambia's situation *is* perilous. No less dependent on copper exports now than a decade ago, Zambia's economy is structurally dominated by major transnational corporations. The South African connection of those corporations combines with Zambia's historic incorporation into the southern African economy to render Zambia's capacity for self-directed development—or even for locally desired responses to external threats—both limited and constrained by South African policies. Zambia's regional situation, and the dominant role of the transnational corporations, condition Zambia's internal political structure. And that internal political structure, in turn, both perpetuates the existing relationships and leads Zambia to fear, rather than welcome, liberation movements struggling against white rule, particularly those with a Marxist ideology. These are not separate dimensions conveniently discussed in separate chapters, but interrelated phenomena whose very interrelationships offer clues to, and explanations for, Zambia's insecurity. By failing to explore those interrelationships, by adopting a static orientation, Pettman fails to recognize the roots of Zambia's insecurity in its own political structure, and the roots of that political structure in Zambia's regional and international situation. Finally, throughout the book, Pettman confuses Zambia with its current leadership: though there are very real challenges to Zambia's existence as an independent state, most of what concerns Pettman has to do with the maintenance in power of Zambia's current leaders.

Eschewing analysis, Pettman offers reportage. But reluctant to pursue links between events, Pettman leaves us with a very incomplete survey. And concerned with control, only incidentally does she focus on change.

JOEL SAMOFF

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Property and Industrial Organization in Communist and Capitalist Nations. By Frederic L. Pryor. (Bloomington: Indiana University Press, 1973. Pp. xviii + 513. $17.50.)

This is a remarkable book by an uncommon economist. The author—who has written on comparative economic systems—seeks to bring definitional precision and (insofar as the data permit) exact measurement to "issues of socialism, inequality, managerialism, bureaucracy, and centralization [that] have raised political passion for many decades" (p. 1). The degree to which he has succeeded will surely depend on the reader's tastes and predispositions, but in the process Pryor has produced a landmark study in comparative economic systems that political scientists would be well advised to notice. Not the least impressive are Pryor's extensive familiarity with the literature in both his own discipline and in many cognate fields of social science, his uncanny ability to scout out quantitative data from many countries and to manipulate them fruitfully, his clear delineation of issues at each step, and the general lucidity (even verve) of his exposition.

Choosing a broad bundle-of-rights definition of property and distinguishing it from mere ownership, Pryor uses the concept (in line with a growing trend in economics) to designate a given set of economic relations between men. Thus, a property right is both control over resources and a source of income, and property rights together with systems of motivation and information define the over-all economic system. His purview is wide indeed, and he reaches out in a number of major directions, carefully defines the concepts and the issues, and measures everything measurable in order to test statistically important propositions with respect to economic institutions. Where there are no usable data the author does not necessarily give up quantitative analysis. Thus, in inquiring into the influence of wealth distribution on income distribution, he turns to simulation models to examine the effects of such variables as the rules of inheritance, differential fertility among income classes, marriage patterns, and the intergenerational savings function (ch. 4). One way or another, Pryor's ultimate test is always: what do the figures tell us about institutions? He is not immodest about his approach, nor too modest about his findings, which reveal a good number of surprises as well as confirmations of conventional wisdom.

Among the empirical studies in the book that this reviewer found most enlightening are: The testing of various hypotheses to explain the degree and distribution of public ownership (nationalization) in different countries, where the causal factors "seem to lie outside the sphere of economics" (p. 65); comparative statistical investigation of the distribution of nonagricultural labor incomes in a number of both East European and Western countries, which he follows up with the finding that "within any modern economic system the inequality of labor income is vastly more important [at least statistically–G. Grossman] than the inequality of wealth holdings" (p. 89); and the concise but highly informative comparative analysis of the East European reforms of the 1960s, couched in terms of changes of property rights and industrial organization (ch. 7). The last is a masterpiece of analytical exposition and is highly recommended for those seeking to make sense of that complex matter. Finally, one should mention the probably somewhat less successful but equally stimulating empirical inquiry into the multidimensional conundra of centralization of control rights (Ch. 8).

Many will find equally helpful the various thorough discussions of major concepts and of the issues connected with them, concepts such as property itself; socialism; managerial control; convergence of systems (which the author views with a large dose of skepticism); and so forth. Often the concepts are further developed in considerable depth in the 60 pages of Research Notes (Appendix A). These sections should make valuable outside reading for courses in more than one discipline. In addition, the long bibliography, a glossary of some 50 terms, and (especially) the liberally annotated reference footnotes in the text, are a bibliographic gold mine for future researchers and students. The 18 statistical appendices provide mathematical explanations and additional quantitative material. There are also a helpful subject index and a name index–and comparatively few printing errors that this reviewer noticed (half of them, unhappily, in a transliterated quotation from Pushkin).

It hardly needs stressing that the application of formal statistical techniques to international or intertemporal data for the testing of propositions regarding economic (or politico-economic) institutions is a powerful method, yet one fraught with considerable problems. To his credit, the author is quite aware of both strengths and weaknesses; there is nothing naive about his approach. Nonetheless–and leaving aside the mainly technical problems of data and statistical methodology–two general types of problem may be indicated briefly.

First, the statistical method perforce tends to accord less attention to the less easily quantifiable factors, such as ideologies and historical forces. Now, Pryor does not completely omit ideology from his ken; in fact, he devotes an appendix (A-19) to alternative definitions of ideology and furnishes a helpful tabular classification of "world views." But not unexpectedly he does almost nothing else with it. As for historical forces, they are to a degree implied in his various "developmental propositions" (cf. p. 372), i.e., findings relating to temporal changes in such variables as public ownership or centralization of control. These generally rest on the analysis of fairly short time series and refer to "history" in only a limited sense. Hence, it is not surprising that the seminal work of, say, Reinhard Bendix or Alexander Gerschenkron–authors who stress the interplay of systems, ideology, and history–is by-passed in the book under review, though Marxism does receive a good deal of attention.

Secondly, findings of international statistical comparisons of institutional phenomena cannot always be taken by themselves; they must be carefully interpreted in view of the associated institutions. Take, for example, comparative findings on the distribution of income or wealth. The statistical measures mean little unless we also take into account such (often unquantifiable) considerations as: freedom of consumer choice and rationing of consumer goods; the prevalence of illegal incomes (which naturally escape the statistics) and black markets, which are of enormous importance in the USSR and other East European countries; differential access to goods and opportunities among social groups in a given country; the significance of ethnic affiliation, status, prestige, connections, and pull in using one's pecuniary resources; freedom of choice of job or "people control" (as in the PRC); degree of job security; and, on a more general plane, the extent of micro-disequilibria throughout the economy. For these reasons, comparisons of the distribution of labor incomes in Western and East European countries (Ch. 3) are even more difficult to interpret than the author warns us to be the case.

This is a book of first-rate importance for the variety of reasons already adverted. Indeed, it is one of a series of important generalizing works in the field of comparative economic systems to appear within a brief span of years—a series that includes, in addition to the present statistical investigation, such very different works as the collection of conference papers edited by the late Alexander Eckstein (*Comparisons of Economic Systems: Theoretical and Methodological Approaches,* 1971), attempts at formal mathematical modelling (as by Jacob Marschak and Roy Radner in *The Economic Theory of Teams,* 1971, and by J. M. Montias in *The Structure of the Economic System,* 1976), and the partly institutionalist, partly neoclassical *Economic Institutions Compared,* 1977, by J. P. D. Wiles. Altogether, substantial fare with appreciable borrowing from other social sciences.

GREGORY GROSSMAN

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The Mexican Profit-Sharing Decision: Politics in an Authoritarian Regime. By Susan Kaufman Purcell. (Berkeley: University of California Press, 1975. Pp. iv + 216. $15.75.)

A case study grounded systematically in a theoretical approach is more interesting generally than unstructured description, and a strength of this book is its attempt to apply aspects of Juan Linz's work (c. 1972) on authoritarian regimes. The decision to introduce a national profit-sharing system in the early 1960s is examined in the context of Mexico as an "inclusionary authoritarian regime." The goal is to understand how political processes in an authoritarian regime are similar to or different from corresponding processes in other types of regimes. Authoritarianism, along with corporatism and dependency theory, is of recent interest to students of Latin American politics due to the dissatisfaction with the "early" (1958–1967) SSRC political development school. Given the likelihood of continued interest in Linz's approach, we might consider its uses and limitations generally and as reflected in this case study. For just as a theoretical approach structures inquiry and provides for cumulative comparative work, it may lead us to expect patterns of behavior, sometimes more in eager anticipation than reasoned choice. Assumptions may become prejudices and hypotheses, points to be proven. We must beware, then, when the handles become manacles.

An authoritarian regime, following Linz, is characterized by limited pluralism, controlled participation, patrimonial rule, and elite consensus. A key premise is that it is useful to conceptualize about authoritarian polities as comprising a distinctive regime type, rather than as imperfect mutants of liberal or totalitarian systems. A second assumption is that the government (as one set of actors) controls societal interests (distinctive and subordinate sets of actors) and can maintain a separate decision capacity. Unlike pluralist systems, the authoritarian decision process is not determined by group interests but by the government, guided by a separate "state interest." The handles allow us to cast off the teleological bias that Mexico over time must somehow approach liberalism or totalitarianism, and facilitate more accurate description and classification. The manacles are its tendency toward static analysis (although this is not inherent), and an emphasis on process dimensions (interest group activity, ideology, style or rule), rather than on substantive policy outputs.

One is struck at the outset of this book by the author's unselfconscious embrace of tautology, the opiate of model-users, as she notes: ". . . there is now substantial agreement regarding the defining traits of an authoritarian regime, as well as a fairly strong consensus regarding the defining traits of an authoritarian regime, as well as a fairly strong consensus regarding the authoritarian nature of the Mexican government" (pp. 8–9). The problem is not vexing in setting the stage, and—based in the strength of static analysis—one of the best chapters is a characterization of the political environment. Here the accumulated literature (accurately and comprehensively surveyed) is synthesized in the authoritarian framework. Particularly good is the description of interest associations, and there is clear awareness of the varying degrees of autonomy and influence enjoyed by the several sectors.

The framework's process bias (style of rule and interest group behavior) begins to cramp when attention is turned to conceptual basics and an analysis of the decision itself. In a rather densely reasoned section on regime definitions, Guillermo O'Donnell's alleged distinction (in *Modernization and Bureaucratic Authoritarianism*) between "inclusionary" (he does not use this term) and "exclusionary" systems according to the government's orientations toward popular demands is significantly altered. O'Donnell classifies Mexico and Brazil as exclusionary regimes because the lower classes are excluded from material and status benefits as a deliberate strategy at a given stage of industrialization. Professor Purcell sees the exclusion from material rewards to be less significant than the role of coercion in *how* they are excluded: "The fact that the lower classes in Mexico do not share equally in the country's riches does not mean that they are excluded from the political system.... An exclusionary authoritarian regime is one that relies heavily on coercion to forcibly exclude one or more mobilized groups from participation in the system.... An inclusionary authoritarian regime does not forcibly exclude any mobilized group from participation" (pp. 7–8). Mexico, thus defined, is an "inclusionary regime," and O'Donnell is left standing on his head. But the significance is that O'Donnell is interested in *what* the policies are, *why* they are pursued, and *who* benefits. Purcell is ambivalent on these questions, in part because the adopted framework leads her to stress *how* the policy was formulated.

Turning to the case study, one wonders whether the authoritarianism framework is well suited to the task of policy analysis. It is perhaps useful in describing the controlled nature of labor and peasantry and characterizing how the policy was formulated, and the best sections of the book describe the sequences and maneuvers in reaching the decision. But the author must resort to eclecticism to account for why the profit-sharing scheme was launched and who benefited from it.

The principal hypothesis derived from the framework on why the decision was adopted is that there exists a separate state interest; the state controls the principal groups; thus if there was a profit-sharing scheme, it was because the state willed it. In Mexico in recent years the government and the business sector have been joined in an "alliance for profits." The fact that the government wanted a profit-sharing scheme prevented the business groups from rejecting it, and the fact that the scheme was not really redistributive means that "it is fair to conclude that the government is not yet interested in profoundly altering the status quo" (p. 147). One gathers that the temporary congruence of state interest and business was largely coincidental. Should the government alter its stand, the business community would surely knuckle under. In the authoritarian imagery, then, we are dealing with a political economy of the "invisible fist."

An alternative hypothesis of merit begins from a different assumption, that governments are constrained by the nature of their supporting coalition. In Mexico, business is a key pillar of economic growth, and the business-government fate is closely intertwined. Thus, the profit-sharing device was adopted in 1961 because: (1) it cost very little and would not retard foreign investment or economic growth; (2) it was a powerful symbolic gesture which aided the government in reducing class tensions and reinforcing the moderate elements of the organized labor movement against radicals who were gaining strength; (3) it was a complicated measure (based on income tax returns!), and thus nicely suited for selective enforcement to reward friends of "cosa nuestra" and punish outsiders. There is ample evidence throughout the book to support this hypothesis, and one senses that the author is powerfully attracted to it.

But rather than seriously questioning the assumptions of the framework or stating coherent alternative hypotheses on *Why profit-sharing?* and *Who benefits?*, we find either assertions taken on faith from the framework or eclectic reasoning. A catch-all list of factors is suggested to account for the decision: ideology, the president's personal interest and prior experience, symbolic quiescence, labor unrest. In the process, unfortunately, the framework tends to get in the way of explanation. For example, an insight: "The 1957–1959 labor disturbances ... resulted from the persistent neglect of organized labor by Mexican presidents. The profit-sharing reform of 1961 was part of a 'pay-off' to organized labor once the labor movement had been demobilized" (p. 135). But the framework emphasizes controlled pluralism and presidential preeminence; thus, "the labor unrest of the *late 1950s* . . . was not followed by new legislation benefitting the labor movement immediately, but approximately *four years later.* The delay not only reinforces the president's patrimonial image, but avoids giving the impression that violent disruption is rewarded. To give such impression would encourage a higher level of mobilization and would indirectly threaten the stability of the authoritarian regime" (p. 137; my emphasis).

Who benefited from profit sharing? The author is ambivalent on this, and the framework provides little assistance. Ultimately, she concludes, the government benefited, because it got what it wanted. A point of dubious validity is that labor also benefited, because government support gave it more than would have been possible in a liberal democratic system (p. 145). In terms of where the money went, we really do not know, but this is not Professor Purcell's fault, because the government has not published data on distributions under the profit-sharing scheme. The silence on this point seems interesting in itself.

Indeed it *is* possible that authoritarian regimes function in a distinctive manner, that Mexico *is* of the species, and that the patterns of decision making there conform to the model. Unfortunately, in this case study the key points in the logic are asserted rather than demonstrated. One feels reasonably comfortable with the framework in certain respects, particularly the description of process dimensions. But the assertion of "state interest" to account for specific policy outcomes is an unsatisfactory way of avoiding some hard thinking about the dynamics of the governing coalition and the realities in which the Mexican policy makers operate.

JOHN J. BAILEY

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Alternative Strategies for Papua New Guinea. Edited by A. Clunies Ross and J. Langmore. (New York: Oxford University Press, 1973. Pp. 263. $18.25.)

Although it is some years since this book was published, it is useful to review it now as the editors claim that much of the analysis and prescription "will remain relevant for some years to come" (p. vii). For those with knowledge of the Papua New Guinea scene it is also interesting to see to what extent some of the ideas here have been adopted by the now independent PNG government.

The very title of the book is misleading. According to the editors, "If this book has a theme, it is that there are alternatives" (p. vii). Few of the chapters, however, put forward a strategy: they make personal criticisms of and suggested amendments to what was then Australian policy on topics covered here, but they do not outline effective and realistic plans to reach specified ends.

Perhaps the most politically relevant chapters to the present political climate in PNG are those dealing with secession. Yet these are the weakest chapters. These chapters by James Griffin, Robert Waddell and Anthony Clunies Ross are uniformly naive, unrealistic, and internally contradictory. Griffin underrates the determination of a government forcibly to prevent secession and appears to believe that if Australian forces refused to be involved "the Papua New Guinea government may lack the resolution and the force to do so" (p. 130). It is irrelevant and gratuitous to state "an independent Bougainville could also be very peaceful without being as boring as, say, Nauru" (p. 127), and his rosy picture of a rich and prosperous independent Bougainville assumes that secession would be peacefully granted and ignores the difficulties of a totally copper-dependent economy (e.g., present-day Zambia). One also wonders why Griffin devotes 13 pages to Papua when he cannot estimate the extent of secessionist sentiment there (p. 103).

"Secession Without Tears" by Clunies Ross, creates its own myths in its title. It claims that those who separate should do so by consent (p. 133), failing to realize that this is where the argument usually begins. He decries the supposed argument that large states are "better" than small ones (p. 131) and proposes that in PNG potential secessionists "might be given a constitutional means of pursuing their objectives." He also proposes increasing decentralization of decision making to meet potential secessionists' claims (p. 132). While the latter is sensible, the proposal for a "secession formula" ignores the political realities in PNG and overrates the patience of both potential secessionists and the central government.

Waddell's chapter, "Constitutions and the Political Culture," undermines itself by internal contradictions and poor analysis. He notes that "to be successful a constitution must be a natural product of a country's political culture" (p. 88), without specifying what is meant by "successful." He notes that the Burmese "states" had little in common save their contact with the British raj (p. 87), but this applies to a number of countries including PNG and may act for a time as a unifying factor. Like the above two chapters he suggests secession before rather than after independence (p. 97) which ignores the Australian government refusal to tolerate such moves. He argues that a multifaceted PNG would be better off with a one-party state (pp. 94–95). But diverse interests often require diverse parties. Waddell fails to say how one party helps this situation where "regional or religious groups fear neglect or persecution if members of other religions or regions obtain power" (p. 94). He is similarly blind when he suggests "the emergence of a large, broadly-based political party with firm leadership is greatly to be desired" (p. 97). Having noted that PNG is heterogeneous (p. 94) and that neither class nor ideology are likely to be the mainsprings of political parties (p. 96), where does he expect this broadly based party will appear from?

The other contributions to this book are more valuable. Tos Barnett's "Law and Justice Melanesian Style" is a fluent outline of the contradictions between Western law and Melanesian life style. He makes suggestions about how some of these contradictions could be overcome or at least lessened, but it is worth noting that some of these "remedies" have similar application in Western societies.

Nigel Oram's "Administration, Development, and Public Order" examines the then administrative organization and considers what administrative reforms are desirable for self government and beyond. He notes the over-centralization and overly complicated nature of the public service (p. 7), and argues for "much closer coordination of governmental activities at district level" (p. 17). For this he suggests the establishment of village courts to bridge the gap between administrative and judicial law enforcement (pp. 41, 49–51; Barnett p. 67), the development of an elite corps of district and assistant district commissioners (p. 55), and politically informed higher-level public servants (p. 56).

John Conroy's chapter on educational policy and John Langmore's on public service pay and localization are both useful outlines of those topics, but they are inconclusive in that they state the problems without providing "alternative strategies." Ross Garnaut's "National Objectives and the Choice of Industries" is a difficult paper for noneconomists and shows, without resolving, the dilemma of economists everywhere—whether to pursue growth or welfare objectives (p. 165). Finally, the two last chapters by Clunies Ross are highly pertinent essays on the economic policies which he believes should be pursued by PNG. He argues that localization of management rather than ownership should be attempted, and resolves Garnaut's unanswered dilemma by arguing for "social planning" and goals "which go beyond the mere increase of 'income' per head or the extension of individual choice" (p. 231). This approach "requires political leadership that is united, popular, determined and sensitive" (p. 233).

This is an interesting book, if not always perceptive.

PETA COLEBATCH

Waigani, Papua New Guinea

From Lenin to Stalin. By Victor Serge. (New York: Monad Press, First Edition 1937; Second Edition 1973. Pp. 160. $6.95.)

The reappearance of *From Lenin to Stalin* adds one more title to the growing list of works by Victor Serge republished in recent years. New editions are now also available in English—translated from the French—of three of his novels *(The Case of Comrade Tulayev, Men in Prison, Conquered City)*; of his autobiography *(Memoirs of a Revolutionary 1901–1941)*; of two other historical commentaries *(Destiny of a Revolution* and *Year One of the Russian Revolution)*; and of his biography, written jointly with Natalia Sedova Trotsky *(The Life and Death of Leon Trotsky)*. Publishers presumably have decided that Serge will attract a number of different reading publics—political scientists and historians interested in the Soviet experience, international Communism, and the origins of totalitarianism; the literary readership on the lookout for social novels imbued with an intense moral concern; and the "New Left" in search of non-Stalinist revolutionary heroes on the model of Rosa Luxemburg, Peter Kropotkin, Leon Trotsky.

Certainly, if one is looking for an intellectual of world repute who participated fully in the romance of the revolution while escaping entrapment in the *apparat* or the Cheka, then Serge fits the bill remarkably well. Born in 1890 in Brussels of parents who were themselves Russian revolutionary emigres—his real name was Kibalchich—Serge became an anarchist while still in his teens and editor of the illegal *l'Anarchie* at the age of twenty. He spent the years 1912–17 in Belgian prisons; took part in an abortive uprising in Barcelona in July 1917; found himself an internee in France in 1918; reached Russia in 1919; threw in his lot with the Reds; lived through the crisis of the Civil War in Petrograd; published a book in 1921 on the role of the anarchists in the Russian revolution; worked with Zinoviev at the executive committee of the Comintern; spent the years 1922–26 as a Comintern agent in Western Europe; was in Germany for the October uprising of 1923; returned to Russia in 1926 to join Trotsky's Left Opposition; spent the years 1933–36 as a prisoner of the GPU; was allowed to leave for the West following "l'Affaire Victor Serge," an intensive campaign waged for his release by Western intellectuals (among whom even Romain Rolland and Andre Gide played a role); worked for the Trotsky cause in the years 1936–37 and for POUM during the closing stages of the Spanish Civil War. He escaped Vichy France for Mexico at the last possible moment and died there poverty-stricken and lonely in 1947.

From Lenin to Stalin was written in 1936, published in 1937, and, like his *Destiny of a Revolution*, belongs to the period of collaboration with Trotsky whom he met soon after reaching the West in April 1936. Trotsky relied on Serge's testimony, and was influenced by it, while then composing his *The Revolution Betrayed,* also published in 1937. In most essentials, Serge's book is a Trotskyite tract. It explains Soviet history along strictly dichotomous lines with the period 1917–23 described as that of "Soviet democracy," the "great years," and the post-1923 period as that of growing "bureaucratic" nightmare. This sudden transition of the body politic from robust health to cancerous degeneration is explained partly in terms of personality (Lenin's illness and death, Trotsky's fall) and partly in terms of sociology (the swamping of the veteran revolutionary elite by the post-October mass of careerists and opportunists).

Yet even here the lines of the impending dispute between Serge and Trotsky can be discerned in various judgments scattered discreetly throughout the text. "It would have been relatively simple to reach a compromise with Kronstadt and avoid useless massacres. . ." (p. 34); "moral criteria sometimes have greater value than judgments based on politics and economic considerations. . . . It is untrue, a hundred times untrue that the end justifies the means" (p. 58). Clearly, the alliance between Trotsky and Serge—eager to grapple with his personal experience of the GPU system—would not long survive. Serge was soon to produce a much more critical and authentic description of the early post-October period in his *Memoirs of a Revolutionary.*

Perhaps the most interesting aspect of the book under review lies in the fact that here was the first eye-witness report on the Stalinist terror as it approached its apex, the *Ezhovshchina.* Serge was the one who by some miracle escaped to tell the tale. In general, his analysis of the Show Trials stands the test and his imaginative reconstruction of Stalin's paranoid monomania likewise retains its interest today, is reminiscent of Solzhenitsyn's portrait in *The First Circle* and is arguably even more powerful.

Monad Press, then, is to be congratulated on re-issuing this book. Perhaps the full corpus of Serge's works will gradually be published in English. And is it not time for a full-scale biography?

JONATHAN FRANKEL

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A Social Basis for Prewar Japanese Militarism: The Army and the Rural Community. By Richard J. Smethurst. (Berkeley: University of California Press, 1974. Pp. xxi + 202. $14.00.)

Though much has been written about the role of the military in political decision making of prewar Japan, little has been known about the involvement of the Japanese army in the rural community. Richard Smethurst's book, which describes and analyzes the prewar Imperial Military Reserve Association *(teikoku zaigō gunjinkai)* and three other organizations, fills that gap.

As the author points out, the pre-1937 Japanese military was quite a small force of less than 250,000 officers and men, which every year drafted only about 15 percent of the 20-year-old men of the nation. In order to enhance the influence of the armed forces, spread military values, and strengthen national unity, the army authorities in 1910 sponsored the establishment of a centralized reservists' association. The new body was open to all men between the ages of 20 and 40 who passed the conscription physical examinations. By 1936 the association had a membership of 2.9 million, half of whom had never served in the armed forces. Before 1933 three other organizations were established under the auspices of the army: the National Youth Association, the military-training youth-schools, and the National Defense Women's Association, all of which were under reservist supervision. The reservists and the other groups utilized the members' traditional commitments to the village, in their effort to mold "national villagers" who would support the military and its policies. In this manner the army succeeded in becoming integrated into the rural community no less than the schools. Moreover, the fact that the local leaders of the reservists, youth, and women groups came from the upper strata of the village contributed to the strengthening of traditional social obligations in rural Japan. Smethurst makes the point that unlike the rise of Nazism in Germany, in Japan the military rose to power not as the result of social dislocation, but on the basis of its roots in a well-organized populace in the countryside. The Japanese army, as he puts it, succeeded "almost without trying" in establishing what Hitler dreamed about but could not achieve: a stratified "organic" society.

The book is well researched. It is based on extensive use of primary sources like unpublished archives, documentary collections, and the publications of numerous little-known organizations. Of special interest is the author's in-depth research into four agricultural communities of different size in central Japan. In these four case studies, detailed information was obtained from local archives, hundreds of questionnaires, and personal interviews. The book contains many tables and figures which provide data on various aspects, such as yearly enrollments of youth-schools or budgets of reservists' branch offices.

One may regret that out of the five chapters of the book, four are about the aims and the structure of the organizations under scrutiny and only the last chapter tells us about their activities. The reader would like to hear more about the involvement of the reservists or the other groups in the various political affairs of the period. Thus only one short passage (on p. 177) is devoted to the famous Minobe controversy of 1935, in which the reservists played a central role (although Smethurst elaborated on it in his contribution to George M. Wilson, ed., *Crisis Politics in Prewar Japan*, Tokyo: Sophia University, 1970). The book does not mention the task assigned to the reservists in the many "renovation" plans of the period, such as the role they were to play in Kita Ikki's "reconstruction" of Japan.

Another problem is the name of the book. We all know well that publishers prefer high-sounding titles which would appeal to a wide audience, though often the book deals only with a small portion of the announced subject. The name *A Social Basis for Prewar Japanese Militarism* for a book on the reservists' association and three other related groups may raise expectations which the author did not intend to fulfill.

BEN-AMI SHILLONY

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The Politics of Pluralism: A Comparative Study of Lebanon and Ghana. By David R. Smock and Audrey C. Smock. (New York: Elsevier, 1975. Pp. x + 369. $12.50.)

Audrey and David Smock, currently with the Projects Office of the Ford Foundation, conducted the bulk of their research for this volume during two and a half years (1969–72) of residence in Ghana and a 15-month period (1972–73) in Lebanon. In this book the authors detail a number of interesting inter-ethnic, country-specific relationships and make policy suggestions which should be of interest to development specialists. Many of the standard "mechanical formulae," which have been posited as offering hope for "national accommodation" in ethnically fragmented societies, are reviewed in this work—for example, the establishment of a "lingua franca" to overcome language diversity; political reform (e.g., the incorporation of minorities into the political system in a meaningful way, by proportional representation or confederation); the creation of a nationally oriented educational system (one designed to inculcate "national" values which transcend parochial loyalties, to foster appreciation of diversity and promote tolerance, and to develop a sense of history and identity with respect to the state); the use of the governmental bureaucracy to encourage the growth of an elite corps committed to the ideas of efficiency, effectiveness, and the larger political unit; the institutionalization of conflict management techniques.

After tracing the historical development of Ghana and Lebanon and examining social relations in those two states, the Smocks concluded that Lebanon's distinctive features (i.e., the presence of a number of minorities who had come to Lebanon seeking relief from persecution, the existence of both intra- and inter-sect conflict, extensive mixing of different groups, and systemic flexibility provided by the possibility of conversion) contributed to the evolution of the "Confessional Principle" in the matter of communal representation—the unwritten National Pact of 1943 established the formula for inclusion of the various communities—and helped to produce a system "able to manage communal competition and to reduce tensions even in a society with deep communal divisions" (p. 332). On a more general level the authors argue that "a political system that recognizes the legitimacy of communal interests and incorporates communal groups in a meaningful manner (i.e., as Lebanon does) has a far better opportunity to effect moderation and conciliation than one that refuses to come to terms with communal political actors (viz., Ghana)" (p. 332). While acknowledging that the "will to live together and to make the system work constitute essential ingredients" in state development (p. 333), the Smocks stress the value of institutional and policy approaches to national accommodation. Ghana's inability to institutionalize ethnic conflict management mechanisms, its lack of success in promoting a more national outlook among its people, its failure to satisfy the economic and social needs/demands of constituent groups, and the government's inability to promote efficiency and rationality as contemporary virtues lead the authors to conclude that, unlike Lebanon, Ghana's future will very likely include ethnic conflict and upheaval (p. 252).

Although they based their book on an extensive review of the theoretical literature on "ethnicity," on intimate knowledge gained from their participant-observer research strategy, and on information gathered from interviews with students and adults, the Smocks failed, as evidenced by the recent Lebanese civil strife, to assess accurately the significance of the issue of identity. In this failure perhaps lies the Smocks' major contribution to the field of ethnic studies.

While it has become fashionable to ask about effective measures of ethnic-awareness and to attempt to develop indicators of ethnic friction, some scholars of nationalism (notably Rupert Emerson and Walker Connor) have not been sanguine about the success of such efforts. The difficulty, it seems, lies in the nature of the identity bond (i.e., psychological and, hence, abstract). Because it is intuitive, this bond defies easy measurement: "The simplest statement that can be made about a nation is that it is a body of people who feel that they are a nation; and it may be that when all the finespun analysis is concluded this will be the ultimate statement as well" (Rupert Emerson, *From Empire to Nation*, 1960. p. 102). Hence the field of identity studies is a difficult one for those who seek absolutes. Perhaps Crawford Young's admonition about the ever-changing nature of the "self" should be a first principle of ethnic studies: "The definition of groups is in constant flux; any theory of ethnic conflict must incorporate change as a central element. . . . At any moment, ethnic conflict may appear to eclipse all other factors in the political equation; a few years later, the same cleavage may appear entirely muted, and quite irrelevant to explication of the political process" (*The Politics of Cultural Pluralism*, 1976. p. 5).

If a beginning is to be made in understanding ethnicity, authors of studies focusing on this phenomenon should seek to be as precise as possible. Discussion of research strategies and methodologies and provision of details would go a long way toward making it possible for scholars working in other geographical areas to replicate studies, to extend findings, and to, perhaps, refine research instruments. The Smocks may have contributed to this effort but there is no way to know for certain. Their study questionnaires are not provided; and information which is found in the appendix of the book indicates that their surveys varied in construction, content sought, populations sampled, and interview situations. Walker Connor ("Nation-Building or Nation-Destroying?," *World Politics*, 24, April 1972) has treated in detail the dangers inherent in a situation of terminological confusion where the proliferation of reference words suggests a multiplicity of entities when, in fact, most evidence indicates that one entity is being examined. In this regard the Smocks might have profitably treated the various Lebanese communities as distinct ethnic groups rather than simply in terms of their Muslim and Christian "confessional" identities. However, none of this detracts from the overall value of this book. The questions raised in this work are important ones and are worthy of consideration. For this reason area experts, students of nationalism, and development specialists would be well advised to place this title on their reading lists.

JOHN E. ROBERTSON

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A Survey of Race Relations in South Africa, 1972. By the South African Institute of Race Relations. (Johannesburg: South African Institute of Race Relations, 1973. Pp. 484. $3.95, paper.)

The South African Institute of Race Relations is an inter-racial organization unrelated to government or party, which engages in research and discussion, as well as more activist pursuits, in the complicated area of race relations in the Republic of South Africa. Aiming at the promotion of understanding and furthering "the social, economic, and political development of all communities" in that heterogeneous country, it periodically issues a *Survey*, which summarizes significant occurrences in its field of interest during the preceding year.

This *Survey* of 1972 is a complete, concise summary of racial developments and an excellent starting point for scholars and laymen who wish to study events within a particular time span. It does not interpret or analyze, that is left to the reader who wishes to penetrate more deeply. But one who reads it will obtain a broad sweep of the ways in which South Africa is moving, even though the sudden shifts from one subject to another make for a rather disjointed reading journey.

It is a compilation of items culled from miscellaneous sources like newspapers, accounts of parliamentary debates, and scholarly publications like the *South African Journal of Economics;* some information appears to have come from government officials directly. Documentation is now always complete, as some compilers have fallen into the habit of citing newspaper accounts of official statements rather than the documents themselves; this is particularly noticeable in references to United Nations actions. Court decision references occasionally suffer from the same defect (see pp. 95–96).

One advantage of this book is that it brings out facts unknown to the average person or even the Africanist whose specialty is not the republic south of the Limpopo, such as information about the education of children of the small Japanese and Chinese majorities, strikes (all illegal) by African workers, or the farming out of African inmates by penal institutions.

The book is not tendentious, however; recital of digested facts is uncolored by emotion and is objective. Since it is concerned with the broad topic of race relations and not with South African affairs generally, one might easily conclude that the compilers have racial axes to grind. Analysis shows this not to be the case. It contains plenty of ammunition for anti-apartheid crusaders, but there is also enough upon which it can be argued that racial relations in that troubled land are really improving.

Events in southern Africa have been moving so rapidly that one regrets that this digest of 1972 events is not more up to date. It was written before the detente with Zambia over Rhodesia, announcement of moderate reforms by Prime Minister Vorster, and South African intervention in the Angola conflict. But the passage of time has not rendered this book less valuable; in fact, it is a useful background for recent events and that is its principal interest to the American reader.

Because the race situation has affected South Africa's foreign relations, the *Survey* properly has a chapter on foreign affairs and a section on the United States. Although one wishes that much documentation had been more direct than the second-hand South African sources used, one cannot call the section biased. It notes the "soft" policy of the Nixon administration to South Africa, as well as the criticism of it, including a congressman's prediction of a Vietnam-type "holocaust" in South Africa.

A chapter on South Africa's territory to the northwest–called, tactfully enough, "South West Africa (Namibia)," with the United Nations name in parentheses–is a useful guide and could be read as an introduction to a study of what has recently happened there.

Early in 1972 the producer of this book was one of four organizations which the prime minister announced he intended to investigate through a parliamentary select committee–an incident recorded without fanfare on page 52. But he complemented the Institute by calling it "the least dangerous of all these organizations."

EMMET V. MITTLEBEELER

The Dynamics of Right-Wing Protest: A Political Analysis of "Social Credit" in Quebec. By Michael B. Stein. (Toronto: University of Toronto Press, 1973. Pp. xv + 256. $12.50.)

Social credit, a Canadian political phenomenon, is an economic and political ideology, a protest movement, a political party, and a manifestation of ethnic conflict. While Michael Stein describes social credit in Quebec with respect to each of these attributes, his objective is to point out that social credit in Quebec is one manifestation of right-wing political phenomena. He refers to organizational characteristics such as developmental patterns, leadership styles, and internal factional disputes to distinguish social credit as a right-wing movement from either political parties or revolutionary movements. As evidence of these characteristics, he draws on the writings of the social credit's original theorist, Major C. H. Douglas; on previous studies of the movement's emergence as a political force in Quebec; and on an analysis of interviews with Quebec social credit leaders and activists.

The interview data do add to our understanding of the attributes and beliefs of political activists and do show, once again, that activists on the right are not estranged social misfits clinging to outmoded traditions. The Quebec *créditistes*, on the whole, have status in their community; are helpful of achieving political power and of having an impact upon public policy. Stein's internal analysis of the data on activists shows that a minority of them do have the characteristics attributed to right-wing extremists in much of the literature on comparable American phenomena of the 1950s and early 1960s, i.e., they are alienated, not well educated, and they join the movement for its social affiliations more than for its political objectives.

A shortcoming of this study is the discontinuity between the focus of the narrative history of the ideology and organization of Social Credit in Quebec and the analysis of the interviews with the movement's leaders. Generational differences in leadership style and goals, a theme of the narrative history, is not discussed with reference to the interviews with leaders. Another shortcoming is stylistic: this study was originally a doctoral dissertation. The conventions of organizing and presenting research particular to a thesis have remained in the book, to the detriment of the flow of the narrative and crispness of the analysis. Nonetheless, *The Dynamics of Right-Wing Protest* should be a reference for students of Canadian politics and of right-wing politics.

SHEILAH KOEPPEN MANN

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Politics, Power, and Bureuacracy in France: The Administrative Elite. By Ezra N. Suleiman. (Princeton, N.J.: Princeton University Press, 1974. Pp. 440. $20.00, cloth; $9.75, paper.)

Every now and then someone submits a doctoral dissertation that does more than demonstrate professional competence. It announces the arrival of a first-rate expert in a substantial field. Ezra N. Suleiman's *Politics, Power, and Bureaucracy in France* does just that. It does it somewhat laboriously with its burden of 639 footnotes, 79 tables, 2 graphs, 8 diagrams, and one questionnaire, plus a bit more social science methodology and apparatus than either the subject or this reader can profitably absorb; but it does it powerfully, with a parade of well-ordered information and ordering insights.

The aim of the study is to examine the ranking civil servants of France, probably the most powerful and possibly the most able of any Western country, as part of the French political-administrative system. Though the ranking French civil servants have been studied more closely in both popular and scholarly literature than Suleiman's introduction would lead one to suspect, the present volume is the most solid in its compilation of information and the most ambitious in its efforts to relate administration to politics. It not only amasses—and controls—an impressive arsenal of empirical materials about their origins, training, attitudes, and behavior (and here "empirical materials" means, most prominently, interview data—the easiest to amass but the most difficult to control), but it also makes sense out of most of that information by relating it to the substance of French politics.

The dust jacket, the introduction, and an earlier piece by the author in *World Politics* represent the study as though it were an intelligent write-up of information gathered and sorted through the careful application of a scientific method of empirical enquiry. For better or worse, and I think for better, it is nothing of the kind. In all its many strengths, it is the author's book, and not the method's. The author's extraordinary knowledge of French politics and administration and his sharp intelligence are what carry the book along. The method—a series of elegantly conducted interviews with 135 very top civil servants—generates lots of information. That information is then carefully presented in tables, charts, and quotations, and the 26-page questionnaire is appended. But the survey findings do not begin to tell us how the French bureaucracy works. Suleiman does that, and not on the narrow and rickety basis of interview data. Except for some unsurprising data about the elite origins and backgrounds of the top civil servants, information readily available elsewhere, the interviews generate cautious statements of views, which are supposed to express attitudes and perceptions—all in all, something quite different from empirical findings about fact and process. An empirical examination of the workings of the French bureaucracy, one that examines how decisions are taken, and more interesting yet, how the decision-making agenda is established, would be an invaluable addition to our knowledge of comparative politics. But that is not what Suleiman does in this book. Essentially, the interview technique permits him to play the role of participant-observer; in this case it is more a literary device than a scientific technique. Because of his rich knowledge of French administration and politics, he is able to profit from that position. The survey results are really his notes, his departure points. The book, therefore, is not a pioneering work in the scientific, empirical study of administration. It is not even an application of some previous breakthrough in empirical methodology to the study of the recalcitrant French. It is, instead, an intelligent and concrete study of the workings of the administrative state based on familiarity, understanding, and insight.

In the course of his reflections on the political nature of administration, Suleiman argues with Grant McConnell on the nature of interest group pluralism; with Michel Crozier on the adaptability of the French system; and with Jean Meynaud on technocracy.

The study treats a golden age of French bureaucracy, the fifties and the sixties, a period in which the ranking civil servants seemed to be not just all-powerful but all-successful. They took on the giant task of overseeing the nation's total transformation, and they succeeded. GNP grew by an average of five percent per year, far faster than the U.S., about as fast as the awesome Germans. They celebrated their success. The atmosphere of those years in the French bureaucracy (not a place traditionally known for its joyful ambiance) made the Best and the Brightest phase of Washington seem like a passing wink. Not only did they transform the nation—and almost no part of its escaped improvement at their hands—but they also provided the commentary on the process. Under various and sometimes amusing pen names, they wrote the books, pamphlets, and articles which praised, explained and even criticized that transformation. Because Suleiman stays so close to the Directors of the Central Administration and the Cabinet staffs (his impressive survey sample) he does not discuss the ranking civil servants who moved—with astonishing agility—all over the complex administrative system: men like Louis Armand, Paul Delouvrier and Francois Bloch-Laine who best represented what Frenchmen had in mind when they changed the name (but not the dose of irony) for the super civil servants from bureaucrats and Mandarins to technocrats. It is a bit like analyzing the administrative politics of New York sans Bob Moses. Perhaps the best empirical indicator of their hold on the nation's imagination, as well as its levers of control, was the results of a poll taken among students at the school for political science in the early sixties. When asked to list the men they most admired, the future *hauts fonctionaires* put Francois Bloch-Laine first on their list. John F. Kennedy came in second. De Gaulle and Sartre finished out of the money.

The technocrats developed and modernized France, and they apparently had no trouble knowing what those words meant. No imaginable group of Berkeley students would be blessed with such ideological ease. Suleiman raises this critical question of what ideology guided their actions. His discussion relies on quotations from his interviews and commentaries on the works of other writers; sometimes he pulls their sentences out of context in order to set them up to push them down. But he does not satisfactorily resolve the question of ideology. The failure is not due to some minor technical problem, nor is it of minor importance. And the difficulties compound when he turns to the delicate relation of the State to big business. He discusses it in terms of how the state deals with interest groups. Within that framework he succeeds in confronting American approaches to interest groups—both mainstream liberal-pluralism and its critics, especially McConnell—with French discourses on the St. Simonian nature of the state. But the whole enterprise springs leaks when he reaches the critical distinction that the civil servants repeatedly make between legitimate and illegitimate interests. That distinction is never fully explained: it just sits there like a kind of Cheshire grin mocking, as it obscures, any real understanding of the relation of the state to big business.

In part his difficulties in these important areas are due to his not having a concrete framework in which to situate the civil servants' actions and their talk. Even a rudimentary "advantages of backwardness" approach, straight out of Veblen, would have added much in the way of analytic power to his explanation of their actions, their ideology, and their unflinching *bonne conscience*. Without such an ordering structure it is hard to relate what they say to what they do, and what they do to what is going on—to the specific character of French economic development which gives the different parts their meaning. As a result, relatively simple but revealing questions and facts are not noted. For example, would their happy balances of ideology and action, of power and independence have been disturbed had they ever opposed a single merger among big industrial firms rather than having consistently promoted and subsidized them? Unless we can relate their behavior (as well as their origins and attitudes) to what in fact they were doing—to their role in a particular phase and form of development—we are left with many well-informed but separate lines of argument. We are also unprepared for developments subsequent to the book such as Prime Minister Chirac's dropping the political subtlety of the Gaullist line and successfully taking a big political stick to the top St. Simonians.

Suleiman tells us in his introduction that he is now working on a study of "the interrelation of public and private elites in France in the formulation of public policy." It seems that it will pick up at just the point where this first study leaves off. It is almost sure to display what so distinguishes this first volume, the author's remarkable knowledge of his subject, plus his intelligence and craftsmanship. If the reliance on attitudinal surveys can be reduced (or perhaps replaced by more robust empirical materials) and perhaps a greater dose of irony can be added—something the subject desperately deserves—it should be a seriously interesting study, not just for French experts, but for all those puzzling over the workings of the modern administrative state. The first volume already is.

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Politics in Zambia. Edited by William Tordoff. (Berkeley: University of California Press, 1974. Pp. xiv + 439. $14.00.)

Zambia—an enigma in African politics. Is Zambia a relatively wealthy and stable African state, guided by a clear sense of moral purpose toward a humane and egalitarian African socialism? Or is Zambia a dependent, externally oriented African state, constrained by its white-ruled southern neighbors, and pushed by erratic copper prices and a rapacious sectional elite toward a very unequal and unstable African capitalism?

These contrasting images reflect both arguments about Zambia and a much larger discussion about Africa as a whole. Zambia offers a microcosm: wealth and dependence, articulated national ideology and severe sectionalism, urban splendor and periurban shantytowns, industrial base and continued food imports, and perhaps most important, a front-line position in the struggle against minority rule in southern Africa.

The recent book by Tordoff and his colleagues is a major contribution to this debate, but at the same time it is uneven and frustratingly incomplete. Like Zambia's independence, it is an event delayed beyond its time.

As outlined in Tordoff's and Molteno's introduction, Zambia (Northern Rhodesia) was colonized as an outgrowth of interests and activities further south. The extension of company rule rendered Zambia a copper-supplying outpost of the southern African economy. Despite historical ties with East and Central Africa, Zambia was turned southwards. Transport ran south to the coast. Imports and exports passed through Zambia's southern neighbors. Manufactures, skilled labor, capital, and even agricultural produce were imported from the south. Settler efforts to control the whole region and a lack of advanced education and training for Zambians delayed Zambia's independence.

While research on other African countries has progressed beyond the general country-level studies common earlier to analyses of localities, regions, elites, development strategies, and so on, this collection remains in the panoramic sweep genre, attempting to review politics in Zambia as a whole. In its sweep, this volume is very well informed and for the most part carefully researched. Its authors did all extensive research in Zambia; most taught at the University of Zambia. Their work is as much informed by systematic data-gathering as by the more informal collection of information possible only for sensitive residents.

Particularly strong is the treatment of cleavage and conflict. Molteno carefully documents the politicization of regional, religious, and ethnic ties and skillfully relates the sectional base of politics to issues of power and control. He returns to this theme in several subsequent chapters.

The authors have documented in Zambia a number of general findings on Africa. Rasmussen's decolonization chapter stresses the local base of anti-colonial grievances and leadership, even in relatively urbanized and mobile Northern Rhodesia. Molteno highlights the shifting salience and situational determinants of ethnicity. Gupta's analysis of the mineworkers' union argues that in Zambia, as elsewhere in Africa, the dominant party quickly moves to control potential competitors for power, even former pillars of support.

This volume is much better integrated than is commonly the case for collected works, perhaps because Tordoff and Molteno have co-authored several of the chapters. Major threads are woven throughout the book; later chapters pick up the discussion of issues raised earlier.

Unfortunately, however, the institutional approach of the papers in this volume, and their focus on "constraints" as the major explanatory variable, prevent the authors from progressing beyond a solid political history.

Tordoff's and Scott's discussion of parties is largely a detailed description of origins and electoral behavior. But that behavior is never systematically related to underlying structural features of the situation (or to other potential explanatory factors). The social origins of national leaders help explain their values, strategies, and tactics. The pervasive influence of South Africa not only constrained but directly determined the definition of key issues. One result of this narrow scope is that nowhere in the volume is the dominant party, UNIP, subjected to the searching analysis its role requires.

Tordoff and Molteno locate administrative difficulties in the structure and (lack of) training of the bureaucracy. But if an institution's structure provides an explanation for its behavior, then we must know why that pattern of organization persists. Is elite socialization the key? Does the explanation lie in the interplay of power centers? Is a bureaucratic bourgeoisie an independent actor? Are foreign influences the determining ones? Trapped in their institutional orientation and in a bureaucratic civil service model, the authors are unwilling to consider seriously a politicized administration, a direction pursued in neighboring Tanzania not inconsistent with Zambia's socialism.

Ideology is treated almost entirely in terms of public statements and official writings. But since the craft of politicians involves the manipulation of symbols and the differential presentation of ideology to different audiences, that cannot take us very far. What is required is a study of ideology as manifested in specific actions, particularly where ideology is itself a tool in the struggle for power. In Zambia, where public leaders are quick to resort to humanism, the official ideology, to condemn opponents, power lies in the ability to define humanism.

Contributors frequently refer to large undifferentiated categories. But surely an analysis is very limited that does not go beyond references to the "government." President Kaunda, public officials, party leaders, and members of Parliament are all the "government," but clearly their interests do not always coincide. Is the "government's" behavior a reflection of conflict between the president and party leaders? Or, of a tacit agreement among certain provincial leaders? or, of a rare assertion of parliamentary authority? Similarly for "Zambians." Although different segments of the population are mentioned and occasionally described–mineworkers, cash-crop farmers, lower-level party officials, aspiring Zambian industrialists–rarely are those differentiations incorporated into the analysis. To what extent, for example, are ostensibly regional conflicts in fact rooted in the differing interests of rural cash-crop farmers and urban mineworkers?

One result of this institutional approach is that major insights remain undeveloped. Molteno's and Tordoff's conclusion drops a perceptive discussion of nascent class conflict in deference to a favorite concept of Zambian leaders: "indiscipline." A second result is that the volume is replete with the moralizing and admonitions of the authors, all too often patronizing and condescending. Finally, a third result is that there is no coherent discussion of development strategy, a very political issue in Zambia. Perhaps some of these gaps will be filled in the promised sequel volume.

Hence, students of Zambian politics are left with the contradictory images. In 1974–75, President Kaunda pursued diplomatic interchange and dialogue with South Africa, a reversal of the dissociation policy reiterated over the past several years. Why? Because of the constraints imposed by Zambia's position in southern Africa? Or because of the efforts of national elites to expand their economic roles and remain in power? Since its independence Zambia has not only failed to reduce its dependence on copper but as well has suffered a drop in agricultural production, policies to the contrary notwithstanding. Has Zambia in fact pursued a viable development strategy intended to maximize earning foreign exchange in order to construct a more solid and self-directed economy? Or, rather, have foreign influences and leaders' perceptions of national and self-interest led Zambia to pursue a strategy of further integration into the world capitalist economy as a peripheral, dependent state?

Tordoff's well-informed, reasonably well-integrated survey (to its credit, available at reduced price in Zambia) can provide only a foundation for a more penetrating analysis. The realities of the current situation cry for much more.

JOEL SAMOFF

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Public Health in the People's Republic of China. Edited by Myron E. Wegman, Tsung-yi Lin, and Elizabeth F. Purcell. (New York: Josiah Macy, Jr. Foundation, 1973. Pp. 354. $7.50.)

Medicine and Society in China. Edited by John Z. Bowers and Elizabeth F. Purcell. (New York: Josiah Macy, Jr. Foundation, 1974. Pp. 176. $7.50.)

These two works are the result of interdisciplinary joint conferences held by the Josiah Macy, Jr. Foundation in cooperation with other institutions. *Public Health in the People's Republic of China* represents a 1972 conference sponsored by the Macy Foundation and the School of Public Health and Center for Chinese Studies of the University of Michigan. *Medicine and Society in China* is based on a somewhat more recent (1973) forum convened by the Foundation jointly with the National Library of Medicine. Both books are published as part of the Macy Foundation Series on Medicine and Public Health in China.

As the list of contributors to these volumes runs to some 20 authors, I will not attempt to mention all the papers individually. Rather, a brief sketch of each book will be followed by a few comments on some specific selections, which were chosen only to give the reader some idea as to the range and variety of contents in the two volumes.

Public Health in the People's Republic of China begins with a historical overview of the specifically Chinese background to public health problems. These first papers outline the relevant sociological features–the awesome socioeconomic gulf between privileged and deprived classes in traditional China, and scope of parasitological problems, and social obstacles to the introduction of Western medical techniques. Subsequent chapters are devoted to the policy and strategies of public-health organization in present-day China; social-psychological aspects of public-health reform and mobilization; the sources, training, and roles of health manpower; the present status of disease control, nutrition, and preventive care; and population planning.

Chu-yuan Cheng's paper, "Health Manpower: Growth and Distribution," points up the difficulties of data collection and interpretation which inevitably tend to impose a conjectural quality on studies of the People's Republic. This problem, which in one form or another is evident in many of the contributions, is explicitly discussed by the editors in several contexts.

An especially valuable feature of this volume is the annotated bibliography, which provides citations and rather detailed abstracts of the accessible literature in Chinese, Japanese, English, and French during the period 1949 to 1971.

Medicine and Society in China begins with a summary outline of Chinese traditional medicine, its theoretical basis, and its preservation in classical texts. The following chapters discuss specific phases in the introduction of Western medicine: early Chinese contacts with Portuguese medical men, cultural problems and the confrontation of Chinese and Western medicine in the nineteenth and early twentieth centuries, the role of American medical educators in China, the development of health services in the People's Republic, and the place of public health as an issue in the Chinese political environment.

Robert Bridgman's "Traditional Chinese Medicine" will supply the non-sinologist reader with some basic notions as to the native Chinese medical tradition and the truly formidable textual, linguistic, and philosophical problems involved in any scholarly study of this material. Carl Nathan's "The Acceptance of Western Medicine in Early 20th Century China" provides some fascinatingly concrete details on popular attitudes and prejudices in the China of several decades ago. This paper and several of the other contributions help to develop insight into specific problem areas in cross-cultural relations, and into the remarkable interrelation of scientific, cultural, and political factors.

The editors might have been more careful in their treatment of Chinese names and terms. In an English-language context, some of the spelling conventions are simply incorrect and should have been avoided (e.g., "K'ong Tsi" for Confucius, "Sze-ma Ts'ien" for Ssu-ma Ch'ien); and no one system is used uniformly throughout the book.

The list of participants to the two conferences reveals the broad interdisciplinary basis of these volumes. Included are sinologists, physicians, historians, political scientists, sociologists, economists, psychologists, demographers, librarians, and others. The group includes a good many scholars with specialized research experience in Chinese matters; some were able to supplement their material with remarks based on personal travel or residence in the People's Republic.

The only real drawback of both these collections is, in a sense, the very problem to which they are addressed: the frustrating lack of normal information flow between China and the West. Time and again readers will wish a more definite picture could be drawn of the subjects discussed here; but the papers themselves will assure readers that it cannot realistically be done. Given the present state of affairs, as general introductions these books are probably as good as can be had.

LLOYD HAFT

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Electoral Politics in the Indian States—Volume I: The Communist Parties of West Bengal. Edited by Myron Weiner and John Osgood Field. (Delhi: Manohar Book Service, Distributed in the U.S. by South Asia Books, 1974. Pp. xvii + 158. $9.00.)

This volume represents the first published fruits of the massive Indian elections data analysis project that has been in progress for a number of years at M.I.T. under Myron Weiner and his colleagues. At least three more volumes are to follow this one.

The current study consists of a monograph of 68 pages and an appendix of 90 pages, in which detailed results are given for all elections to the West Bengal Legislative Assembly since 1952. This balance may appear to be a bit short on the analysis side, but in fact 68 pages give the authors ample scope for an exhaustive treatment of their topic, the electoral performance of the two communist parties of West Bengal over the 1967 to 1972 period.

The focus of inquiry is an interesting one for several reasons. First, the time period was one of unprecedented success at electoral politics for the Indian communist parties, both at the polls and in forming ministries at the state level. Second, the context was a most unusual one in that four elections took place in West Bengal within a five-year period, all with the same constituency boundaries (usually these boundaries are changed after every second election); thus all four elections could be compared directly with each other. Third, the authors bring to their subject an intimate familiarity with Bengali culture and politics; it can reasonably be expected that little of importance in the bewildering world of Bengali radical politics will escape them.

The central theme of the study is the changing electoral fortunes of the Moscow-oriented Communist Party of India (CPI) and the more leftist Communist Party of India-Marxist (CPM), as they moved into and out of alliance with each other and the Indian National Congress. The authors develop a number of useful taxonomies and typologies to sort and classify the various constituencies in the context of the different elections. It is doubtful that anything more in this line could be done that would be of use or interest.

The comparisons are all between parties, elections, and constituencies. The authors did not work with survey data, nor did they include any socioeconomic data in their analysis. They report that when they did use such election-related data as turnout, number of contestants and the like, they found no significant relationships. The study, then, does not relate election behavior with what is outside the electoral universe, but rather focuses on explaining voting patterns in terms of the world of parties, electoral alliances, and the voting results themselves. The statistics employed are those of the percentage table, so the analysis is quite easily understood.

The results of the analysis will not be surprising to students of Indian politics—that the two CP's did better at the polls when acting together than when fighting each other, that when one of them (the CPI) ganged up with the Congress, the effects were hard on the other, and that both but particularly the CPM were able to build up core support areas where they were able to pull in a reasonably steady vote in election after election. It is good to have these findings nailed down, though, in a rigorous quantitative sense.

The authors' ultimate goal is to delineate the normal or "institutionalized" support each of the CP's can expect in a West Bengal election. They find themselves unable to pin down this level of support for any party, largely because the "exact nature of the party's standing with the electorate remains unclear" (p. 27). The answer will lie in more analysis of future contests. "In short, several more elections—and much more research—will be required," before dependable party support can be determined (p. 38). What they seem to be saying implicitly is that underlying the shifts that take place from one election to another, there is a "normal vote" that each party can count on; that is, there is a basic allegiance toward party on the part of the electorate—or a large part of it, at any rate—from which actual voting results represent deviations. Explaining election behavior would amount to accounting for the deviations.

It would appear to this observer, however, that a "normal election" is a chimera, particularly in the Indian context. There has been much heated debate over whether the "normal election" concept is useful in analyzing American voting history, but for India the evidence would appear heavily against the notion's having any validity, especially in volatile West Bengal and most especially during the period covered by this study. Each election has been in a very real sense *sui generis;* there really is no "normal election," either behaviorally or as an abstract entity, so it is not a surprise that the authors fail to find the "normal vote."

HARRY W. BLAIR

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Inward Hunger: The Education of a Prime Minister. By Eric Williams. (Chicago: University of Chicago Press, 1971. Pp. 352. $7.95.)

It is rarely the case that Third World political leaders are able to find the time to write a series of scholarly books which are both informative in a general way as well as instructive as to factors which influence their policy directions. This book by Eric Williams, the Prime Minister of the Caribbean English-speaking state of Trinidad and Tobago, represents a further effort in a series of historical and political works. This work provides a clue as to why this scholar-politician continues to write in spite of the pressures of office which, as recently as 1970, almost culminated in the overthrow of his regime by an army mutiny and mass riots activated by Black Power militants. Dr. Williams remains an academic searching for acceptance and full recognition by the Western academic establishment in spite of having occupied the role of leader of the government of his country's parliamentary democracy for a continuous period since 1956 and the pre-eminence of being the major architect of nation building in Trinidad in the postwar period.

The book itself is rich with attitudinal and behavioral data with which to grasp the overall process of socialization which shaped the ideological and political development of Caribbean colonial intellectuals in the postwar period. It mirrors the deep assimilation of Western European values as well as the ambivalence and sense of wounded rejection that comes from exposure to the ethnocentricity and double standards of the colonizer.

It reveals the essence of the conservative chatacter of early postwar middle-class nationalism in the Caribbean region, tied as it was to a Western European frame of reference and a deep distrust of the creative culture and capability of the non-European Caribbean masses. Fundamentally, it is a nationalistic perspective that makes power claims on behalf of the intelligentsia but expresses great doubts about the viability of the Caribbean societies in whose name and interest the calls for decolonization were articulated.

Williams reveals himself to be the classic colonial intellectual in the way in which he assumes the mantle of political leadership as maximum teacher and high priest pointing the way forward for the society. He presents no vision of the future society to be built or claim to be articulating the indigenous stirrings of the people of Trinidad and Tobago. Nowhere in his writings is there the semblance of a belief in a Trinidadian or West Indian path to development. Instead, he claims the mantle of teacher and maximum leader to enlighten the masses in the ways of the colonizer. In other words, he becomes by virtue of his socialization an instrument of British upper middle-class cultural imperialism. It is therefore no surprise that this distinguished scholar-politician became the target of attack from the more recent nationalist stirrings generated by the first wave of indigenously-bred middle-class nationalists produced by the University of the West Indies.

One fact that is made clear in this work is that in spite of being a victim of cultural imperialism, Williams has no illusions about the benevolence of the Western metropolitan powers, particularly Great Britain and the United States. Indeed he articulates a view of the need to preserve national and West Indian self-interest in the face of the predatory intentions of the major Western powers which is ideologically consistent with his earlier historical work exposing the economic basis of both slavery and the emancipation movement in the U.K. and expounding on the fascinating thesis of how the slave colonies contributed to early British industrial development. Where Williams falls short is in the failure to follow through on this critique to develop a vaible strategy of development outside of dependency on the major capitalist powers. This partial and incomplete view of the economics of decolonization reveals itself in this work as rooted in his antisocialist ideological tendency and a view of Trinidad and Tobago and the Caribbean as necessarily tied to the path of Western capitalist development and by that very principle limited to a satellite status vis-à-vis major powers like the United States.

The work is written with a frankness and openness in revealing motives, emotions, and gut feelings that give the reader a close view of the inner forces motivating and influencing Prime Minister Williams at critical points in his academic and political careers. In this respect the book represents a rich source of data on the sociology of Third World political leadership that should be of interest to political scientists who may have no particular specialization in Caribbean political studies.

CARL STONE

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Population, Politics, and the Future of Southern Asia. Edited by W. Howard Wriggins and James F. Guyot. (New York: Columbia University Press, 1973. Pp. 402. $8.50, cloth; $2.95, paper.)

This book is the product of a 1971 conference on population and politics sponsored by Columbia's Southern Asian Institute, with some additional material and updating. The contributors, from several countries and disciplines, agree that population will continue to grow at high rates in most of Southern Asia for the rest of the century; population movement, into cities and into areas dominated by hostile peoples, will continue to create tensions; demands by the rapidly growing and increasingly "modernized" (the term usually used in the book) youth cohorts will increase and strengthen; and there is very little optimism that governments will be able to achieve the great changes seen as necessary to handle these processes. Thus, the prospects for political stability and successful "modernization" are dim.

As usual, generalization on a more specific level is not achieved. The contributors emphasize the limits of their work, and particularities are evident. Migration in India and Pakistan, described in excellent papers by Shahid Javed Burki and Myron Weiner, differs from migration elsewhere because of the post-Partition upheavals. The famous 1941–51 "notch" in Indonesian age cohorts is unique (and, Nathan Keyfitz demonstrates, partly illusory). The editors attempt in an introductory article to draw the studies together, but the patterns they identify are so broad and elementary that I was led to wonder what audience they had in mind.

Still, this is a very valuable book, full of information and stimulating suggestions and conclusions. The approaches are various, including analysis of census data, survey research, and nonstatistical reports based on the contributors' experience. Political scientists will find the demographic material useful, and will benefit from cautions about the use of data and the acceptance of popular wisdom. For example, Weiner shows that a significant portion of internal migration in India is composed of women traveling for marital or child-bearing purposes. Sidney Goldstein, finding that two-thirds of the population increase of Bangkok during the 1960s was natural, suggests that Bangkok may be an example of a kind of maturation of Third World cities. He also finds that 10 percent of those born in Bangkok lived elsewhere in 1960, which is unexpected. Goldstein and Aporodicio Laquian provide evidence for the proposition that shanty-town dwellers are more likely than not to be satisfied with their living conditions, and often actually prefer shanty-town life to that in low-income housing developments.

Studies of Asian population always provide some chilling illustrations, and there are some good ones here. "Ceylon is often cited for having reduced mortality in a single year by an amount that in Scandinavia took 100 years" (p. 37). Vincent Whitney and Gavin Jones estimate that the world population is growing at a rate of 2.1 percent annually, in contrast with a rate of less than 0.5 percent before the Industrial Revolution. Keyfitz estimates that the provision of modern-type employment for the youths entering urban areas in Java alone would require an annual investment equivalent to one-sixth of the total present national income of Indonesia. Howard Wriggins and C. H. S. Jayawardene report that in 1969 Sri Lanka's universities produced 100 arts graduates for every graduate in the agricultural sciences. Several of the authors mention the potential "avalanche" of urban migration that would result from a modest increase in the proportion of rural youth who move to cities.

The papers can be summarized only very selectively. In the first part of the book, dealing with general population trends, Whitney and Jones point out that serious governmental efforts to limit population growth began only in the mid-1960s, and that so far real reductions in growth rates have occurred only among Chinese inhabitants of the region, in Singapore and West Malaysia. They offer tentative predictions of population at the end of the century: more than a billion in India, 250 million in Indonesia, 75–100 million in the Philippines and Thailand. Goldstein helps to explain the lure of the cities when he estimates that average family income in Bangkok is three times higher than rural family income. Burki uses an ingenious comparison of census data and arrest records to suggest that the erosion of support for Ayub Khan had important roots among migrants from land-owning families in the towns, as opposed to the cities, of West Pakistan. Burki provides illuminating and plausible, if sometimes over-generalized, observations on the differences among cities and among migrant groups. Weiner surveys intercultural migration and "nativist" reaction, finding that hostility to in-comers is almost completely confined in India to middle-class migrants; working-class migrants are ignored (note the difference in Bangladesh). Laquian's paper, mostly a parade of stereotypes about cities and trouble, is the least satisfactory contribution, but it does offer some information about the ethnic basis of Manila street gangs and the attitudes and conditions of slum-dwellers.

The second part of the book concentrates on the burgeoining youth cohorts. Keyfitz shows the extent to which migration is a phenomenon of youth: overwhelmingly, at least in Indonesia, people who move are around the age of 20. He points out, too, that the decine of the rural birth rate in Europe as land became less available has not been paralleled in Java, because in Javanese communal ownership patterns larger families were provided with more land, and also wage labor on sugar plantations was plentifully available during the colonial period. Donald Emmerson draws on limited survey data for some tentative conclusions about differences of attitude beteween Indonesian officials who identify themselves with the revolutionary generation and the present student generation. He argues that these attitudinal differences do not necessarily bode ill for political stability in Indonesia (I found his argument a bit confusing). He also challenges the view that ethnicity is of major importance in student activism. Cora Du Bois admirably summarizes the ills of formal education in India, showing that education is highly valued in the abstract and strongly disliked in practice by everyone involved. It is a hollow system that in nearly every particular works against its own goals. Wriggins and Jayawardene analyze what may be the most surprising event in the region in recent times, the 1971 "youth rebellion" in Sri Lanka. They point to underlying causes (rapid growth of population, stagnation of the economy caused partly by over-spending for welfare in response to politically expressed demands) and precipitating events (organization of the disaffected in rural areas of unprecedented effectiveness). The paper seems more convincing, though, in its explanation of the improbability of the event than of the event itself. This is not criticism of the authors, who do as well as could be done, but another appearance of the old problem of the difficulty of explaining complex events with limited and questionable data. In a concluding note, Kenneth Thompson wisely stresses the difficulty of dealing with the population problem, observing as an example of its complexity that the difficulties of life in an "isolating and insecure" modern world might increase the importance of the family for many people and encourage the maintenance of the birth rate.

The tone of analysis of the Third World has changed in the past 20 years from heady technocratic confidence to discouragement and even despair. This book provides more documentation of the darkness of the prospects. And yet—these grim conditions have prevailed now for some time, the revolution of rising expectations was identified a quarter of a century ago, the gap between rich and poor grows steadily and visibly. But still most people do not starve and do not die violently; most political leaders enjoy a reasonable longevity in office. Is it possible that our data data and our impressions overlook a human capacity to survive and, if not to prevail, at least to cope?

WILLIAM S. HARDENBERGH

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The Systematization of Russian Government: Social Evolution in the Domestic Administration of Imperial Russia, 1711–1905. By George L. Yaney. (Urbana, Ill.: University of Illinois Press, 1973. Pp. xvi + 430. $13.50.)

This is a book about government efforts to make contact with and exert some control over Russian society. While this may seem a paradox for an absolute, centralist, autocratic regime, George Yaney comprehensively demonstrates that isolation of capital city society and the central bureaucracy was a critical political problem in Russia and that solutions were forthcoming only with the greatest difficulty. The study's chronological purview extends from 1711, the year of establishment of quasi-representative government. In one sense, therefore, *Systematization* is a contribution—and an important one—to the small but growing literature on Russian administration which has been heavily influenced to good effect by the Soviet historian, P. A. Zaionchkovsky. Considered at a merely descriptive historical level, Yaney's effort is impressive. Students of administrative development and the history of Russian politics will find new material on both the central and local governments. For, although Yaney has not made the imaginative and extensive use of archives characteristic of S. F. Starr's *Decentralization and Self-Government in Russia,* in my opinion he more than makes up for this by what appears to be total command of printed and microfilmed material—works by Russian scholars from the eighteenth century through the 1960s; works, including unpublished doctoral dissertations, by Western scholars; the relevant legislation, "official" histories, and so on. If one wishes to understand the basic structure and operation of central and local Russian government, especially in the nineteenth century, a careful, patient reading of Yaney's chapters 5 through 9 will be rewarding.

In addition to being a historical description of the "evolution" (Yaney's word) of Russian government, *Systematization* is a study of development administration which is quite remarkable in its breadth and depth of perception. The significance of the author's perspective is manifest: "The Imperial Russian Government was the first in history to attempt—albeit inadvertently—to introduce radical social change into a backward society. Its servitors were the first men to sense the necessity of forcibly uprooting social order to preserve political order" (p. 398). The fact that the imperial servitors failed in their effort has, for too long, obscured from many historians and students of political development the more important fact that their effort gave scope and character to the development of Russian government, literally for centuries. Mr. Yaney's capacity to give voice to this phenomenon, to describe and analyze it, has created a work which is conceptually self-conscious (still unusual for the work of a historian) methodologically imaginative, as well as rich and provocative in its language. This effort gives much added interest to the book but, inevitably perhaps, it has resulted in a prolixity which has made *Systematization* highly controversial since its appearance.

Yaney's notion of the evolution of domestic administration is dialectical. That is, he is constantly at pains to identify, describe, and explain the significance of the nodal point at which the political-administrative ideal is organized into contact with society. Contact creates, in turn, a synthesis of social-administrative interaction which works in spite of the fact that no one predicted it and no one really understands it. In his effort to explain the behavior of individuals in the context of this evolutionary process, Yaney makes effective use of the notion of myth, which he understands in Benedict's sense as "a wishful projection of a universe of will and intention. . . ." Again and again Yaney notes that both observers and participants in the evolution of Russian administration have been handicapped by distorted vision of the Russian historical experience. "No regime in Russia should be measured against the liturgical formulas that its servitors and supporters repeated. Russian government in all eras should be comprehended against a background of the problems with which it had, consciously or unconsciously, to cope" (p. 318). These formulas made up a large part of what Yaney refers to as capital-city government's myths about Russian politics and administration. His point is important and valid, I believe, for Russia: to the extent that Russian political theorists and statesmen evaluated policy in terms appropriate to another (Western) culture, they were so far unlikely to evaluate themselves and their society with the necessary degree of realism.

In spite of exertions toward a synthesis of the sociology and politics of administrative development, *Systematization* underscores rather than erases the difference between alternative disciplinary approaches to the history of social change. Nowhere, it seems to me, is this more clearly evident than in the application of the concept of "system" itself. One searches in vain—either in the text or the large bibliography—for reference to Easton, Deutsch, Forrester, or Parsons. Mr. Yaney may have fallen prey to his own mythos—in this case a peculiar idea about the nature of system in society and politics. For him system appears to be that kind of administrative behavior which is regular enough to be predictable by the development administrator. Thus "systematization" becomes something that can gradually overtake the interactions between state and society, an alternative to some previously existing set of relationships, a characteristic of increasing sociopolitical cohesion, and a kind of bureaucratic-political mobilization of a backward society. Does this argue that traditional behavior of peasant society (as a society and a polity) was nonsystematic? Evidently, in spite of the fact that such a conception would create considerable problems for a social- or open-systems theorist. Such use of a basic concept seems needlessly idiosyncratic.

Systematization is an impressive, provocative work. Inasmuch as it raises fresh questions as readily as it answers others, it cannot be regarded as definitive. Because it sets out to do so much, its real achievements—in descriptive, synthetic history and in analysis—may be overlooked. Nevertheless, in my opinion the work has established a high standard of historical research in development administration against which any subsequent effort will be measured.

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International Politics

Foreign Policy and Federalism: The Nigerian Experience. A. B. Akinyemi. (Ibadan, Nigeria: Ibadan University Press, 1974. Pp. 217. $6.00, cloth; $3.00, paper.)

This short new book is another product of B. J. Dudley and E. U. Essien-Udom's excellent Ibadan social science series and consists of a number of case studies on Nigerian foreign policy from that well-trod ground of the early 1960s. If the material, mostly from the lively Nigerian press, produces no surprises, it offers new insights on problems considered in many other volumes.

Ostensibly Akinyemi's book is a "study of the pressures operating on the government of a federation—Nigeria—on specific issues of foreign policy" (p. 2). If only to shed light on how pressure groups (in this case regional states and parties) influence foreign policy in Third World countries, such an objective would be worthy. The real burden of the book, however, is to affirm that, because of such pressures, "a dynamic foreign policy is almost impossible in a federation" (p. 199). Certainly in the first Nigerian republic such was the case, though one could hardly generalize much beyond, since the pertinent point is less the fact of federation than the fact of the diffusion of power. The USSR is istensibly a federation and has a dynamic foreign policy.

But there is little mention of the fact that pressure groups have either disappeared, or have had to operate in a fundamentally different manner, in Nigeria as in much of the rest of Africa since the early 1960s: the object now is to find favor with military factions; and one does not organize public conferences to do this, as radical students did in 1961 to affect Nigeria's Africa policy. A book such as this one, on the *second* republic, would have to find a new kind of source.

Akinyemi misses this point because he is really trying to prove another one, not foreshadowed in the introduction. This is to discredit the pragmatists who directed the foreign policy he studies. His evidence leads him to note their success, for example, in creating something of a "Pax Nigeriana" with the founding of the OAU. (This puts it a bit strongly, since Nigeria was left out of the central elite originally running the OAU, though it properly focuses on Nigeria's contribution to the climate of moderation that made possible so much progress in African international relations.)

But Sir Abubakar Tafawa Balewa, the architect of Nigeria's foreign policy in this period, lacked vision, it is argued. He could merely "advance practical solutions" (p. 109). But as Nigeria and Africa were "boiling volcanoes threatening to spew out destructive ashes of revolution" (p. 201), radicalism and idealism, for reasons not specified, were called for. Sir Abubakar was therefore "out of touch with the realities of the Nigerian and African political systems" (p. 210), as the book concludes. Or was he? Those pressures had an outlet, and one is bound to wonder whether the author's rage would not be better directed at leaders who have proved intolerant of those pressure groups that are no longer around to be studied.

One other point. It is heartening to see the initiative in such studies passing from the hands of Western social scientists who once held a monopoly on it, to Africans themselves. It is less encouraging, however, that a book like this, which goes over ground so thoroughly plowed elsewhere, acknowledges so little debt to the writers whose work, on internal evidence, helped him focus his questions. In some cases, sources have been left wholly unmentioned; in others, only the most cursory reference is made—unless, of course, the Western writer is perceived as being wholly in agreement. This makes for little dialogue of a scholarly sort.

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The Causes of War. By Geoffrey Blainey. (New York: The Free Press, 1973. Pp. x + 278. $7.95.)

War Without Weapons: Non-Violence in National Defense. By Anders Boserup and Andrew Mack. (New York: Schocken Books, 1975. Pp. 194. $6.50.)

These two volumes on war at first are seemingly different, but upon closer examination, turn out to be quite complementary. While the book by Blainey treats the origins of war in the modern period, the Boserup and Mack volume analyzes alternate nonviolent strategies in response to a frequent consequence of war, namely, invasion by a foreign power and occupation. One analysis is in the mode of explanation; the other is more in the realm of policy formulation in the area of strategy and tactics. As a result, these treatments stand at the two ends of a continuum beginning with the origination of international violence and ending in a major possible response in the form of nonviolence. A point of interest here is that Blainey is an Australian economic historian while Boserup and Mack are European conflict and peace researchers. Both books, then, provide a somewhat different perspective on war from that normally seen by an American social scientist.

The Blainey volume is the more historically oriented of the two, as one would expect, given the greater number of wars historically than nonviolent responses. Most of the instances of war are in the modern period stretching roughly from 1700 to the present. The analyses are not systematic in the sense of formal theory building and hypothesis testing, nor is there a sequential building of a nonquantitative analytical framework. Rather, much of the book examines and then rejects many of the commonplace notions held about the causes of war. For example, the concept of international prosperity as a deterrent to war is effectively treated in the detailing of serious instances of wars which were begun in times of widespread relative prosperity (e.g., World War I). The related expectation of peace under conditions of trade, tourism, and intermingling of cultures is effectively countered not only by the example of World War I, but also perhaps by the American Civil War. Blainey resurrects Macfie's argument that it is in times of relative prosperity and high expectations for the future that wars break out.

Other assumptions attacked by Blainey are that the balance of power leads to peace and that "Pearl Harbors" are all that exceptional in the history of war. Blainey argues rather convincingly that the balance of power is more likely to lead to war, if only that a less decisive condition exists within a state of balance and that a particular country might be tempted to initiate a confrontation. The relatively lengthy peace after the end of a long war in which the victor is shown to be clearly more powerful than the vanquished, serves to illustrate the dangers of the balance. Blainey does an effective job of enumerating the many instances of surprise attacks or declarations of war after the battles had begun, to counter the usual assumption of the rarity of surprise attacks such as Pearl Harbor. Perhaps it was the massivity (and success) of the Japanese attack rather than its timing which led to the subsequent unsavory reputation. In any event, it is commonplace assumptions such as these which are examined rather effectively by the combination of verbal analysis and use of historical example.

Some surprising and rather useful information is brought forward by this approach. The death-watch-wars, for example, or those begun immediately after the death of a monarch in the eighteenth century, may be an instance of an immediate power loss (uncertainty increase) which leads to the initiation of war at the moment of the monarch's death. This may be another instance of a power loss leading to war, much as an alliance between a major power and a smaller one has been found by this reviewer in his own volume on war to yield a power loss as an uncertainty increase which results in war. In addition to the onset of war, Blainey also deals with the duration and extensity of war. Wars occurring at the power center of Europe are found by him to be longer than the colonial or other shorter wars occurring at the periphery of Europe.

In contrast to the loose, generally anecdotal style of Blainey's book, the Boserup and Mack volume is more self-consciously analytical and systematic. Beginning with a general overview of treatments of both positive and negative forms of nonviolence—the former designed to win over the opponent and the latter aimed simply at thwarting the occupation—Boserup and Mack opt for the latter. The remainder of the book is devoted to the application of several theoretical frameworks to the strategy of nonviolence.

One such framework is that of guerrilla warfare in which the militarily weaker side adopts strategies which avoid the opponent's strengths and aim at its weaknesses. This is said to be one strategic model for nonviolent defense. Group theory is used to show the importance of group cohesion in the effective use of nonviolent tactics against the occupiying force. Clausewitzian strategy is also applied to demonstrate the importance of the defense in any military strategy including nonviolent ones. Indeed, according to Clausewitz, the mode of defense will determine the mode of offense, and the authors use this to show how an appropriate nonviolent defense can force the occupying armies to adopt strategies consonant with the aims of the defenders. Examples are drawn from the twentieth-century European experience such as the Ruhrkampf of 1923, the Czechoslovak resistance of 1968, and nonviolent resistance to military coups as in the Kapp Putsch of 1920, or the French generals' revolt in Algeria in 1961.

However, despite its virtues, there are several weaknesses to this treatment. First, the Clausewitzian emphasis on the defense as a justification for the use and possible success of nonviolent tactics ignores the interactive quality of international warfare. This is especially the case when the occupying power and nonviolent resistance are frequently in conflict over a relatively long period of time. There are frequent "battles" as it were, and this allows invaders to modify their strategy when faced with a temporarily successful instance of nonviolent defense. Perhaps this explains why the Czechoslovak resistance ultimately foundered, even after a successful initial high level of cohesion among the Czechoslovaks. By the making of apparent concessions and the establishment of alternate sources of legitimacy within the country, the Soviets managed to defeat the resistance within a relatively short time. It is precisely because of the time allowed the invader to engage in prolonged interaction with the nonviolent resistance on the occupier's own terms that foreshadows an eventual demise even for an initially successful effort.

Another problem is the almost exclusively European domain of the analysis. The authors are heavily influenced by the events of the Second World War and the recent Czechoslovak invation and occupation. There is little if any treatment of resistance to colonial authorities in the now independent Third-World Nations, or responses to military coups in these countries.

Despite this emphasis on the recent European experience, there is a surprising and notable omission from the overall treatment. This is the assumption concerning the willingness of the occupying power to subscribe to certain moral standards of conduct. If, as in the case of the Nazis during World War II, the occupying power is willing to engage in the destruction of entire population groups such as the Jews or to destroy whole villages as in Czechoslovakia, then the nonviolent enterprise obviously fails.

In comparing this volume on nonviolence with the Blainey treatment of the origins of war, one arrives at a somewhat anomalous conclusion. While the Boserup and Mack book is much more theoretical, analytical, and conceptually more systematic, the Blainey volume seems to have a more solid set of conclusions. Indeed, these are included in a separate concluding chapter which can serve as a set of hypotheses for future research. One wonders why a book which is essentially historical-anecdotal and much thinner analytically would arrive at a more substantial set of findings than a more systematic one. The answer would seem to lie in the Problem of N. There have been all too many instances of war in the past two to three centuries, while the cases of nonviolent defense really have only occurred with some regularity in the twentieth century with the rise of large-scale civilian participation in war. As a result, an economic historian such as Blainey who obviously has immersed himself in the various histories of war would have a much larger data base to work with. Boserup and Mack had to rely on a few contemporary illustrations and some considerable theorizing to arrive at their set of conclusions.

Because of the use of a largely theoretical social scientific approach, most American social scientists probably would be more comfortable with the Boserup and Mack treatment. Their concern with a systematic development and the extensive use of concepts such as legitimacy, polarization, and behavioral contagion, lend a more familiar quality to their analysis. Blainey, on the other hand, hardly uses concepts such as these and, in addition, the use of chapter headings such as "Paradise is a Bazaar" or "While Waterbirds Fight" seems to be a rather unnecessary literary indulgence. Nevertheless, the extensity of Blainey's familiarity with instances of war and some little-known (in America) but intriguing observations on war, make the book worthwhile reading.

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In Search of American Foreign Policy: The Human Uses of Power. By Lincoln P. Bloomfield. (New York: Oxford University Press, 1974. Pp. 173. $6.95, cloth; $1.95, paper.)

The author sees himself standing with one foot in the camp of American foreign policy makers and with the other in the camp of their critics. The critics are often wrong, but the notion that U.S. policy is not inspired by the "human perspective" has merit. The policy makers, he tells us, mean well and possess qualities of mind and character. Still, Bloomfield's disquisition conveys the impression that American foreign policy is continuously out of date and wrapped up in demagoguery. Intelligence estimates are mishandled and policy tends to be based on "best case" hopes. Furthermore, the State Department's long-range planning functions have been curtailed drastically.

I am afraid the roots of the disaster run deeper. Methodological ignorance of what should be considered—that is, reductionism—has been a potent cause of our troubles. The State Department, the Pentagon, the National Security Council, and the White House are continuously operating on images of truncated reality.

Bloomfield himself is an accomplished practitioner of the reductionist art. For example, he argues military power is relevant only to "some" national strategies: but who would fight international inflation with tanks? He asserts that the Soviets define the protracted conflict with the United States as not "primarily military." But what role does their conflict doctrine assign to military power? He thinks the risk of nuclear war cannot be tolerated indefinitely, but can the risk of the human condition itself?

Bloomfield talks about arms reduction in terms of naive variations on the overkill theme, somewhat like discussing modern medicine as though microbes and microscopes did not exist. He refers to the need for "agreed verification procedures," but hides that the Soviets refuse even token verification. He proposes unilateral American steps to get arms reduction moving but forgets that the United States already has been following this prescription with painful effects.

Bloomfield knows the USSR is America's foremost security problem. Yet he treats this subject with massive doses of reductionism. Analysis of the USSR is a function of the analyst's position in U.S. internal politics. The sovietologists know "too much" (p. 69). Threat assessments are mutually influenced by mirror images propounded by rightwingers in the United States and "ideological purists" in the USSR. That the leading Soviet "purists" belong to the USSR's top leadership group and that purism has marked the announced policies of the CPSU for nearly 60 years remains as unmentioned as the fact that right-wing analysts hold no power in the U.S.

After minimizing the Soviet threat and dismissing those who would describe it realistically, Bloomfield avers that the Soviet regime is "blindered," "terrified by open criticism, . . . brainwashed by its own propaganda, and . . . hopelessly immobile," "caught in the dilemma of wanting to loosen up but not knowing how, . . . fearing above all to lose control over its empire," and trying with all its might and skill to avoid "all-out war and all-out peace." That the Kremlin communists *also* work effectively for the world-wide victory of Communism is eliminated from this caricature of bewildered men.

"Common values are largely absent" in the U.S.-USSR relationship. Yet the U.S. "must work hard toward better relations." "Common values will flow from shared experience" and may result in "feelings of community." Together with the USSR, we should establish "spheres of abstention" all over the world and "shape structures and relations that provide a framework for evolution." Bloomfield evidently believes in politics as the art of the impossible.

"Common interest abound." Common interests of the U.S. with *whom?* With the kolkhozniki or the industrial managers? The ruling purists in the politboro or the Young Turks in the Central Committee? Or of presidential candidates with Messrs. Andropov, Grechko, Ponomarev, and Brezhnev?

Pending the emergence of a Soviet policy of abstention, the U.S. must not prop up "undemocratic tyrants" within its spheres of influence. America likes to bed down with purveyors of snake oil democracy, such as the Greek colonels, Haile Selassie, and Ayub Khan, and supports countries whose last "free election" Bloomfield can't remember. While waiting for free elections and the overthrow of "undemocratic tyrants," should we ignore the Arab world, and most of the governments of black Africa, Latin America, and Asia? And if tyrants are off limits, how can we develop common interests and improve relations with the worst of the species? Will the tyrants in the Kremlin help us to humanize the use of power?

"The words of liberalism remain, but the spirit was betrayed by both men and events." What is this liberalism which can't be fitted to the facts? It is support of "change, nonviolence, and rational discourse." Change isn't always improvement; nonviolence rarely stops aggression; and rational discourse seldom is relevant in political negotiations. So why are those liberal goals?

To rescue liberalism Bloomfield proposes a "reinvigorated" approach. The following "value premise" should override all interests "except actual survival": "Human beings who live in this country—and for that matter everywhere—represent the irrefragably highest value for American policy." Indeed, in Soviet-ruled Eastern Europe the U.S. sought stability "far too often at the price of democracy or human rights."

This premise is very persuasive. But U.S. policy can be humane only to the extent that it promotes the full enjoyment by all of human, civil, and political rights wherever those rights are most harshly repressed. If Bloomfield were serious and consistent, he would argue that human rights within the USSR should be a primary and obligatory U.S. concern.

At this crucial point, the reinvigoration runs out of vigor and the goal disappears in the fog. Instead, Bloomfield makes the priceless recommendation that "a reformed foreign policy" should "design around obstacles." In other words, the U.S. must not face up to reality. As Shelley said: "From the contagion of the world's slow stain, he is secure, and now can never mourn . . . a head grown grey in vain."

STEFAN T. POSSONY

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The United States and India, Pakistan, Bangladesh. By W. Norman Brown. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 462. $16.00.)

This third edition of the late Norman Brown's well-known study was updated to take account of the bloody emergence of Bangladesh as an independent state. The overall format is as before, with considerable emphasis upon the historical background to the contemporary scene. The book provides an excellent introduction for college students and others approaching the subject afresh. Because of the wide scope of the survey it necessarily cannot explore any facet in detail. Earlier reviewers praised the 'balance' achieved by Professor Brown in discussing issues which are still very much matters of controversy. This balance is maintained in the last revision; indeed, Professor Brown's approach, while always exact, was also unusually restrained.

The appearance of this work in a series entitled "The American Foreign Policy Library" always seemed somewhat fortuitous, and the last chapter on relations between the three South Asian countries and the United States serves to demonstrate how marginal and peripheral that relationship has been in the totality of American foreign policy. Norman Brown was concerned to emphasize American scholarly interests in the area, and their growth since the 1950s. Perhaps his manuscript was completed before it became glaringly obvious that these studies may be past their zenith in all but a few American universities. His last word was to warn against viewing South Asia from the United States in terms of American values and American interests. Americans should "take a genuine and intelligent interest in the people of the subcontinent for their intrinsic human worth rather than as so many millions of bodies to stand in support of American international policies." Professor Brown always sounded a humane, tolerant, liberal note. It deserves to be recalled when those values are fast fading from the South Asian (and the world) scene.

HUGH TINKER

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Pakistan's Foreign Policy: An Historical Analysis. By S. M. Burke. (London: Oxford University Press, 1973. Pp. 432. £6.30.)

Mainsprings of Indian and Pakistani Foreign Policies. By S. M. Burke. (Minneapolis: University of Minnesota Press, 1974. Pp. 308. Price not listed.)

These two volumes are the fruits of several years of research and reflection by a Pakistani administrator-diplomat-scholar who had had a long and distinguished career as a member of the Indian Civil Service, a senior Pakistani diplomat, and a professor of South Asian studies at a leading American university. *Pakistan's Foreign Policy* is by far the best volume on this important subject. *Mainsprings of Indian and Pakistani Foreign Policies* presents, in a more abbreviated way, much the same material relating to Pakistan, continues the story to 1974, and reviews the course of India's foreign policy, especially vis-à-vis Pakistan, as well. While the reader will have no difficulty in identifying the nationality of the author, the treatment of India is remarkably balanced, with some lapses. Compared, however, with the treatment of Pakistan's foreign policy, and especially of Pakistani views on foreign affairs, the material on Indian foreign policy is sketchy and rather superficial.

Each volume begins with a brief survey of the historical background, with emphasis on the "two closed systems" of Hinduism and Islam and on the "unbridgeable gulf" between them. The bulk of each volume consists of an essentially historical narrative. Three phases of Pakistan's foreign policy are discerned: the "nonaligned years" (1947–1953), the "aligned years" (1954–1962), and a period of reappraisal (1963–1970). Three phases for the combined discussion of the foreign policies of India and Pakistan are also described, labeled "the first moves" (1947–1953), "*panchsheel* vs. defense alliances" (1954–1958), and "warclouds over South Asia" (1959–1974), centering first around the Sino-Indian conflict and then around the Indo-Pakistani conflict.

The main theme in each volume is Indo-Pakistan relations. Important secondary themes are also discussed at some length, including Pakistan's relations with other Muslim countries, India's nonalignment, Pakistan's policy of "bilateralism" in recent years, and the relations of both India and Pakistan with the United States, the Soviet Union, and China. The 1971 crisis in South Asia is treated in a brief postscript in *Pakistan's Foreign Policy,* and more fully, but just as gingerly, in *Mainsprings.* The much-criticized "tilt" toward Pakistan of the Nixon administration is characterized as a "realistic handling of the South Asian conflagration."

As historical narrative the two volumes deserve high praise. They are, however, deficient in many aspects which many contemporary students of foreign policy regard as essential, such as the role of ideology (especially relevant in the case of Pakistan), geopolitical factors, linkages between domestic politics and foreign policy, decision making, the organization of the government for the conduct of foreign policy, interest groups, elites and the quality of leadership, styles in foreign policy, and economic aspects. *Mainsprings* does contain a chapter on Jawaharlal Nehru, stressing his "love for Kashmir" and "his other biases." Surprisingly, there is no parallel discussion of Jinnah or any other Pakistani leader, perhaps because no Pakistani leaders gave as much attention to foreign affairs or was as well known in international circles. If Professor Burke were revising this book today, he might find reason for singling out both Indira Gandhi and Zulfikar A. Bhutto for special attention for their leadership in foreign as well as in domestic policy.

In the final chapter of *Mainsprings* Professor Burke departs from his historical analysis to speculate on larger aspects of the experience of India and Pakistan since independence. He suggests that both countries have belied "high expectations" and have been no less timid than their foreign predecessors "in tackling socio-religious reforms." He offers some advice to the great powers, and raises a poignantly timely question about the prospects for democracy in India and Pakistan. "The great powers," he writes, ". . . can help India, Pakistan, and Bangladesh best by ceasing to treat the subcontinent as a part of the arena where they measure their respective capacities for world hegemony and, instead, join hands for the constructive purpose of assisting the three neighbors to compose their differences on a fair and enduring basis" (p. 243). The lessons of history, he points out, reveal that "no modern nation has achieved real democracy before attaining prosperity and literacy," both of which are conspicuously lacking in South Asia. "Democracy seems to be the delectable reward for successfully tackling the grim toils of national construction. Have India, Pakistan, and Bangladesh the capacity to reverse this established sequence?" (p. 242)

NORMAN D. PALMER

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Social Policy of the European Economic Community. By Doreen Collins. (New York: Halsted Press, 1975. Pp. ix + 286. $19.75.)

This book on the evolution of social policy in the European Community is strictly for the specialist. It is, in the author's own words, "essentially descriptive of the social tasks faced during the years from 1958 to 1972" (p. 39). The study is informed by a humane sense of the importance of social policy for the lives of European workers. It is also a comprehensive and well-balanced piece of research. In the final analysis, however, I found the book disappointing, because it is neither analytically revealing, nor of much policy interest beyond its wealth of descriptive detail.

The two parts of the book describe, respectively, the application of discrete portions of the treaty dealing with particular aspects of social policy (employment, wages and working conditions, free movement, and social services) and the more general efforts to harmonize national social policies and/or develop a distinctive European social policy. This approach is reasonably serviceable for the author's descriptive purposes, although it does lead to a certain amount of repetition as substantive policy problems reappear in different contexts. While on editorial matters, it should be pointed out that the writing is rather cumbersome and wordy, with a good many sentences of forbidding length and complex structure. The book concludes with a sober but not gloomy assessment of the Community's accomplishments and with a kind of agenda for the future.

The basic argument developed by Collins is that, while progress on social policy has been unsatisfactory, the Community has recorded some notable achievements. In addition to isolated successes (on free movement of wage earners across national borders, for example), she points to commission-sponsored studies which have provided a reliable data base and set the stage for extensive exchanges of views among interested parties. "Such work is not only the pre-condition for any effective development of social harmonization but is a contribution to a fundamental tenet of social policy, namely that a rational base for action may be established through greater knowledge and understanding" (p. 221). Collins is also pleased by recent signs that the Community is prepared to cease thinking about social policy as merely "a compensatory mechanism for damage done" as a result of economic integration (p. 226). She detects, instead, an increased willingness to see the human aspects of social policy as ends in themselves. Only this humane approach, Collins contends, is compatible with the Community's more sweeping aspirations:

> If it [the Community] is to level off on the plateau of a free trade area ... its social activities will find it difficult to be more than the marginal adjustments made necessary to cushion the changes its creation has brought. If, however, the Community is concerned to achieve political unity, it is inconceivable that it could operate without acceptance of the social functions of modern government which both express and give deeper meaning to the human group from which it comes (p. 248).

The main stumbling blocks to more rapid progress on social policy have been, as Collins sees it, the national governments. First, they failed to provide a clear enough mandate in the treaty and, subsequently, they have consistently thwarted the many initiatives undertaken by the Community's executive, the commission. The author is additionally mindful of other obstacles like the technical complexity of these matters, their sensitive ideological implications, and the uncertain melange of human aspirations attaching to social policy. In the end, however, the commission and particularly Commissioner Levi-Sandri emerge as the heroes and the member-states as the principal villains in this presentation.

I do not wish to take issue with these conclusions but would suggest that they do not take us very far. No serious effort is made, for example, to explain why success has proven so elusive in social policy while things have gone forward quite well in other matters. Even the standards employed by Collins for judging success and failure are open to question, because she does not consistently distinguish between formal agreement at the European level and actual policy implementation by the Community and/or the national governments. These analytic problems stem, I think, from the basic research plan which focuses almost exclusively on documentary sources. Typically, analysis begins with the relevant treaty provisions, deals next with commission proposals for carrying out treaty obligations, goes on to mention consultation with various bodies and the statements made by parties to the negotiations, and concludes with the final disposition by the Council of Ministers. Since virtually all of the research seems to come from documents and other more or less public sources, we never get behind the scenes to the forces that influence the positions taken in negotiations. When judgments are occasionally made about the relative importance of various factors in determining the final resolution, these judgments tend to be ad hoc—that is, without sufficient attention to systematic evaluation of the evidence.

My criticisms of this book, thus, go to its lack of conceptual rigor and its failure to penetrate the surface of policy analysis. In fairness to the author, I must repeat that she acknowledges the essentially descriptive character of her project. At that level the book succeeds well enough—being carefully researched and judiciously presented. For the reader trying to learn something about Community policy processes or about the politics of social policy in Western Europe, however, the book will not be very satisfying.

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Public Papers of the Secretaries-General of the United Nations. Volume 4: 1958–1960, Dag Hammarskjold. Edited by Andrew W. Cordier and Wilder Foote. (New York: Columbia University Press, 1974. Pp. 629. $22.50.)

The publication of Volume 4 of the *Public Papers of the Secretaries-General of the United Nations,* which covers the period 1958–1960 during the Dag Hammarskjold years, evokes a remembrance of happier times for the United Nations and its chief executive officer. For the papers in this volume represent the work of a secretary-general at the very peak of his personal prestige and influence, and at the pinnacle of the historical development of the office itself as an actor of some significance in international relations.

The political growth of the office of the secretary-general is well known. Begun in 1920 at the League of Nations as an essentially administrative function under Sir Eric Drummond of Great Britain, the office was quickly politicized. Sir Eric's position as a neutral behind-the-scenes adviser and mediator for governments in dispute was highly valued, primarily because of his skilled and imaginative diplomacy. From 1920 to 1933, Drummond built up the international civil service he had created, while simultaneously demonstrating that the office of secretary-general could be of political value to the world community, although without visibility or public notice of any kind.

Drummond's successor as secretary-general, Joseph Avenol of France, made no further contribution to the development of the office during his tenure from 1933 to 1940. Indeed Avenol weakened the structure well before the League collapsed in 1939–40. Avenol's sympathies for Germany and Italy were quite sufficient to reduce his influence, and his frequent indolence served to diminish the office still further. Sean Lester of Ireland, the League's third and last secretary-general, administered a caretaker operation in Geneva from 1940 to 1947.

Trygve Lie of Norway, the U.N. secretary-general from 1946 to 1953, continued the behind-the-scenes advisory and mediation functions of the Drummond years, but he further politicized the office by adding a more public dimension to the secretary-general's activities. Lie spoke out frequently before the General Assembly and Security Council on political matters; he took public stands on issues of critical importance to the international community, including the superpowers; he negotiated preferred positions in the public spotlight. In brief, he generated a highly visible political role for the secretary-general, while maintaining the international civil service created by Drummond.

Dag Hammarskjold, the brilliant Swedish economist and civil servant, developed the political role of the office to its highest point during his administration from 1953 until his death in 1961. Hammarskjold maintained the Drummond and Lie functions, but went further. His carefully constructed theory of the UN's proper role as a neutral intermediary with interests and policies somewhat independent of the great powers led to a conception of the office as an instrument with certain discretionary powers over significant U.N. activities, including peacekeeping forces, observation teams, and other instances of a U.N. presence between or among powers in dispute. He insisted upon and for a time gained acceptance for the notion that secretary-generals had an independent capacity, and might act on their own initiative even without instructions from the U.N.'s political organs.

Neither U Thant (1961–1971) nor Kurt Waldheim (1971–present) have been able to do more than hold the line where Lie had left it. Soviet and French resistance to the Hammarskjold conception, as well as the rapidly fading U.N. role in world politics, have combined to retract the office to more modest dimensions.

The 1958 to 1960 period covered by volume 4 of the *Public Papers* represents the peak just prior to the Congo affair in the early 1960s which finally resulted in the insistence by the Soviet Union, maintained ever since, that the secretary-general's role be essentially that of chief administrative clerk of the organization. In today's United Nations, perceived and utilized more frequently as an instrument of political pressure than as a neutral aide to dispute settlement, a minimalist conception of the office has supplanted the activist role of Hammarskjold.

Volume 4 of the *Public Papers* is a meticulously edited book containing speeches, press conference transcripts, diplomatic communications, official statements and other records detailing the work and views of Dag Hammarskjold from early 1958 until mid-1960 and the start of the Congo crisis. The period was marked by Hammarskjold's successful intervention in the 1958 Lebanon-Jordan affair, which included his enlarging the U.N. Observation Group in Lebanon despite a Soviet veto of a Japanese proposal that would have had the Security Council expand the mission. In 1959 Hammarskjold made a personal trip to Laos and installed a personal representative there whose real task, despite a different public explanation, was to assist in the neutralization of the country. These moves were a prelude, of course, to his crucial role in the Congo from mid-1960 until his death in September 1961.

Volume 4 is a valuable addition to the store of historical data on a key figure in the quest for a peaceful world order.

ARTHUR W. ROVINE

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Soviet Policy Toward India: Ideology and Strategy. By Robert H. Donaldson. (Cambridge, Mass.: Harvard University Press, 1974. Pp. x + 338. $15.00.)

One of the Soviet Union's most significant foreign policy successes of late has been its relations with India. This study by Robert Donaldson is a detailed analysis of the Soviet background to that success in the sense that it depicts the transformation of Soviet policy toward India from a policy mired in dogma to one grounded in a far more flexible conceptualization of India and of the Soviet role in South Asia.

It should be stated at the outset that Donaldson's study is less diplomatic history than intellectual history, for Donaldson is not prepared to dismiss—as so many have—ideology as merely a means of justifying any and all actions pursued by the Soviet Union. Donaldson sees a far more subtle, more complex relationship between ideology and strategy, one in which ideology—while admittedly often bent to accommodate the needs of strategy—nevertheless poses conceptual challenges to and limitations on Soviet strategy makers.

As the author views the evolution of Soviet policy toward India, that policy has gradually undergone a "de-eschatology" in its concern to adapt to a rapidly changing international power structure. In the Leninist and Stalinist periods, India was largely beyond the focus of Soviet foreign policy—although the Soviets were more aware of India and its problems than they were of other areas of Africa and Asia. As Donaldson shows, the problem for the Soviets during this period was not simply a lack of knowledge about India but, more importantly, a conceptual rigidity which caused policy makers to distort what information they had as well as their decisions as to the data they thought they needed. The result was a foreign policy which neither supported Soviet interests in South Asia nor assisted the indigenous Communist movement in India.

Soviet foreign policy toward India radically changed between 1955 and 1961, a change that Donaldson depicts in great detail. This transformation had little to do with conditions in India. Rather, Soviet attitudes toward India shifted as the Soviets began seeking Indian support for the intensifying "anti-imperialist" posture of Soviet foreign policy. Soviet willingness to accommodate the Nehru government grew as the USSR found itself in competition not only with the West but with China as well.

Donaldson's contribution lies less in his illumination of Soviet policy toward India than in his effort to describe some of the consequences of ideology for the policy makers themselves. To be sure, ideology was bent to strategic needs, but it also functioned to screen out useful information from Soviet academicians who were knowledgeable about India but forced to shift with domestic ideological winds. The relatively recent decisions to give researchers increasingly free rein to experiment with Marxist categories as they applied to Asia may have been brought about by the need of policy makers for new justifications. However, what the policy makers ultimately received was far more—a more flexible base upon which to build a foreign policy.

For all of its positive attributes, the Donaldson book is somewhat limited by its focus. Its concern with the relationship between Marxist-Leninist doctrine and Soviet Foreign policy winds up telling us more about changing doctrine than changing policies. It is easy to see, after reading Donaldson's book, how the interpreters of doctrine and policy makers in the Soviet Union have been influenced to change their perception of reality and how they have gained flexibility as a result. Far more difficult to understand is, having achieved this flexibility, how Soviet foreign policy makers come to make the choices that they do. It is useful to know that the Soviets narrowly missed failure in their India policy because of Marxist constraints. It would be equally useful to have Donaldson's explanation for the generally correct choices the Soviets have made in their dealings with India as of late.

Overall, Donaldson's study is a good one and will be of interest not only to students of Soviet foreign policy and of India but also to all those interested in Soviet social science. In this detailed scrutiny of Soviet academic writing on India—and of the pressure on Soviet academicians—Donaldson has given us insight not only into Soviet foreign policy but also into the problems of doing political science in the USSR.

GERALD A. HEEGER

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Beyond Dependency: The Developing World Speaks Out. Edited by Guy F. Erb and Valeriana Kallab. (Washington, D.C.: Overseas Development Council, 1975. Pp. xii + 238. $3.95, paper.)

This is a collection of ten articles and seven official and nongovernmental statements dealing mostly with economic and political relations between the developing and the developed countries. According to the introduction by Overseas Development Council President James P. Grant, this is meant to be a companion volume to the ODC's *Agenda for Action* series, and its principal purpose is "to ensure that the views of those in developing countries are heard more widely in the United States." Therefore, Mr. Grant hastens to add, the book includes only one article by a developed-country contributor, namely, Guy F. Erb.

As it turns out, that is too bad; Mr. Erb's paper is by far the best of them all—and one of the few that makes some sense. Although ninth in the contents, Erb's paper ("The Developing World's 'Challenge' in Perspective") provides a balanced and concise explanation of what changes have been taking place on the international scene, what is right and what is wrong with the "theory" of dependency, what it is that the developed countries have failed to do, what would be the costs of increased confrontation for both developed and developing countries, and what are some of the realistic choices before the world community.

Then there are two papers that, though narrower in scope, do deliver something. The article by Krishna Roy ("Population Policy from the Southern Perspective") is a neat outline of prevalent views on population planning in the developing world and a useful survey of what is being done about the issue in Africa, Asia, and Latin America. The paper by Bension Varon ("Ocean Issues on the International Agenda") is a good summary of the beginnings, evolution, complexities, and prospects for an international approach to seabed and other ocean-related problems.

The remaining seven articles can safely be skipped. Three of them deal with international politics, but what is said here has been said better at the United Nations and at other similarly enlightened gatherings: that developing countries want a self-reliant type of development, that greater attention must be placed on insuring that social justice accompanies growth, that social and institutional changes must receive greater priority, that regional arrangements might succeed in doing what world-wide groupings have not, and that petrodollars should be channeled toward the poor countries. I am referring here to the papers by Samuel L. Parmar ("Self-Reliant Development in an 'Interdependent' World"), Soedjatmoko ("Reflections on Nonalignment in the 1970's"), and Ali A. Mazrui ("The New Interdependence"). Two additional articles by Félix Peña and Constantine V. Vaitsos) deal with multinational enterprises. Of course, such enterprises are guilty of everything under the sun. The Vaitsos article is, on this account, particularly vicious. According to him, for instance, foreign investors are a damnable bunch for catering "primarily to high-income consumers," for being uninterested in investing in the areas of health, nutrition, and housing, and, in general, for attracting "scarce local technological capabilities and other resources into areas that do not represent the basic needs of the developing countries." A paper by Mahbub ul Haq outlines some negotiating principles and strategies for the future and, finally, a refreshingly simple article by Soumana Traoré describes an African experiment in grass-roots development based on as simple a prescription as showing people who they can make a profit by establishing a cooperative.

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The Politics of Trade: The Evolution of the Superbloc. By Douglas Evans. (New York: John Wiley, 1974. Pp. 128. Price not listed.)

The Politics of Trade purports to be an analysis of the relationship between power politics and a drift toward protectionism in world trade. The book begins with a brief history of modern protectionism, and then proceeds to a chapter ambitiously titled "The Theory of Economic Multipolarity." What is asserted to be theory, however, is not theory in a positive sense, but rather a series of factual assertions about protectionism, economic superblocs (U.S., USSR, European Economic Community, Japan, and China), and multinational corporations. Evans summarizes his theory as follows (pp. 15–16):

> The modern industrialized world has become heavily interdependent and rationally should become more interdependent. However, a small number of great powers (superblocs) have arisen. . . . They currently contain protectionist features which are bound to bring the superblocs into conflicts. . . . The inescapable conclusion is that the present structure and tendencies, by accentuating the self-sufficient character of the superblocs and reversing the trend toward interdependence, contain the seeds of economic warfare.

Unfortunately the logical links of the argument are not spelled out in a manner that would make his 'theory' capable of being tested. Indeed Evans himself sees theory as not distinct from evidence. "It must be said at the outset that . . . documentation came first. . . . Without such detailed evidence there could be neither theory nor conclusions" (p. vi). This is not to say that the book is not useful, but the political scientist or economist seeking to find a meaningful theoretical link between economic and political phenomena will not find it in this volume.

Evans' work is better described as a journalistic investigation into recent international economic and political history woven around the theme of economic multipolarity. It is journalistic in that it is largely descriptive, offering neither theoretical insight, verification, or documentation. Such books can, of course, be useful in the library or the classroom on occasion, but the book's chronology ends before such critically important events as the worldwide increase in relative food prices and the successful cartelization of OPEC, thus limiting its potential value as a guide to contemporary international political and economic relations.

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French International Policy Under de Gaulle and Pompidou: The Politics of Grandeur. By Edward A. Kolodziej. (Ithaca: Cornell University Press, 1974. Pp. 618. $24.50.)

Studies on French foreign policy since 1958 tend to fall into two categories. A large number of studies have focused upon the personality and perspective of Charles de Gaulle and his heirs and their attempt to implement a very special view of the role of France in global politics. Other studies focus upon the details of French diplomacy in a given issue area, like the building of Europe, the duel with the Anglo-Saxon countries, policy toward the Soviet Union or the Third World. Professor Kolodziej's lengthy study of French foreign policy in the Fifth Republic straddles both of these approaches. He has thus performed a great service not only to students of French politics and foreign policy, but also to undergraduate and graduate students who are just being introduced to this fascinating subject. Kolodziej's study is, moreover, especially valuable now that scholarly interest in the international policy of the Fifth Republic has significantly declined since the death of the first two presidents of the Fifth Republic. It is therefore not likely that any additional or more complete overviews of Gaullist diplomacy will be available for a number of years. Under these circumstances, Kolodziej's study is likely to be one of the major source books on the foreign policy of the first decade and a half of the Fifth Republic.

The study is admirable in several aspects. It fully integrates Gaullist security policy—in Europe, toward NATO and the Soviet Union—with economic, and especially international, financial policy. It integrates policy toward the industrialized West with policy toward the East and the South (and here, it traces the continuities and discontinuities between policies which were developed before, during, and after the process of decolonization). It also links the international policy of Pompidou with the precedents laid out by de Gaulle. All of this is fitting for a study which interprets the most consistent and grandiose foreign policy in the post-World War II period.

The book suffers from a few weaknesses, most of which stem from the author's effort to focus on foreign policy alone, rather than to dissect the relationship between foreign and domestic policy, or from his emphasis on his own interpretation of the meaning of grandeur in Gaullist policy. Thus, his interpretation of Gaullism is somewhat atemporal, and while the author notes that Gaullism was derived in part from the French president's reaction to events, he minimizes the amount of change which occurred in French policy or style of statecraft. Analysis of domestic politics and its effect on foreign policy is virtually absent, and thus seriously weakens Kolodziej's interpretation of policy under Pompidou. It also leads Kolodziej to neglect the linkage, which began under de Gaulle, between French policy toward the Soviet Union and the government's effort to reduce the influence of the French Communist party in French domestic political life. The author's emphasis on the "illusory" nature of the Soviet threat in the 1960s and his argument concerning the parallelism between French and Soviet goals in the Third World are overstated.

In summary, Kolodziej's study offers no new insights on the French policy of grandeur, but presents a thorough analysis of the major axes of Gaullist foreign policy. Experts will quibble with selective aspects of his interpretation, including his view that "de Gaulle's conception of his authority . . . rested ultimately on his self-defined contract with France, and not the French people" (p. 430). But by and large this analysis should remain a basic reference for some time. Especially valuable for an understanding of contemporary French policy are the chapters on *franc* diplomacy and on Mediterranean affairs.

EDWARD L. MORSE

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The Erosion of a Relationship: India and Britain since 1960. By Michael Lipton and John Firn. (London: Oxford University Press, 1975. Published for The Royal Institute of International Affairs. Pp. xvi + 427. $32.50.)

The study was planned in 1970 as an essay on the decline of Indo-British relationships since 1960, and became on its completion in 1975 a large reference work on decolonization. The text covers 236 pages; references, notes, and appendices on the tables extent from page 237 to page 303; and the tables, figures, and index end on page 427. This is an important case to study, ponder, and critically assess. It is not a book that can be reviewed in any normal fashion. Furthermore, every year that passes introduces new elements into the argument. For example, the consequences externally of the Indian Emergency of 1975 could not be encompassed in this book, nor have all of the recent trade implications based on oil imports and processing been treated.

The authors started with abundant evidence of a drastic reduction in Indo-British relationships during the 1960s. In 1970, for example, exports from Britain to India were valued at half the amount of 1960. Britons owned about five percent of Indian private capital in 1948, but the proportion had dropped to three percent in 1970, and in the same year 55 percent of India's foreign private capital was non-British. The authors assert that "India is a net donor of technical assistance to Britain" (p. 122), taking into account the cost of training in India of professionals, especially doctors, who ultimately practice in the United Kingdom. Even in military supplies, Great Britain now competes with the USSR and other countries for shares in India. Needless to say, India's diversification of trade, aid, shipping, exchange in invisibles, etc. have altered since the 1960s as have Britain's.

The authors started with a theory, a thesis, a methodological problem, and a determination to be rigorous in seeking evidence. The theory was that decolonization need not lead inevitably to a break or even substantial decline in mutually advantageous, adjusted relationships between a former metropolitan country and a former colony. The thesis is that such a decline did take place between Great Britain and India since 1960, but that it need not have been so, and might yet be reversed by incorporating countries such as India in the long-term benefits of the European Economic Community. The methodological problem was to locate accurate quantitative data on the segments of Indo-British relations (economic, social, academic, professional, media, political) over time, and to construct an equation that might be applied in other cases. The difficulty in finding verifiable quantitative data on many crucial themes, reflected in the text by the heavy use of words such as "about," "approximately," "in general," and other cautious phrases, show that the elusive "hard" equation is not yet ready for presentation because rigorous measurements are not possible at this stage.

This is a book that will be of value to many. The themes include trade, exports (British to India and vice versa), invisibles, private investment, aid and technical assistance, education and research, population exchanges, defense, the media, and political relations. Each part is buttressed by thorough referencing and relevant tables. Economists and political scientists will find each of these sections grist to the mill of contemporary analysis both of Britain and of India.

To this reviewer, the prime utility of the study is as a contribution to the work of what now are called "political economists" in political science. Here is a collection of data, carefully collected and cautiously analyzed, on a major case in decolonization. There is little overlay of ideology or grand theories from the Marxist-Leninist catechism. Where the evidence is thin or absent, the text bluntly says so. Where the data are less than flattering, either to Britain or India, conclusions nonetheless flow from the data.

This is a book that is important enough to warrant a new edition after a few years of battering in the stream of academic and political criticism. There is not a chapter, a subsection, or conclusion unworthy of meticulous reexamination by specialists. The result could be a work on decolonization that might begin to bring rigorous standards to this murky area of pseudo-science in the discipline. Even as it stands, it is a contribution of distinction.

RICHARD L. PARK

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Soviet Naval Policy: Objectives and Constraints. Edited by Michael MccGwire, Ken Booth, and John McDonnell. (New York: Praeger, 1973. Pp. 663. $32.50.)

One of the great strategic mysteries of our time is why the Soviet Union, the epitome of a continental power, has invested the resources that are necessary to become within a single decade a major, if not *the* major, navel power. "How serious is the Soviet naval threat?" and "For what purposes will the Soviet fleet be used?" are questions around whose answers hinge a potential naval armaments race, the likely scale of which the world has not yet begun to appreciate.

Soviet Naval Policy: Objectives and Constraints is part of a continuing series of publications by the Political Science Department at Dalhousie University which takes as its mission the analysis of Soviet naval developments. Though we will not know whether the Dalhousie effort correctly deciphers the objectives and constraints of Soviet naval activities until these objectives and constraints are revealed to us "in due course," one cannot but be impressed by the sophistication of the work to date. Among the many books currently available on the Soviet Navy, the Dalhousie series, and this volume in particular, are the most outstanding.

The collection contains 34 essays and is the product of a seminar held in Halifax, Nova Scotia, in October 1973. It is organized into five sections, each one of which is either set off by a symmary essay by one of the editors or contains one of their own contributions.

The first section is the bravest but of necessity the least successful as it attempts to penetrate the impenetrable, the Soviet decision-making process. In it Matthew P. Gallagher speculates on the role of the military in the formulation of Soviet national policy, Edward L. Warner, III, outlines his view of the bureaucratic politics of the Soviet weapon acquisition process, John McDonnell traces in detail the structure of the Soviet defense industry and the movement of the industry's alumni into the higher reaches of the government, and Philip Hanson analyzes the burden of defense expenditures on the Soviet economy. Although the section's authors are persuasive in arguing that it is likely that the military and related interests form important pressure groups within the Soviet system, they are, as might be expected, ineffective in their attempts to define the extent to which such pressure groups can and do influence policy even in an area as limited as the expansion of naval forces.

The second section explores through case studies the impact of Soviet naval power on the politics of the Third World. Alvin Z. Rubinstein, Uri Ra'anan, Robert O. Freedman, George S. Dragnich, Oles M. Smolansky, and Anne M. Kelly separately examine the situation in the Middle East; Geoffrey Jukes looks at Soviet actions in the Indian Ocean; Charles C. Petersen describes Soviet port clearing operations in Bangladesh; and Robert L. Friedheim and Mary E. Jehn discuss Soviet policy at the Law of the Sea Conference. About all that can be gleaned from this experience is that while the Soviets have thus far gained relatively little from the showing of their naval ensign in distant waters, they are likely to keep trying in anticipation of future benefits.

The third section contains a great deal of background information on the Soviet navy indluding totals for various ship types, a listing of all foreign port visits, and annual statistics for at-sea deployments. It also contains two essays of greater significance. One by Michael MccGwire provides a clear description of Soviet shipbuilding capacity and is particularly useful in sorting out the many classes of Soviet submarines, the backbone of the Red Fleet. The other by Nigel D. Brodeur gives meaning to the data on the size and shape of Soviet ships by comparing the quality and function of Soviet and Western weapon systems. According to Brodeur, the Soviet navy, though inferior on several technological dimensions to its Western counterparts, has added an open sea offensive capability to its traditional territorial waters defensive capability.

The final two sections examine Soviet naval thought and policies. Robert G. Weinland creatively attempts to reconstruct the potential purpose behind Admiral Gorshkov's famous series, "Navies in War and Peace," by analyzing Soviet publication practices. Harlan Ullman weights the counter-Polaris task of the Soviet navy. Bradford Dismukes describes potential roles for the Soviets' general purpose naval forces. And Franklyn Griffiths examines the tactical use of naval arms treaties from a Soviet perspective. But the strength of these sections and in fact, the volume as a whole, lies in the several essays by MccGwire. In one he argues that a major turning point in Soviet strategic and naval policies came in 1961 largely in response to perceived U.S. initiatives. In the second he points out the operational weaknesses of the Soviet navy in fulfilling its various potential missions and especially its need for overseas bases. And in the third he notes that though the Soviets have never embraced the Mahonist notion of the importance of the command of the sea in world affairs, they are certainly willing to use naval power selectively to advantage.

The mystery of Soviet naval intentions remains, however, despite the best efforts of MccGwire and associates. If this volume has a hero it has to be Admiral Gorshkov, not only for presiding over the bureaucratic battles which have given the Soviet navy its opportunity for growth, but also for his extensive writings which are reverently examined here as in other places and which, their bulk not withstanding, reveal so little about Soviet intentions. We know that the Soviet navy serves state purposes, but what those purposes are we still search to discover.

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The International Law of Communications. Edited by Edward McWhinney. (Dobbs Ferry, N.Y.: Oceana Publications, 1971. Pp. 170. $7.50.)

The foundations of international law are indeed political, as this slender volume suggests in so many subtle ways. A nation-state binds itself to and even obeys international agreements to the extent that the conveniences they provide—in the form of agreed-upon technical norms (definitions, measurements, units of exchange), standard operating procedures, and above all predictability in a generally important area of life—outweigh inconveniences caused by constraints imposed upon the state. On matters deemed vital to what decision makers see as "national interests," however, and on which no international consensus has yet emerged, the effort to establish "law" may be premature and fruitless, if not downright deleterious for the continued success of rules on which there is substantial agreement.

The volume comprises 11 papers, many of which were first presented at a colloquium held in the Institute of Air and Space Law at McGill University, Montreal, in May 1970, chaired by the Institute's director, Edward McWhinney. Other authors were currently, or had been, high-level civil servants in international bodies (United Nations, Council of Europe), top-ranked legal officers in national governments (Nigeria, Brazil, France, India, and the United States), and legal scholars from Canada, Czechoslovakia, and the United States.

Given current trends in international communications and the sponsorship of the conference, it is not surprising that most papers focus on legal developments responsive to or needed because of technological developments in satellite communications. A common theme is the high level of interaction among technicians and political officers in making new agreements. Over the course of the last half-century, technological growth in the communications industry has been astounding: the very notion of space satellites would surely have boggled the minds of early radio pioneers. As in many other areas of regulatory law, both national and international, rulemakers simply must be in constant contact with technologists.

Such interlocking has ramifications for communications law. First, the regulators must perforce rely upon the information of technologists, who can, in effect, define the context within which the rule makers must operate. Still more general policy makers, such as national legislators, often trail far behind in their own conceptions of the issues—an example being the proclivity of United States congressmen to view communications satellites as basically similar to transoceanic cables, as "cables in the sky." Second, the lag in conceptions and prescriptions, which can take several years to overcome in a body such as the International Telecommunications Union (ITU), often hampers technological progress. Technologists complain that they must rely on outmoded equipment or even continue to build incompatible components because regulatory agencies cannot act fast enough in approving new equipment.

Still another consequence is that the interaction of technology and law concentrates information and hence influence in the hands of the more technologically advanced countries, most notably the United States but also the Soviet Union. With one exception, however, the international protocol of the conference did not permit authors to do much more than note the overweening role played by the super powers in creating regulatory bodies (and their agendas) at the international level, express mild resentment at some of the more outrageous attempts by these super powers to swing their weight around, or cite approvingly the super powers' efforts to build up other countries' communications facilities and tie them into their own communications systems—acts that less generous critics, elsewhere, would term the creation of "dependency" relationships.

The exception is Abram Chayes' excellent analysis of "Unilateralism in United States Satellite Communications Policy." It describes how Congress, pushed on by the private communications industry, first turned over control of American communications satellites to a private corporation, Comsat, and then, in the Communications Satellite Act of 1962, tried to create an international system that would be primarily the domain of American business. European resistance forced American negotiators to back down, but the interim Intelsat that emerged nonetheless made Comsat its manager and gave to the United States majority representation that permitted it to veto anything it did not like.

The content of international messages is another area in which the creation of international law has been hampered by national interests. Occasional pieties aside, the only internationally accepted (albeit not necessarily preferred) norms are self-regulation, bilateral and regional agreements to avoid beaming offensive material, and jamming incoming programs. More insidious than outright propaganda, which nation-states may well eschew because they are vulnerable to retaliation, is the transmission of programs or information that structure people's perspectives (values, images, and predispositons), such as family-situation comedies that predispose people to accept as a norm for their own behavior the high consumption of luxury goods. The control of structural programming—indeed, the question even of whether it should be controlled—is a topic that international legal experts have not yet broached.

RICHARD L. MERRITT

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The United States and West Germany 1945–1973: A Study in Alliance Politics. By Roger Morgan. (New York: Oxford University Press, 1974. Pp. 282. $22.50.)

The period covered in this study witnessed a diplomatic revolution in German-American relations. Who could have imagined in 1945 that a U.S. ambassador in 1973 would hail before a German audience the "special relationship" uniting his and their country for more than two decades? Time was when a special relationship with Britain had been the center piece of American policy in Europe, and Germany had been the common foe in two world wars. But that was before the Cold War, before the division of Germany, and before the precipitous decline of British power. While British leaders vainly sought to maintain the vestiges of the old bilateral association with the United States, the latter forged a new one with West Germany. According to Morgan, "The most striking theme running through German-American relations in the period of more than a quarter of a century surveyed in this book is the high degree of harmony established between two nations which during the previous generation had been enemies in war." Frequent disagreements notwithstanding, he informs us, "the partnership as a whole showed a degree of mutual confidence rare in the history of relations between two major powers" (p. 247).

Given the title and subtitle of Morgan's book—co-sponsored by the Royal Institute of International Affairs and the Harvard Center of International Affairs—this reviewer welcomed its publication. An innovative, comprehensive exploration of the German Federal Republic's transformation from supplicant to senior alliance partner has long been needed. Most previous studies have either been descriptive accounts of the American presence in West Germany or provided at best only bits and pieces for a more extensive analysis. It is therefore all the more a pity that this book does not live up the promises of its introduction. There Morgan declares that his study has two objectives. One is to relate the story "as accurately and clearly as possible." The other is to present that story "in such a way as to bring out its significance as a case study in the politics of alliance, some of whose features are likely to be found in other bilateral alliances widely separated in time and space from the particular example selected for analysis here" (p. 1). Regrettably, the book falls considerably short of achieving either aim.

The story Morgan has to tell is essentially a diplomatic history of the relationship between a small group of official foreign policy makers in Washington and Bonn. He allows that "various nongovernmental forces of a psychological or economic character . . . obviously had profound effects on the relations between government," but maintains that they must be kept "strictly in their place" (p. 7). As a consequence, what is taken to be self-evident in Morgan's approach is largely ignored in his presentation. His discussion of economic factors, for example, pays little or no attention to the impact of West Germany's trade dependency on its relations with the United States. And his consideration of psychological factors does not go much beyond such curious observations as that Chancellor Adenauer was "obsessed" with the pursuit of German reunification (p. 100). A novel idea, indeed, and so is the notion that the restoration of a united Germany was all along a basic purpose of decision makers in Washington and Bonn (p. 46).

But even strictly on his own terms, Morgan's efforts to tell his story as accurately as possible is flawed. One does not have to subscribe to a revisionist approach to wish that he had made some innovative attempt to re-search and reinterpret the extensive literature on his subject. As it is, we get a summary of the now all too familiar official and quasi-official Cold War version of "what actually happened," along with a detailed who-was-who inventory of governmental leaders and advisers. Morgan thus relies heavily on Adenauer's self-serving *Memoirs* and similar "inside" accounts. For a small part of his story, he has gone to the Dulles papers and oral history interviews at Princeton University; to judge by the results, these were either not especially useful for his purposes or not particularly well used. The book also contains frequent footnote references to "private information" not otherwise identified, but apparently provided by some of the surviving actors in and out of public office. Apart from these sources, Morgan constructs his highly selective tale on a very limited number of standard secondary and tertiary sources, including newspaper and periodical articles all too often inadequately identified.

Morgan's efforts to tell his story as clearly as possible are hindered rather than helped by his ambitious second objective. Here his intention is "to contribute to the construction of a theory of how alliances operate, by presenting the data of the German-American case within a rigorous framework of concepts." His purpose, as he tells us, "is to demonstrate the explanatory value of these concepts in elucidating the forces at work in the particular case analyzed here, in the belief that the same method will prove valid for the study of other bilateral alliances too" (p. 5). However, Morgan never gives us a clear definition of his concepts nor states his underlying premises. His inductive approach to theory construction involves a good deal of circular reasoning and leads him to adhere rigidly to an organizational scheme that does not come off too well. Each episodic chapter of his chronological narrative disaggregates interrelated data in a sequential presentation of internal politics, bilateral relations, alliance politics, and relations with "the adversary" (i.e., the Soviet Union). Why Morgan chooses to follow this particular sequence is not entirely clear. He arrives at the not very startling conclusion that a common foe was the most decisive factor in German-American relations, which suggests that a reverse order might have served Morgan's purposes better. As it is, his disaggregative schema not only obscures rather than illuminates the interrelationship between his four levels of analysis, but produces a host of repetitious and "as we shall see" statements.

The book ends with a 1974 postscript—designed to bring it up to date, but in effect dating it. There is little point in trying to catch up with the latest headlines in such a study and it is anyhow a hopeless undertaking. By all indications we have entered upon a phase of German-American relations very different from those in the era of Morgan's study and nothing is gained from viewing the 1970s in terms of the fifties and sixties.

LEWIS J. EDINGER

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Foreign Policy and Interdependence in Gaullist France. By Edward L. Morse. (Princeton, N.J.: Princeton University Press, 1973. Pp. 336. $14.50.)

This provocative exploration of French foreign policy is a must reading for students of

Gaullist foreign policy and, more broadly, of international relations *and* comparative politics. It argues that French foreign policy represents a special case of a fundamental transformation that has occurred in the relations of states and in the relations of the governed and governing. These systemic changes in interstate and intrastate relations arise from the accelerated modernization of societies around the world, especially in the post-World War II era. Economic, scientific, and technological interdependencies so link national societies that the achievement of any regime's foreign policy objectives are a function increasingly of the policies of other states and actions of other societies and groups.

These transnational forces foreclose the pursuit of an independent foreign policy. The distinction between foreign and domestic policy and politics is blurred; the goals and strategies that a state may pursue relative to its own population or to those abroad are significantly restricted; policy objectives are altered and ultimately transformed in response to these intertwining eco-scientific-technological transactions and the incipient international patterns of interstate behavior and institutions they foster; the domestic foreign policy process is pushed from legislative and cabinet rule to executive and bureaucratic domination; and the gaps that develop between these transnational interdependencies and national control mechanisms to regulate them pose the conditions for crisis diplomacy between and among states pursuing conflicting purposes. French foreign policy tried to be different, to escape these limitations, to be independent; but failed, as evidenced in the May events and in de Gaulle's subsequent resignation after losing on regionalization and Senate reform.

Morse's work is significant on a number of counts. It is one of the few foreign policy works that systematically argues that low politics—involving nonsecurity or diplomatic factors—is the principal determinant of the foreign policy of a modernized state. In looking beyond the anecdotal in French foreign policy behavior and departing from current interpretations of Gaullist behavior, the theoretical importance of the work cannot be ignored. A measure of the book is that analysts who dissent from its diagnosis and conclusions about Gaullist foreign policy are forced to frame their critiques in equally theoretical terms.

There is room for doubt whether the Gaullist case is as suitable as Morse believes for his purpose. He has to posit de Gaulle's search for independence as genuine from the start, and not partly, and importantly, a stratagem itself. A good *prima facie* case can be made that de Gaulle acutely understood the limits of state action from the start. He was engaged simultaneously on a number of policy levels (economic, technological, military, and diplomatic), in a number of geographic settings (Western and Eastern Europe, the Atlantic Community, and the Third World), and in a variety of international arenas (United Nations, Atlantic Alliance, European Economic Community) to increase France's (and his own) bargaining power within a refracted and discontinuous system of global politics in which foreign and domestic actors constituted a single field of action potentially subject to the French president's manipulative skills. De Gaulle's ostensible search for personal and national independence and *grandeur* obscured what he recognized from the beginning was his (and France's) ensharement in the politics of interdependence. Morse attempts to demonstrate what de Gaulle might be assumed to have accepted at the outset of his political career: first as the man of June 18, 1940, who called for allied help and, second, as the beneficiary of the spring *coup d'etat* in 1958 in which temporary domestic allies were accepted (the defenders of the empire and the remnants of the colonial army) only to be later repudiated.

De Gaulle's conception of the politics of interdependence appears to be broader and deeper than Morse suggests. First, there was the French president's interdependence with the French people and nation. De Gaulle never claimed independence from an electoral test although he repudiated the election paraphernalia of the Third and Fourth Republics. Second, there was recognition of France's dependency on its European partners for modernization. EEC restrictions were accepted in a partial trade-off for the common agricultural policy (CAP). German capital was expected to make French farms *and* industry more competitive. Third, and most important, de Gaulle tried to turn strategic inferiority and dependency into interdependency which implied increased influence over allied policies. The Algerian War was liquidated and former domestic allies foresworn; the *force de frappe* was not only counted upon to influence Washington, Moscow, and Bonn but also relied upon to gain control over the army and (from Morse's perspective) to accelerate the modernization and competitive strength of the French economy.

If French foreign policy changed after 1968, one can doubt whether it was due to the transnational movements that Morse identifies in trade, monetary flows, and investment and their impact on public opinion or to other, more basic factors. There is no mention of the Russian invasion of Czechoslovakia, of the limits set by Bonn and accepted by de Gaulle in his handling of detente with the Soviet Union, or of the tortuous distinctions made by Paris between NATO and the Atlantic Alliance which aimed at maximizing French diplomatic initiative and advantage without a corresponding loss in access to the American security guarantee. There is irony, too, in the price paid by de Gaulle's government for its criticism of Israel and for its subsequent arms embargo after the Six Day War. France's industrial development and continued modernization hinged on Arab oil. While French diplomacy worked for economic advantage abroad, it ran afoul of public opinion at home for its pro-Arab stance, a factor that played a partial role in de Gaulle's defeat in the spring referendum of 1969.

Part of Morse's problem is definitional and conceptual. For Morse, modernization leads to interdependence among states and societies, and interdependence prompts cooperative strategies. It is not fully clear what Morse means by modernization or how it is linked empirically to the transnational processes of interdependence. The linkage Morse makes is more suggested than demonstrated, yet his case study depends critically on establishing the French connection. There is also neither a logical nor an empirically verifiable reason why interdependence should be narrowed to cooperative relations. Both negative and positive interdependencies, as Rosecrance defines them (in *World Politics*, 26 [October 1973], 1–29), are an invitation to the domestic elites dominating a state's foreign policy to manipulate these conditions to the disadvantage of other states and groups. Strategies of conflict are often more attractive than those of cooperation. Witness de Gaulle's withdrawal from NATO to facilitate detente, which, paradoxically, has fostered interdependencies among rival political units. Finally, the disputes of states over economic questions, turning on questions of monetary reform, trade, investment, and the cost of raw materials, while certainly shaped by transnational factors, are also subject, often decisively, to the bargaining and rule making of governmental elites. These latter processes are only partially the product of the factors Morse focuses upon. Through feedback, they are shaped by the governmental mechanisms of the nation-state in ways that still escape the researcher's probe and scalpel.

These criticisms should not detract from the service Morse has performed. He focuses our attention away from exclusive concern with strategic and diplomatic considerations in looking for the determinants of foreign policy. He has enriched the study of foreign policy and of French foreign policy in particular. It is a book that deserves close reading and wide circulation.

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Comparing Foreign Policies: Theories, Findings, and Methods. Edited by James N. Rosenau. (New York: Halsted Press, 1974. Pp. 442. $20.00.)

The comparative study of foreign policy represents a subfield of international politics and political science which has certainly evolved and yet at the same time failed to really progress. Such "misevolution" is evident within Rosenau's latest collection of 17 essays (by 12 primary authors). As the subtitle suggests, the volume is organized around theories, findings, and methods, and may be comfortably reviewed around such divisions.

The first essay in the theoretical section by Patrick J. McGowan introduces the notion of "positive" foreign policy theory. The essay itself is in fact more concerned with advancing yet another set of exposes on the Rosenau framework and the merits of social scientific foreign policy analysis. What is particularly distressing is McGowan's repeated reference to Rosenau's *theory* of foreign policy behavior. More accurately, Rosenau's "pre-theory" constitutes a *framework* for the analysis of foreign policy behavior and a device for the subsequent *development* of a theory of foreign policy. Misnomers of this nature are particularly damaging to a subfield already plagued with severe growing pains.

The subsequent essays in the first section by McGowan, O'Leary, and Thorson are all devoted to the exposition of yet another concept of Rosenau's national adaptation. These essays are sometimes interesting yet shed no light upon the more fundamental problems associated with foreign policy theory building.

Section two is devoted to findings. The essays by Rosenau and Hoggard; Powell, Andrus, Fowler, and Knight; Moore; Moore; East and (Charles) Hermann; and Harf are all devoted to assessing the impact which national attributes exert upon foreign policy behavior. What the essays fail to note is that attribute variables cannot exert a causal/explanative impact upon foreign policy behavior because by their very nature they are static and therefore unable to produce explanations which are necessarily dynamic. The essays thus fail to employ attribute variables solely as means to the classification of nations and the *description* of foreign policy.

The essays by Harf, Hoovler, and James, Jr., and (Margaret) Hermann are more interesting and much more productive. Harf, Hoovler, and James, Jr., draw an interesting distinction between dynamic external and systematic variables and employ it convincingly. While they are certainly not the first to draw a distinction between external variables of substantially different natures, they are among the first to apply the distinction quantitatively.

Similarly, Margaret Hermann's essay represents one of the very few valuable quantitative studies concerned with the relationship between idiosyncratic variables and foreign policy behavior. Much like Harf, Hoovler, and James, Jr., she correctly conceptualizes dynamic variables as causal in search of *explanations* of foreign policy behavior.

The volume's third and final section is erroneously entitled "methods." Like the misnomered first section which deals with only one "theoretical" concept (adaptation), the third section deals only with the methodology surrounding the generation and use of events data. The essays by Hoggard, Burrowes, and McGowan are concerned with the generation of valid events data, while Salmore and Munton are concerned with the development of a typology of foreign policy events. These essays are interesting—although highly specialized—and constitute a logical end to a volume which increasingly moves away from the general and substantive and steadfastly toward the specialized and abstract.

The specialized and abstract nature of the volume serves to narrow the audience which might find it interesting and comprehensible, and virtually eliminates any possibility of the essays contributing to the formulation and implementation of actual policy. The quest for policy relevance is thus hardly advanced by this latest Rosenau reader.

The real value of the volume lies in its ability to make us look—once again—at the subfield of foreign policy analysis. Where is it heading? Where has it been? Clearly, while volumes like the one at hand are interesting to a decreasingly identifiable group of scholars, they do not advance or well publicize the subfield. If little else, they illustrate the "coherence and consensus" of but a few, and fail to exploit creatively the disarray which other subfields manage to deal with productively. While many of the essays might have served usefully as isolated articles or research notes, together they do not generate a whole greater than the sum of its parts.

STEPHEN J. ANDRIOLE

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The End of Nowhere: American Policy toward Laos since 1954. By Charles A. Stevenson. (Boston, Mass.: Beacon Press, 1972. Pp. 367. $8.95.)

This account of the vicissitudes of U.S. policy toward Laos opens with the first Geneva Conference and ends in the fall of 1971 when the clandestine American-supported military operations against the Lao Communists were gradually emerging into the limelight of public discussion. The author showed prescience when in judging the final outcome of the American efforts he speculated (p. 269) that "by insisting on a whole loaf, the United States may wind up with only a charred crust." The anti-Communist Royal Lao Government, maintained in power only thanks to intensive American bombing and massive economic aid, has since fallen and American influence has vanished from Laos. On December 3, 1975, the Pathet Lao announced the abolition of the monarchy and the establishment of a Lao People's Democratic Republic. This study attempts to explain, how American policy leading to such results was formulated and why it eventually failed to attain its objectives.

The author has painstakingly pieced together the available information on the evolution of the American role in Laos. He has combed with great care through the open literature and studied the extensive congressional hearings on the subject. Many U.S. officials involved in the making of Laos policy granted him interviews. The 86 respondents included such key figures as Richard Bissell of the CIA, Chester Cooper of the NSC, Admiral Arthur Radford, William Bundy, and Averell Harriman as well as most diplomats who served as U.S. ambassadors in Vientiane.

Yet, the reader may wonder whether a history based on the public record can do justice to the complexities of American involvement in a remote country where a secret proxy war was being waged under CIA auspices and where American officials were under instructions to avoid any open violation of the Geneva Accords. The publication of the Pentagon Papers provides an answer to this question. The papers appeared after Stevenson completed his manuscript, but he had occasion to peruse them before it went to the publisher. In a postscript to his study, the author summarizes what he learned from reading these documents and how they modified his interpretation of events. On the whole, Stevenson believes that his conclusions stand up rather well, but admits that this new evidence forced him to modify his judgments in some instances. The reader might find it useful therefore to read this study in conjunction with the Pentagon Papers.

The eight chapters detailing the history of U.S. involvement in Laos between 1954 and 1971 (the appendix contains also a helpful chronology) are preceded by a brief introduction and followed by a more substantial chapter presenting the author's conclusions. As suggested by the book's title, the focus of the study is on American policy rather than on developments in Laos. (For information on the latter, Arthur Dommen's *Conflict in Laos* remains indispensable.) As a result, the reader without background knowledge about Laos never gets a feel for the country and for the people the U.S. policy makers were attempting to influence and direct. A broader and more penetrating introduction to the Lao setting of U.S. policy might have helped to fill this gap.

Stevenson's study is both an historical account and a case study in U.S. foreign policy and decision making. He sees the American failure in Laos as rooted primarily in the exaggerated strategic importance attached by the U.S. government to that country and the "obsessive anti-communism" (p. 240) of American policy. The United States thus was never able to adjust to what he terms the political realities of Laos. But he assigns much responsibility also to the behavior of contending U.S. bureaucracies—the State Department, AID, the Pentagon, and the CIA. Each had its own perspective, shaped by its primary interests, and each acted accordingly. The author's analysis is rich in observations regarding the ways in which such differences in bureaucratic perspectives tended to be reflected in the policy-making process and how they prevented the United States from altering its course. Despite the author's heavy emphasis on the analysis of the bureaucratic process and its consequences, he appears to conclude that in the end it was primarily not change of U.S. policy machinery that was required in Laos, but a change of minds. Writing in 1971, he suggested that greater American sensitivity to the conditions of Laos and a greater willingness to accept Pathet Lao participation in the government might have obviated the need for the kind of destructive warfare that charred Laos and turned a large portion of its population into refugees.

PAUL F. LANGER

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Nuclear Proliferation Problems. By the Stockholm International Peace Research Institute. (Stockholm: Almqvist and Wiksell and Cambridge, Mass.: The MIT Press, 1974. Pp. 312. Swedish Kroner, 58.00.)

After the People's Republic of China exploded its first nuclear device in October 1964, a spate of books and articles were written which predicted that the club of nuclear weapons states would rapidly increase to 10, 20, or more. It was rather surprising, therefore, that it took almost a decade before India became the sixth nuclear nation by conducting a "peaceful nuclear explosion" in May 1974.

Why were these predictions in error? In retrospect, a number of considerations relevant to nuclear proliferation were not well appreciated. Security guarantees provided assurances to some states while constraining their movement toward an independent nuclear capability. The domestic opposition to the nuclear option within certain countries was underestimated. The economic and technological wherewithal necessary to embark on a nuclear weapons program was beyond the reach of many "threshold" states. Some governments did not value highly the political payoff of going nuclear. And, for most nations, the weapons-grade material required to make bombs was simply unavailable.

The Indian explosion and the prospect of the rapid spread of nuclear energy-related technologies, stimulated in part by the 1973 OPEC oil embargo, have now once again raised serious concern about the spread of nuclear weapons. This concern has been reflected in a wealth of publications within the last two years on the technical, economic, and political dimensions of nuclear proliferation.

Among the first of these publications was this volume produced by the Stockholm International Peace Research Institute (SIPRI). Indeed, the SIPRI volume was planned prior to the Indian explosion. It is the product of a conference convened by the Institute in June 1973 which was intended to speak to the issues discussed at the May 1975 Review Conference of the Treaty on the Non-Proliferation of Nuclear Weapons (NPT).

The volume is divided into four parts. The first is a series of nine chapters written by five different authors summarizing various aspects of nuclear technology. The second focuses on the issue of NPT safeguards and techniques to deal with nongovernmental nuclear proliferation. The third is a relatively brief discussion of the prospects for cooperation in the peaceful applications of nuclear energy. And the fourth is a collection of analyses on security problems facing non-nuclear weapon states, including separate chapters on Italy, Japan, India, and Israel.

The book has a number of obvious points in its favor. Contained within its covers is an extraordinary amount of detailed information, particularly on the technology of nuclear power. Moreover, the inevitable unevenness of a multiauthor format is more than compensated for by the differing perspectives brought by the contributors, who are drawn from 12 different nations, are authorities in their fields, and represent a variety of independent, national government and international organization viewpoints. The fact that this volume has been widely used as an information source in Congressional hearings and other forums devoted to nuclear proliferation testifies to its overall value (although this value is diminished by the absence of a much-needed index which would enable it to be used more easily as a reference work).

Unfortunately, however, events are now rapidly overtaking much of what this volume has to offer. The last part of the book, and potentially the most interesting, is how hopelessly out of date. The chapter on Italy and the nuclear option was written prior to the Italian ratification of the NPT. The same holds true for Japan, which deposited its instrument of ratification in June 1976. The discussion of Indian attitudes toward the NPT must now be evaluated quite differently in light of India's demonstrated nuclear capability. The chapter on Israel, while containing some useful historical information, necessarily suffers from having been written before the October 1973 war. And a final chapter on European security and the NPT is of limited value because of the introduction of the issue of tactical nuclear weapons into the Mutual Balanced Force Reduction (MBFR) Talks, the ratification of the NPT by the Federal Republic of Germany, and the continued vigor of German and French nuclear exports.

Moreover, the less time-dependent sections dealing with nuclear technology, NPT safeguards, and the peaceful applications of nuclear energy are nonetheless increasingly dated and incorrect. Substantial revisions have been made in the last two years concerning the estimated demand for nuclear power, the economic viability of the fast breeder reactor, and the status of uranium enrichment technologies, particularly with respect to laser isotope separation. The prospect of nongovernmental nuclear proliferation has now been exhaustively treated in the highly controversial work by Mason Willrich and Ted Taylor, *Nuclear Theft: Risks and Safeguards*, which advances the case far beyond Willrich's brief essay in this book. And the issue of peaceful nuclear explosions, discussed briefly by V. S. Emelyanov of the Soviet Union, has been treated exhaustively in a recently signed agreement between the United States and the Soviet Union related to the Threshold Test Ban Treaty.

The likelihood of nuclear proliferation in the next ten years, unlike a decade ago, is quite high: security guarantees are weakening; domestic opposition in many nations to the nuclear option is diminishing; the economic and technological wherewithal is becoming more readily available; the political payoffs for a variety of states appear to be substantial; and perhaps most importantly, the ability to obtain weapons-grade material is on the increase.

The incentives and disincentives on the nuclear decision require thorough examination, as does the assessment of viable nonproliferation strategies such as nuclear supplier agreements and multinational nuclear facilities. SIPRI would do well to turn next to this task.

MICHAEL NACHT

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World Armaments and Disarmament: SIPRI Yearbook 1974. By the Stockholm International Peace Research Institute. (Cambridge, Mass.: The MIT Press, and Stockholm: Almqvist and Wiksell, 1974. Pp. 526. Sw. kr. 75.00.)

This is the fifth in a commendable series of World Armaments and Disarmament Yearbooks compiled by the members of SIPRI, an independent institute financed by the Swedish parliament. It is a massive undertaking and one must think a discouraging one to the members of the institute who are basically opposed to the arms race and armaments and yet conclude that "unfortunately the events of the previous year have not provided grounds for altering our assessment of the lack of progress in disarmament. . . . In our view, disarmament—with abolition of nuclear weapons having the highest priority and leading to general and complete disarmament—is essential if nuclear holocaust is to be avoided. *It is our basic commitment to* *effective disarmament that makes inevitable the 'harshness' of our judgment of the value of existing arms control measures"* (p. v, italics in original).

The yearbook is divided into four sections which detail the substantiation for this judgment. "Part I: 1973, the Year in Review" discusses in two chapters military developments in the Middle East and Southeast Asia and then devotes separate chapters to mutual force reductions in Europe, nuclear deterrence strategies, and new trends in strategic nuclear weapons developments. The analyses are detailed and comprehensive, if not always prescient. For example, the scant mention of the development of the cruise missile does not hint at the difficulties this new system has brought to the extension of the SALT agreements. Reading the text, accompanied by graphs of always rising weapons levels and expenditures, one senses SIPRI's growing frustration with the slippery debates of deterrence theorists and with the burgeoning U.S. and USSR nuclear arsenals. Finally, the section concludes: "No nuclear strategy, past or present, based on deterrence is credible to rational persons, particularly when the powers concerned are equally armed. Such strategies must be seen for what they are—mere rationalizations for the development and deployment of weapons made available by military technology" (p. 70). Of course, this is too simple since not all weapons systems which could be built are built. Nonetheless, from the institute's point of view, it is certainly too true since many weapons systems which have no mission soon find one, if only as a bargaining chip. In fact, the submarine-launched cruise missile was itself part of what Secretary of Defense Laird called, in 1972, a "triple play for peace," the three elements being the ABM treaty, the Interim Agreement on Strategic Weapons, and finally the president's national security budget replete with bargaining chips such as the cruise missile.

"Part II: Developments in World Armaments," the lengthiest part of the yearbook, consists of two chapters, "The Dynamics of World Military Expenditures" and "World Armaments, 1973." The first is a brief discussion of the rising world military expenditure. SIPRI estimates that in 1972 $207 billion was spent on world military expenditures and that this represented 6.5 percent of the total world output. This is slightly smaller than the 7 to 8 percent share claimed in recent Vietnam war years; but as pointed out, it is a much higher proportion "than those which prevailed in any other peacetime period in recent history: in the years preceding World War I, and the inter-war years, the proportion was some 3–3.5 percent" (p. 123). This expansion is attributed to a variety of reasons, including the rising cost of personnel; but the main reason is ascribed to the "technological nature of the armaments process" (p. 134). One wishes that this claim could be substantiated more completely than is done here.

The chapter "World Armaments" begins with a brief introduction outlining trends in military expenditures. However, the heart of the chapter is in its appendices, including an excellent discussion of the problems of estimating Soviet military expenditures and some 80 pages of tables showing, since 1952, annual military expenditures for each country. The tables are nicely arranged by region and by defense alliance. Most tables appear twice, once with the entries in constant 1970 U.S. dollars using 1970 exchange rates and then in local currencies and current prices. Last are two lengthy registers, one of indigenously designed weapons in development or production in 1973, and one of licensed production of foreign-designed major weapons.

"Part III: Advances in Technology" consists of three chapters covering reconnaissance satellites, antisubmarine warfare, and the automated battlefield. They are brief but elaborate reviews of such systems as are now available to the U.S. and the USSR. Here the coverage is biased, by the availability of data, toward the capabilities of U.S. systems. However, there is a lengthy list of Soviet reconnaissance satellites launched in 1973, and two figures showing ground tracks of Soviet maneuverable and unmaneuverable satellites over the Middle East during the October War of 1973 demonstrate their usefulness and some of their capabilities. The section on antisubmarine warfare emphasizes the current stability of the seabased deterrent forces of the U.S. and the USSR and concludes with a few timid suggestions for enhancing this stability, including the development and deployment of a new long-range missile.

Part IV ends the yearbook with a review of disarmament negotiations in 1973, a review of the status of the implementation of agreements related to disarmament such as the Seabed Treaty, the Nonproliferation Treaty, and the Treaty of Tlatelolco, and finally with a chronology of disarmament-related events of 1973.

The yearbook is an impressive compendium of facts and analysis and an excellent bibliographic tool. It would be slightly enhanced if more of the tabular data appeared graphically; but it should be of considerable value to all who are interested in either the numerology or the nuance of armaments and disarmament. It is a welcome companion to the preceding SIPRI yearbooks.

MICHAEL D. ROSENTHAL

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Normalization of Japanese-Soviet Relations 1945–1970. By Savitri Vishwanathan. (Tallahassee, Florida: The Diplomatic Press, 1973. Pp. xii + 190. $15.00.)

"Geographic closeness breeds special interests between nations," Vishwanathan asserts in the preface to this short account of postwar Japanese-Soviet relations. While one may not challenge this assertion, it is important to understand why "special" ties have yet to develop between these two countries–despite their geographic propinquity.

The thesis of the book, written by an Indian scholar at the Jawaharlal Nehru University in Delhi, is that Japan's postwar Soviet policies have been shaped almost entirely by outside forces, and especially by the U.S.-Soviet relationship. Both superpowers, she argues, have consistently adapted their policies toward Japan to fit the more compelling needs of their mutual rivalry: Washington has pressured Japan to limit its political and economic commitments to the USSR; the Soviet Union has continuously sought to enlarge its role in Japan and to loosen the American grip there by assorted, and mostly futile, tactical maneuvers. Japan's latitude in setting its Soviet policies has been a direct function of these contending pressures and of Soviet-American tension levels. Thus, Japanese independence was lowest during the peak Cold War years and most extensive during the recent "era of détente."

The author's preoccupation with external factors leads her to assign a clearly secondary role to domestic Japanese political factors. She discusses some of these domestic elements, such as interest groups, public opinion, and intraparty factional conflict, but not systematically. For example, she does not assign relative weights to the different party factions and interest groups favoring, and those opposing, normalization of relations with the USSR during 1955–56. Also, the positions of the political parties are not defined clearly. Further, she treats Japanese opinion data uncritically and sometimes without giving references (e.g., that Soviet objections to U.S. occupation actions on fishing "antagonized public opinion," pp. 40–41; and that Communist China's participation in the 1951 peace treaty "echoed the sentiments of the Japanese people" p. 48).

A more dynamic approach to the subject would have included not only the description and correlation of these domestic political factors but also the identification of some of the linkages between the domestic and international forces that have influenced Japanese policy choices. Vishwanathan writes persuasively that Japan has benefited from the postwar rivalry between the superpowers, but her related contention–that Japan has sought actively to exploit this rivalty–is far less convincing. For instance, she maintains that Japan chose to align itself with the U.S. in 1952 because of the "Soviet-American split" (p. 49) and the U.S. seemed "stronger" than the Soviet Union (p. 144). Could these have been the reasons? In any case, if the author's portrayal of Japan as a pawn in the superpower struggle is accurate, how could the Japanese government have chosen its principal ally–a truly landmark decision–at the zenith of Cold War tension? And, most broadly, if American pressure and high superpower tension levels have caused Soviet-Japanese relations to be limited, then why has the reduced U.S. pressure and détente of recent years not brought the two countries closer?

The point is simply that Vishwanathan's rather deterministic approach neglects the central element of choice. No doubt superpower rivalry has broadly affected Japanese policy choices, but it has not made any decision inevitable. Japan has not been an American satellite–it has made its own decisions according to calculations of what is best for Japan. In these calculations, moreover, the U.S. and the USSR have not had an equal impact–the coalition of business and conservative political leaders responsible for Japan's postwar foreign policies and the vast majority of the Japanese people have preferred an alliance with the United States. A Soviet "alternative" has not been seriously debated. It should be remembered, too, that the Soviets have not given high priority to seeking a rapprochement with Japan. Moscow's inability to improve its image in Japan has stemmed from its stubborn unwillingness to make the compromises on trade, territory, and fishing rights that might have eased deep-seated Japanese suspicions. At bottom, therefore, neither Japan nor the Soviet Union has worked very hard at building a "special" bilateral relationship.

Vishwanathan uses Japanese and English materials on selected Japanese-Soviet negotiatory encounters on fishing, trade, territory, and the repatriation of Japanese prisoners of war. Much of this material is dated, however, and the book's focus is almost wholly (and curiously in view of the title) upon events before 1960. The book is also in need of a firmer editorial hand–translations of key Japanese groups are sometimes omitted, and there are distracting misspellings and errors in dates and names.

The major shortcoming of this book, however, is that the author allows her obvious desire for improved Japanese-Soviet relations to cloud her perspective of what a more detached observer would surely see as a relationship purposefully kept limited by both sides because of a combination of psychological, historical, and political reasons. Vishwanathan may be right in expecting that détente will usher in a "new era" in Japanese-Soviet affairs, but wishing does not necessarily make it so.

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International Economics of Pollution. By Ingo Walter. (New York, Toronto: John Wiley, 1975. Pp. 208. $17.50.)

The cover blurb on Professor Walter's book informs us that he is a consultant to OECD, which is very appropriate since the chief merits and limitations of his book are those of an OECD report. Walter is a specialist in international trade and finance with a particular interest in environmental issues. His book is competent and thorough. If you wish to compile a list of several dozen ways in which the internal environmental policies of trading nations can subtly influence the international flow of commodities and capital, Walter will provide that for you, carefully detailing each in a way that a modest background in economics will allow you to follow. If you want to bring yourself up to speed on PPP (polluter pays principle), TFP (trans-frontier pollution), or the potentially constructive role MNC's (multinational corporations) could play, then again Professor Walter is your man. In general, if you are doing political research that deals with international economic impacts of environmental policies, you will do well to have a copy of Walter's text on your shelves. It is a very useful handbook.

The book's principal intellectual commitments are to a wholesome environment and to free trade, but with rather more fervor (I sense, but perhaps Professor Walter would strongly disagree) for the latter. The goal of socially responsible governments and corporations should be to protect and enhance the environment in such a way as to minimize unwarranted effects on international trade: specifically, Professor Walter is concerned about the prospect that environmental policies would be distorted in ways which, in effect, serve to protect domestic industries–providing the functional equivalent of tariffs, export subsidies, import restrictions, and so on.

Ideally, all actors should pay the full social cost of any damage due to their activities. We may desire, in principle, that the owner of a car pay the costs of pollution emitted during its use, and that the manufacturer pay the costs of pollution due to the production of the car. But any such simple principle runs into numerous practical problems. It is almost always hard to get agreement on the social costs of pollution; ambiguities arise about who is properly responsible for the damage done by pollution; firms, work forces, and communities vary in their ability to adapt to environmental constraints, and consequently in their ability to pay; and so on. This makes it difficult to lay down clear-cut rules for who ought to pay how much for various kinds of pollution. These problems would arise even if every actor was wise and reasonable and clearly motivated by some abstractly definable sense of "social responsibility." But since real actors are not always wise or reasonable, and certainly not always motivated by a common sense of social responsibility (all the more so across international boundaries), the potential exists for a good deal of mischief, aggravated by suspicion about what is motivating others.

As an example, a controversy has lately arisen between Japan and its OECD partners (but chiefly the U.S.) about Japan's emissions requirements for automobiles. The Japanese see their regulations as the proper response to the particularly severe pollution problems of Japanese cities; others, particularly American car makers, see the regulations as a device for making it difficult to market foreign cars in Japan. Similar controversies arise in connection with strict limits on DDT contamination of beef which have had the effect of closing European markets to Uraguayan beef; or regarding the reluctance of England and Germany to require controls on sulphur emissions (or provide compensation) that takes account of the consequent damage done by acid rains over Scandinavia.

What Professor Walter does is provide us with a comprehensive survey of the kinds of difficulties we can expect to arise. He also provides a good deal of normative advice on how reasonable and socially responsible nations ought to behave (or, equivalently in most economists' usage, how rational actors alert to their long-run self-interest ought to behave).

This is much less useful, and even at times a little tedious, probably because it is much easier to lay down admirable principles than to provide operationally meaningful criteria for what is reasonable when admirable principles conflict, as they so often do. This difficulty is aggravated by an almost total absence (either due to lack of space or to a reluctance to say anything controversial) of discussion of concrete cases.

Are the problems that Walter discusses live ones or only abstract possibilities? In other words, are national environmental policies being significantly distorted by real or imagined concerns about adverse effects on a country's international economic position? I gather that Professor Walter does not think such problems are yet getting out of hand; and that (much more casually) is my own impression. But the set of issues no doubt warrants some concern; and the very limitations of a narrowly economic approach to a problem that is richly political suggests that useful contributions can be made by political scientists. A reading of Walter's book would be a good way to begin such an inquiry.

Let me add that a topic of great political interest, but which lies largely beyond the scope of Walter's treatment, is what we might call the "economics of international pollution," or better, "the political economy of. . . ." I mean here pollution which has impacts which approach being, or literally are, global in scale, such as the depletion of the earth's ozone shield, or the potentially far-reaching second order effects of Soviet proposals to reverse the flow of certain Siberian rivers. On this kind of problem anticipated costs and benefits are quite likely to be vastly different for different countries; many (or all) countries will have a stake; and the very scale of the phenomena makes even small risks of large consequence, and/or substantial probability of even very hard to observe effects sufficient grounds for real concern. We can expect such issues to play an increasingly prominent role on the agenda of international politics in the decades ahead.

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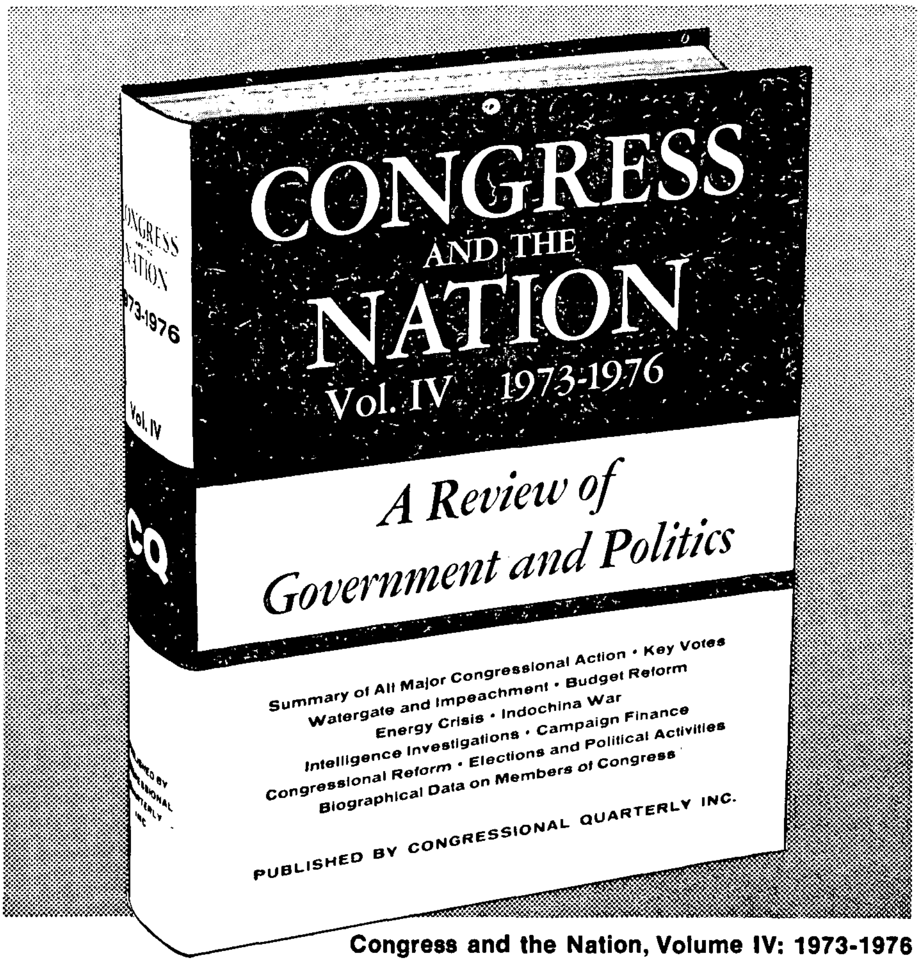
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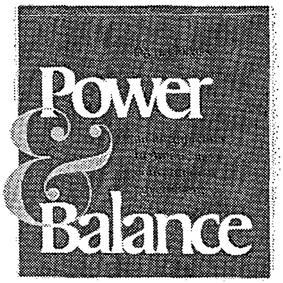

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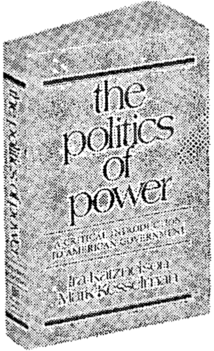

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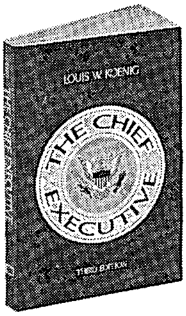

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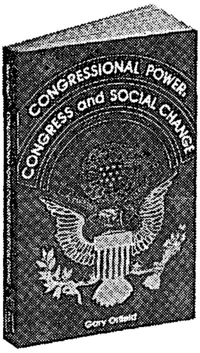

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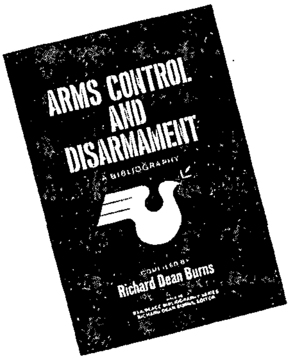

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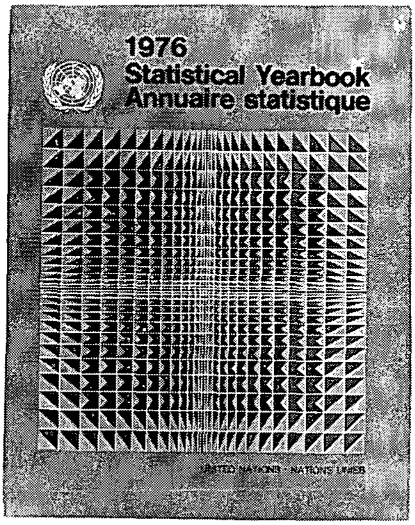
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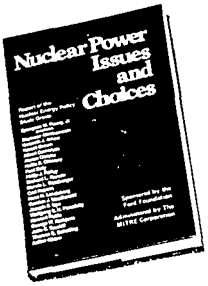

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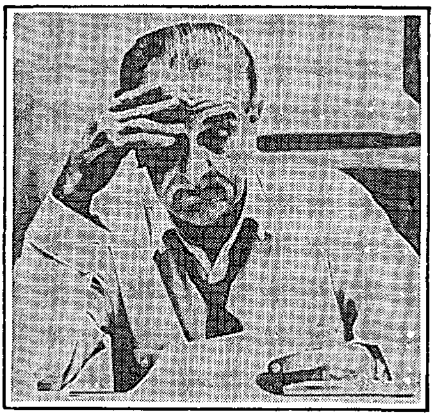

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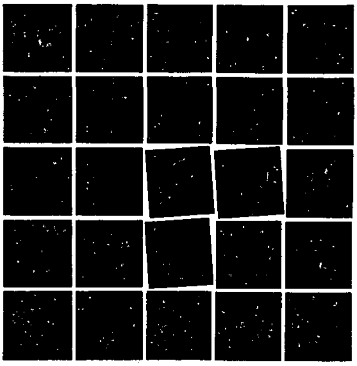

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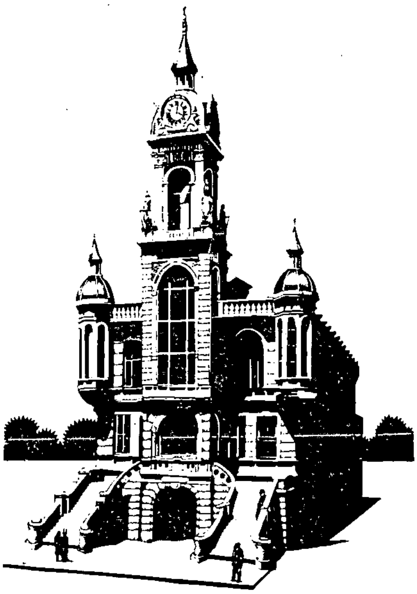

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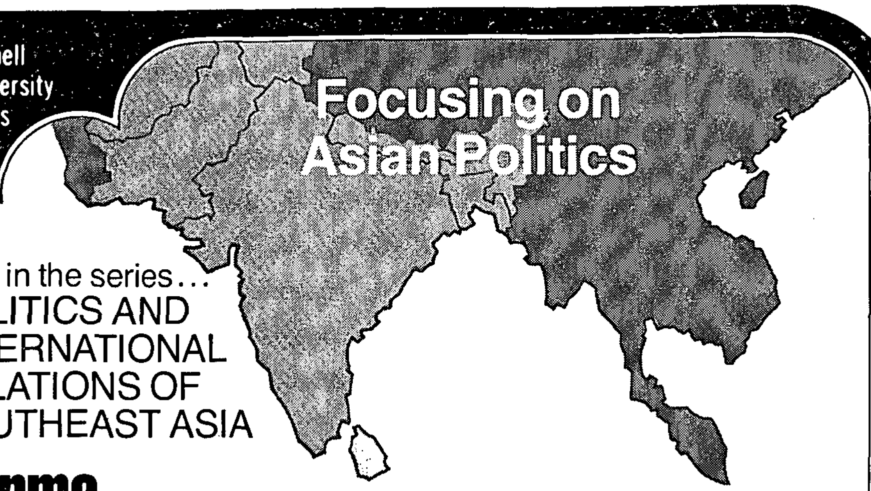

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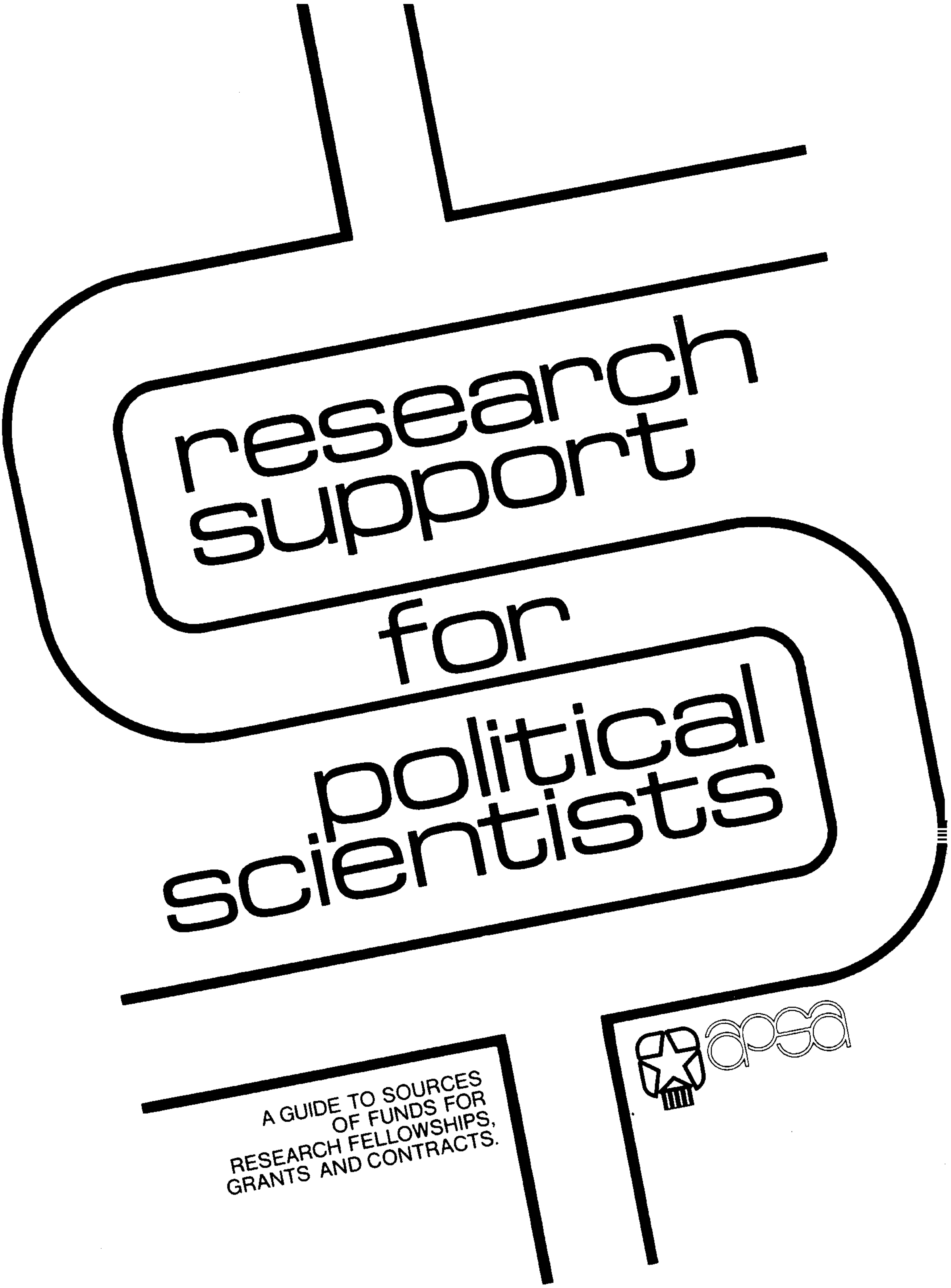

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